

Odious Comparisons: Incommensurability, the Case Study, and “Small N’s” in Sociology*

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Case studies and “small-N comparisons” have been attacked from two directions, positivist and incommensurabilist. At the same time, some authors have defended small-N comparisons as allowing qualitative researchers to attain a degree of scientificity, yet they also have rejected the case study as merely “idiographic.” Practitioners of the case study sometimes agree with these critics, disavowing all claims to scientificity. A related set of disagreements concerns the role and nature of social theory in sociology, which sometimes is described as useless and parasitic and other times as evolving in splendid isolation from empirical research. These three forms of sociological activity—comparative analysis, studies of individual cases, and social theory—are defended here from the standpoint of critical realism. In this article I first reconstruct, in very broad strokes, the dominant epistemological and ontological framework of postwar U.S. sociology. The next two sections discuss several positivist and incommensurabilist criticisms of comparison and case studies. The last two sections propose an understanding of comparison as operating along two dimensions, events and structures, and offer an illustration of the difference and relationship between the two.

All comparisons are odious.

Cervantes, *Don Quixote*¹

Comparisons are odious, because they are
impertinent . . . making one thing the standard
of another which has no relation to it.

W. Hazlitt ([1821] 1903)²

The faculty of comparison is that which
produces ideas, and is therefore the
foundation of intellect, and all the
intellectual powers of the human mind.

Lord Monboddo (1774)³

The lasting effects of the post-1945 methodological *rifondazione* in American sociology, especially the closing off of the explicit epistemological discussions that had

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¹Cervantes (1950 [1604–1616]:619). Cervantes’s phrase “comparisons are odious” often is attributed to Sir John Fortescue (ca. 1385–1479).

²Hazlitt (1903 [1821]:141), essay XI (“On Thought and Action”).

³Monboddo (1774–1779:68), vol. I, part I, book I, ch. 6.

characterized sociology during its early decades, have left the discipline poorly prepared to confront the seismic shifts in understandings of science and knowledge during recent decades.⁴ Of special concern in this article is the dilemma facing those sociologists who work on single cases or who conduct comparative research on a small number of cases (conducting what often are called small-N comparisons). These sociologists, found primarily in the subfields of ethnography, cultural and historical sociology, and area studies, long have faced criticism from methodological positivists. But the same researchers, especially those working on non-Western societies or subaltern cultures, or on seemingly incomparable events like the Shoah, increasingly have confronted criticisms from nonpositivist directions as well. Theorists of “incommensurability” have questioned the basic ontological and epistemological legitimacy of comparative research. Qualitative social researchers thus are caught between the opposing pressures of positivism and incommensurabilism.

This article attempts to make analytical sense of the arguments for and against small-N comparisons and case studies and to offer a defense of both forms of social analysis. Indeed, my conclusion is that case studies and small-N comparisons should be seen as privileged forms of sociological analysis, due to the ontological peculiarities of the social. In defense of comparative analysis, I argue that comparisons operate along two dimensions, events and structures, corresponding to one of the main lines of ontological stratification of the social-real. Critics of comparison often conflate these two dimensions. Thus, the thesis of the incomparability of the Shoah (Lyotard 1988) is focused on the level of events, while arguments for geographically specific analytical categories (e.g., Marriott 1990; see Chakrabarty 2000) typically are located on the level of causal mechanisms.

In this article, first I review the practical/philosophical standpoint from which the case study has usually been rejected as nonexplanatory and hence as unscientific. This position, which I characterize as methodological positivism, has dominated the sociological field in the United States for almost a half century, largely defining what counts as “scientific capital” (although its preeminence has started to wane in recent years).⁵ In criticizing sociology’s methodological positivism, this section also adumbrates an alternative, based on the critical realist philosophy of science.⁶

Next, I examine positivist views of comparison and the case study. Although some methodological positivists reject small-N comparisons as intrinsically inferior and unscientific (Liebersohn 1991, 1994), contemporary qualitative sociology has its own internal version of positivism, which understands small-N comparison as allowing qualitative research to attain a degree of (what it construes as) scientificity (King, Keohane, and Verba 1994; Mahoney and Rueschemeyer 2002). Critical realism reveals the shortcomings of these arguments against small-N comparisons and case studies.

The next section turns to a more difficult set of objections to comparison, organized around the concepts of incommensurability, the uniqueness of events, and the

⁴I discuss the postwar epistemological refounding of U.S. sociology in more detail in Steinmetz (forthcoming a).

⁵For the notion of scientific capital, see Bourdieu (2001:110, 122 and 1975); on the domination of the postwar American sociological field by positivism, see Smelser (1986); Turner and Turner (1990). The same, arguably, is true of American political science (see Hauptmann 2005; Mihic, Engelmann, and Wingrove 2005).

⁶Within the space limits of this article I am only able to present those aspects of critical realism that are relevant directly to the topics of comparison, case studies, and incommensurability. Readers are referred to Bhaskar (1979, 1986, 1997) and Collier (1994, 2005) for the foundations of critical realism and to Archer et al. (1998) for an overview of current work in this area; Steinmetz (1998) discusses the possible relevance of critical realism to historical sociology. The only sustained critical realist treatment of comparative social research I have found is by Lawson (1998, 1999), whose views are discussed below.

impossibility of translation. These objections, rooted variously in Romanticism, structuralist linguistics, poststructuralist and postcolonial theory, and historical reflection on the Holocaust, have assumed greater significance within sociology in recent years as the field increasingly exposes itself to the changing macrosocial conditions of knowledge production (Steinmetz forthcoming a). I reject some of these arguments while trying to show that others are compatible with a critical realist understanding of comparison.

Then I propose a critical realist account of comparison. Like positivism, critical realism suggests that comparison is an indispensable part of any social science. Against the positivist understanding, however, comparison cannot be understood as the only legitimate style of research; case studies and explicit theoretical work are equally essential parts of any social science. Like some of the arguments for incommensurability, critical realism acknowledges that many social events and processes cannot be subsumed under general, comparative categories; Auschwitz is perhaps the best token of this meta-type of the incomparable. But critical realism insists on the ontological difference between events and mechanisms and on the ubiquity of the contingent, nonrecurrent, conjunctural determination of events within open systems like the social. This means that even events incomparable at the phenomenal level still may be amenable to explanation in terms of a conjuncture of generative causal mechanisms. (By extension, a sequence of unique events may be explained in terms of a sequence of conjunctures of mechanisms.) Recognition of the ontological peculiarities of the social helps to clarify the invaluable role of case studies and historical narratives, along with small-N comparisons and social theory, all of them developing in partial autonomy from empirical research within the overall array of forms of sociological activity.

Critical realism makes sense of the distinction between two types of comparative activity that often are confused or conflated in sociological discussions: comparison across generative causal mechanisms and comparison across empirical-level events. The final section of this article illustrates this difference with a brief example from my own research on German precoloniality and colonial state formation.

THE UNCANNY PERSISTENCE OF POSITIVISM IN SOCIOLOGY AND THE OTHER SOCIAL SCIENCES

American sociology has long been dominated by an epistemological, ontological, and methodological position that can be summarized as methodological positivism. The predominance of this formation in sociology does not imply hegemony in a strong Gramscian sense; I am referring instead to the way in which actors within the sociological field have tended to recognize scientific capital as accruing especially to methodologically positivist positions.⁷ The domination of the American sociological field by methodological positivism was the result of an overdetermining conjuncture of forces internal and external to the discipline during the postwar period. The most important external conditions were related to the emerging Fordist mode of

⁷See Bourdieu (1977) for a discussion of doxa, orthodoxy, and heterodoxy; and Bourdieu (2001) for a discussion of the concepts of symbolic capital, habitus, field, and the specific characteristics of the discipline of sociology as a field (in France). Bourdieu's emphasis of the dialectics of mutual *recognition* of (cultural) symbolic capital is one of the many features of his theory that distinguish it from utilitarian models of capital. Bourdieu's emphasis on the ways in which the dominated positions within a field may be substantively unlike the dominant ones while nonetheless contributing to the reproduction of their own domination breaks with the (neo)Gramscian focus on the *contents* of ideology.

regulation in the United States. The political, cultural, social, and economic regularities of postwar Fordism resonated powerfully with positivist social epistemology, strengthening the hand of the more explicit positivists by orienting other sociologists' spontaneous epistemologies in their direction (Steinmetz forthcoming a, forthcoming b; Steinmetz and Chae 2002). The same positions were promoted within the discipline by the articulation of Parsons' "grand" theory (which was in many ways anti- or nonpositivist) with the exigencies of positivist epistemology and a vision of methodology framed as scientific naturalism.⁸ And as Turner and Turner (1990) have shown, the circulation of an array of actors in and out of the field and among sociology departments, free-standing research centers, and specific branches of the American state played a central role in this methodological consolidation. Funding priorities obviously played a key role as well (Kleinman 1995; Ross 1991).

Methodological positivism's prevalence was especially pronounced between the 1950s and the mid-1970s. Since then it has lost some of its compelling power, for macro-sociological and intradisciplinary reasons that cannot be discussed here in any detail. Many of the societal-level patterns that reinforced positivist social epistemologies disappeared with the end of Fordism. But forces internal to the discipline and the habitus of sociologists have prolonged positivism's life "unnaturally." If positivism is no longer "doxic," it is still at least "orthodox" in many influential corners of the discipline.⁹

Defining the scientific capital of the American sociological field in philosophical terms has several advantages for an historical analysis. First of all, *positivism* is one of the most multivalent of signifiers (see Despy-Meyer and Devriese 1999:95–143). This is especially true in sociology, where discussions of positivism have often focused on the views of August Comte rather than more influential philosophical influences (Comte 1975; Halfpenny 1982; Scharff 1995). More important than Comte in 20th-century U.S. sociology have been the epistemological positions associated with names like Ernst Mach, Karl Pearson, Rudolf Carnap, Karl Popper, and Ernest Nagel, and, during the key period of postwar epistemological reconsolidation, sociological advocates of positivism such as Lundberg (1939) and Lazarsfeld (see Mills 1959:59–66).¹⁰ Defining methodological positivism analytically also allows us to distinguish between social scientists' explicit descriptions of their own position—their manifest projects—and the latent assumptions expressed in their texts and other kinds of activities. As Kuhn ([1962]

⁸A key role in this translation of antipositivist theory into a form compatible with a positivist, naturalist version of sociology was played by the *triade capitoline* (Bourdieu 2001:198) of Parsons, Merton, and Lazarsfeld. Gouldner occupied a complex and evolving position vis-à-vis the methodologically positivist formation; see Steinmetz and Chae (2002). On Parsons, see especially the contributions by Camic (e.g., 1987, 1989) and Alexander 1982–1983, vol. 4).

⁹One anonymous reviewer of this article felt "that the characterization of the dominant methodological perspective in the discipline" was a "straw man." My characterization of the rise to dominance of this epistemological position in the 1950s and the 1960s and of the challenges to methodological positivism since the late 1960s is based on historical research as well as two decades of participation in several U.S. sociology departments. Methodological positivism may seem chimerical for the same reasons that other dominant ideological positions are invisible to those who profit from them (or to those who adjust to their own domination, developing a "taste for necessity"). It perhaps is worth pointing out that perceptions of the current status of positivism vary enormously from one sociologist to the next, even among the critics, underscoring the unsettled character of the present period. Bourdieu (2001) and Freitag (2001), for example, claimed to detect a continuing hegemony of positivism within sociology, while Gouldner felt that the position he called "positivism" was waning already at the end of the 1960s (Gouldner 1970), and Flacks (1989) believed that it had disappeared by the late 1980s.

¹⁰Abbott (2005) argues that Lazarsfeld's understanding of temporality broke in important ways with positivist understandings of "if A then B" conjunctions. I am suggesting that each of these theorists contributed specific elements to the overall epistemological reconfiguration in American sociology, however, and not that each one of them accepted the entire package. It also is worth noting that Mills (1959) also paints a very different picture of Lazarsfeld's epistemological impact on American sociology. Perhaps this taps into a difference between Lazarsfeld's manifest and latent projects and into heterogeneities in the overall texture of his work.

1970) pointed out, the socialization of scientists into their deepest epistemological and ontological commitments is mainly an implicit process, embedded in practical training. This means that scientists are usually not fully aware of their own epistemological positions. The main site for the communication of positivist orthodoxy to sociology students has been introductory statistics and methods training (e.g., Blalock 1964; Hanushek and Jackson 1977).¹¹

Defining positivism philosophically also allows us to avoid using the term as an epithet and to acknowledge positivism as a well-established tradition (see Giere and Richardson 1996; Stinchcombe 1996; Turner 1993). Explicit endorsements of positivism are in any case quite rare among contemporary sociologists, suggesting that even if it still is powerful, it now appears under novel guises. A sharp definition of methodological positivism allows one, finally, to make sense of some of the arguments for and against small-N comparison and single-case studies, which is my main goal here.

Methodological positivism is an amalgamation of three main components: empiricist ontology, positivist epistemology, and scientific naturalism.¹² The first of these comprises an array of *ontological* assumptions about the nature of social reality, objects, and causality. Closely articulated with these ontological foundations are a set of *epistemological* precepts concerning the way in which social facts can be known. The third component of methodological positivism is a *scientific-naturalist* understanding of the unity of the social sciences and the natural sciences, with important implications for ontology, epistemology, and methods. Research methods in the narrower sense are limited and selected by these three basic sets of assumptions. Linked more loosely to these four components, but still important, are tastes and guidelines concerning the presentation of sociological research.

Empiricism is an *ontological* position, one that posits that “there is no real difference between ‘essence’ and ‘phenomenon’” (Kolakowski [1966]1968:3; see also French 1989). It rejects the invocation of theoretical, abstract, and unobservable entities. Bhaskar (1997:ch. 2) calls this proscription on ontological depth “actualism”; Kolakowski ([1966] 1968) called it “phenomenalism.” Empiricism suggests that causal mechanisms—explanans—are located at the same phenomenal level of reality as their explanandum (Hempel and Oppenheimer 1948) and that reality, or at least any reality that can be talked about usefully, is observable under current observation technologies (Miller 1987:359–63). As Collier (1994:7) points out, empiricism in the social sciences is expressed more often as a vague “actualism,” that is, a stance denying the existence, plausibility, or usefulness of conceiving of “underlying structures which determine . . . events, and instead locates the succession of cause and effect at the level of events.” It is important not to conflate empirical research with empiricism; critical realism strongly favors empirical research but without reducing (social) science to the empirical, strictly observational level (see Pawson 1989:ch. 5).

Founders of American sociology such as Franklin Giddings drew on Mach’s argument that science should be based on sense impressions (Bannister 1987:72–73; Ross 1991:227; Toulmin 1969:33–35). Pearson’s *The Grammar of Science* (1892), which was adopted by many of the first generation of American sociologists in their self-transformation from reformers and social evolutionists into social technicians (Bannister 1987:151; Levine 1996; Pearson 1892), reproduced Mach’s sketch of the

¹¹Other key texts from the 1960s and early 1970s are cited in Gartrell and Gartrell (1996:144); for a critical discussion of Blalock, see Miller (1987:240–41, note 11).

¹²This framework has also been typically linked to specific research techniques, especially quantitative ones, but it is by no means limited to those. See Layder (1988) for an interesting discussion of the “interdependencies” among specific methods, epistemologies, and theoretical discourses.

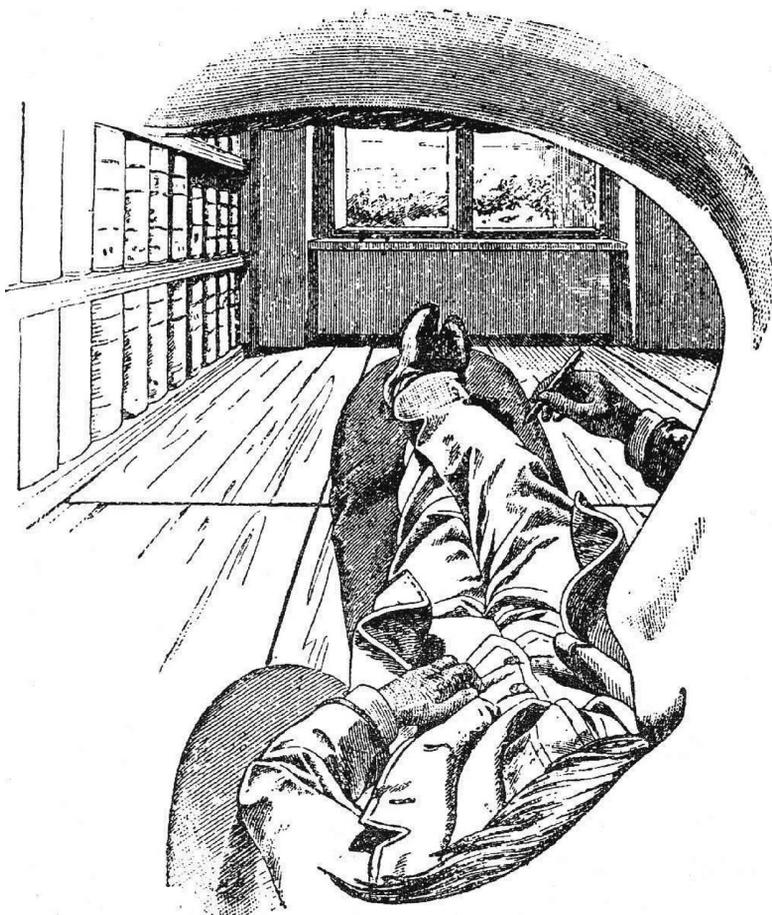


Figure 1. The empiricist moustache (Pearson 1892)

view from inside the scientist's head, looking out at the world (Figure 1). This provided a direct icon for the classic empiricist argument (e.g., Locke [1689]1997:109) that all knowledge is built directly on sensory experience.

Empiricism does not necessarily encompass specific concepts of causality, however. This is why we have to distinguish between empiricism as a position within *ontology* and positivism as a position within *epistemology*. Indeed, Foucault (1972:162) and other poststructuralists sometimes converge with older versions of empiricism in their proscriptions on "depth hermeneutics" (see also De Certeau 1984:59; Nancy 2000), yet they do not embrace a positivist epistemological insistence on general laws.

Actualism can be contrasted with *depth realism* (Peirce 1931–1932; Sokolowski 1997; Archer et al. 1998), which begins from a vertically stratified picture of reality. Realism is simply the position that reality exists independently of our senses or our descriptions of reality. This means that many familiar forms of realism either are empiricist versions of it or are indifferent to the question of ontological layering.¹³

¹³When Rorty (1979) and Putnam (1975) discuss realism, for instance, they seem to be referring either to what I am calling an empiricist realism or most generally to the belief in the intransitive existence of objects of knowledge; Quine's (1960:233–38) definition of realism as the belief in "abstract universals" comes closer to critical realism.

Ontological stratification between the levels of the empirical and the real allows for disjuncture between underlying causal mechanisms and observable phenomena. Depth realism also allows for a horizontal stratification, a diversification of causal structures, suggesting that a multiplicity of mechanisms typically will combine in conjunctural ways to produce any given event (Bhaskar 1986:110). Critical realist ontology also encompasses a theory of the *emergence* of ontological levels, and it sketches out the basic lineaments of a specifically *social* ontology, organized around the difference between human agents and social structures and the differences between social and natural mechanisms—specifically, the time, space, concept, and practice dependency of the former (see Bhaskar 1979, and discussion following).

Positivism is distinct from empiricism, even if the two positions have some common origins and have frequently appeared together. As Harding (1999:132) points out, epistemologies and ontologies do not inexorably one another entail, even if specific pairs are “intimately entwined.” Positivism is best construed as a position within epistemology, rather than ontology, one that insists that scientific explanations take the general form of “if A then B” statements (or more elaborate versions of these Humean “Constant Conjunctions of Events”); see Hume [1748] 1975:76). The causal invariance such statements presuppose can be combined with an actualist ontology. But it is also possible to break with empiricism and to retain the rule of invariance, as long as the relevant underlying causal mechanisms, or explanans, are uniform across all instances of a given explanandum. This sort of realist positivism (or positivist realism) can be found, for example, in versions of orthodox Marxism that posit a unitary underlying cause of all cases of a given empirical event. And as Somers (1998) has convincingly argued, this approach is also characteristic of those rational choice approaches that are realist about ontology but positivist about epistemology. This specifically concerns those versions of rational choice theory that posit a subject operating within the “pure universal subject-medium of theoretical reason” (Freitag 2001:10) and that deduce (or predict) specific decisions from interactions between rational subjects and the constraining structures of choice options or games.¹⁴

The third component of methodological positivism is a strong version of scientific *naturalism*. Naturalism in this context refers to the philosophical assumption that the social world can be studied in the same manner as the natural one. *Scientism* is a more stringent variant of naturalism that claims a complete unity between the natural and social sciences (Bhaskar 1994:89). Scientism therefore is linked closely to assumptions about ontology and epistemology, but it has additional implications for sociological method and research design. Specifically, scientism in postwar American sociology implied that the human sciences should not differ in meaningful ways from the natural sciences. Due to the central place of quantification, large-scale data sets, experiment, and prediction in the natural sciences, it was assumed that these were plausible and appropriate goals for sociology. Qualitative data and methods are of course also quite compatible with positivism and empiricism. But in the actually existing assemblage of rules, the “disciplinary matrix” (Kuhn 1977) that made up sociology’s version of methodological positivism, quantitative research on large data sets was preferred even to experimentation.¹⁵ The scientific convergence in sociology stemmed from the prestige of physics and the other mathematized sciences (including economics) both at the time of the discipline’s founding and especially in the conjuncture of the

¹⁴Of course many rational choice theorists are not realist about the concept of human rationality but describe it as a heuristic fiction; see Friedmann (1953).

¹⁵See Breslau (1998) for a revealing case study of the competition between econometricians and experimentalists for control of one specific arena of social research.

refounding of sociology after World War II (see, for instance, Hauser 1946).¹⁶ The postwar shift in emphasis toward more technical statistical training meant that when sociologists did engage in epistemological debate, they often derived their philosophical positions from statistical methodology (e.g., Lieberman 1994:1227). (In the first half of the 20th century, by contrast, technical procedures were often derived from, or at least were defended in terms of, explicit philosophical programs (e.g., Lundberg 1939).) The privileging of the statistical analysis of large data sets has been another source of criticism of case studies. As we will see in this article, some qualitative small-N social researchers have responded with simulacra of statistical methods that are still scientific and positivist (e.g., King, Keohane, and Verba 1994; Skocpol 1984).

Scientism had three crucial implications for sociologists' perceptions of social ontology. Scientism militates against the recognition of the time, space, and concept dependence of social structures and practices. First, geospatial variation and historical transformations in social structures tend to be ignored or underestimated. For example, Skocpol (1984:378) wrote that the goal of comparative historical research was "to identify *invariant* causal configurations" (emphasis mine). This orientation toward invariant or universal laws partly reflected sociology's mimicking of the natural sciences. After all, causal mechanisms in nature change much more slowly than social ones, if at all, and do not vary from one location to the next.¹⁷ The scientific extension of categories across time and space, from the contemporary West to the non-West and the historical past, also was rooted in the asymmetrical structure of the capitalist world system, the dynamics of imperialism and colonialism, and assumptions of universalism.¹⁸

Concept dependency refers to the claim that human practices and social structures "do not exist independently of the agents' conceptions of what they are doing in their activity" (Bhaskar 1979:48). Concept dependence therefore can be summarized as the dependence of human practice on systems of "meaning" or "signification." Hacking (1999) speaks similarly of the looping effects of theoretical classifications on social realities. Social practices, in other words, are not "brute facts" (Taylor 1975:ch. 3, 1979). All forms of social analysis, including small-N comparisons, are much simpler if we can dispense with the step of making sense of how the people we study define the social world. It is worth noting here that critical realism is willing to deploy theoretical concepts that do not map onto anything in the *conscious* worldviews of the subjects studied and thus is distinct from some versions of interpretive sociology. But even if social analysis goes beyond conscious or expressed meanings, the latter are necessary points of entry, as illustrated by the psychoanalysis of manifest symptoms or dream contents in order to discover latent sense. Interpretation is an essential step in order to understand whether the empirical practices we are trying to account for are

¹⁶The preference for statistical methods over experimentation also had to do with the fact that the social is an open rather than a closed system. This openness is responsible for the implausibility of experimental closure in the social sciences, that is, the impossibility of blocking the effectivity of all causal mechanisms other than those of theoretical interest. This renders experimentation *Stricto sensu* impossible in the social sciences, a fact that seems to have been recognized in the social sciences, as indexed by the lower prestige of experiments.

¹⁷To the extent that sociologists have discussed so-called "scope conditions," they have acknowledged time dependence. Unlike the notion of "path dependency," which only makes sense if certain social processes are not overdetermined conjuncturally, the concept of scope conditions does not imply that social processes typically have standard causal configurations from which deviations can be gauged. Some writers also combine the idea of scope conditions with a neopositivist "falsificationism" (see Walker and Cohen 1985). Scope conditions properly should refer to underlying causal mechanisms. But the expression of these mechanisms can be suppressed, making falsification a misleading test.

¹⁸See Gulbenkian Commission (1996) and Chakrabarty (2000); and for a conservative critique of universalism, Huntington (1996).

winks, twitches, fake twitches, or fake winks that signal something else again (Geertz 1973:6–7). Without taking the signifying dimensions of social practices into account, we literally will not be able to know what sort of practices we are comparing (i.e., whether they are like or unlike).

But as writers like Blumer (1956:686), Mills (1959), and Shils (1948) already were pointing out in the 1940s and 1950s, mainstream sociology embraced the opposite assumption: *concept independence*. Social processes were construed as connections between thing-like “variables.” Sociologists spoke approvingly about the ways in which social research could inform policy (Freitag 2001:19–20), and this typically relied on a view of policies as dependent on “intransitively” existing, invariant social laws.¹⁹ Awareness of “looping” effects crept into social psychology and survey research in the guise of technical questions such as the discussion of “interviewer effects.” Methodological positivism has not acknowledged, however, that *all* of the supposedly intransitive social realities we study are potentially co-determined by the social sciences.

There is nothing intrinsically positivist about the postulate of concept independence. Rather, the insistence on treating social facts as thing-like flowed from the *scientific* assumption that because natural objects can be studied as cultureless material things, the same must be true of human practices. Sociology’s ability to ignore problems of concept dependency was abetted by the fact that most natural phenomena in fact are not concept dependent in the same way as human phenomena. As Hacking (1999:32) remarks, quarks “do not form an interactive kind; the idea of the quark does not interact with quarks”; they “are not aware that they are quarks and are not altered simply by being classified as quarks.” The postulate of concept independence was also abetted by the empiricist suspicion of theoretical concepts and unobservables like meaning, thought, signification, or habitus. Positions around naturalism/scientism are relatively autonomous from specific ontological and epistemological postulates, just as the latter are analytically distinct from one another.

A further dimension of sociological scientism was its attempt to banish “values” from sociology. The fact-value distinction was enshrined in some of the earliest statements of sociology’s mission (Weber 1949; see also Poincaré 1913:347–52) and then was reasserted in post-World War II American sociology and during later periods of epistemological-political unrest.²⁰ As Habermas (1971:73) noted, early positivism placed “scientific-technical progress” itself “in the place of the epistemological subject.” A meta-value was thus enshrined: the injunction that science be technically utilizable and predictive, with normative values entering only into the choice of topics but not shaping the way social objects were to be seen and analyzed. Against this dominant “aperspectival” objectivism (Daston 1992), sociologists such as Bourdieu (1981:258) have insisted that the “scientific field itself produces and presupposes a specific form of interest”—an interest that actively prevents science from being “disinterested.” Freitag (2001:24) characterizes modern social science itself (along with myth, religion, the doctrine of Universal Reason, and contemporary postmodernism) as an ideological effort to disavow the inherent normativity of social

¹⁹ Bhaskar’s use of the term *intransitive* in this context is patterned on the idea of the intransitive verb, which does not need an object; similarly, the existence of an “intransitive” object does not depend on a human subject.

²⁰ A recent example of this reassertion of value freedom involved criticisms of ties between social movements and academic research by the National Association of Scholars; see Coleman (1992). On the political conditions in Wilhelmine Germany which led to German sociologists’ rapid convergence around the doctrine of value freedom (*Wertfreiheit*), see Rammstedt (1988); for critical discussions of Weber’s “positivism” see Hekman (1994) and Freitag (2001).

life and argues that the social sciences should instead engage in a “critical reflexive reconstitution of positive normativity, understood as a fundamental ontological dimension of all human existence” (see also Nancy 2000). Alternatives to the assumption of value freedom include the acknowledgment that values “pervade and must pervade the practices of scientific (systematic empirical) inquiry” (Lacey 1999:259); the systematic analysis of the knowledge-constitutive interests of social scientists and of the ways in which descriptions of social states of affairs may legitimate or even may help to constitute those affairs (Harding 1991); and exploration of the possibility of nonarbitrary ways of deriving “value and practical judgments from deep explanatory social theories” (Bhaskar 1991:145).

I use the adjective “methodological” to modify positivism in order to signal that American sociological scientism also had practical and methodological implications (as in classical Greek meaning of *méthodos*). Positivism was not just a wishful fantasy of philosophers or armchair methodologists. Instead, it was embedded within an entire matrix of scientific practices that made it part of the sociologist’s habitus and a condition of entry into the field. One example of what I mean by the specifically methodological side of the positivist formation involves the mode of *presentation* of sociological knowledge. Sociology was not supposed to be presented in narrative form. The only nonnumerical and nontextual elements were tables, graphs, and the like; photos, drawings, and other forms of imagery were rare. Authorial voice tended to be subordinated to technicist language or to be submerged in long lists of co-authors. Another example of sociological scientism has been the often-expressed idea that science involves a movement toward final convergence with its object, a lasting resolution of “underlying issues” (i.e., causal mechanisms), and a correspondence theory of truth (e.g., Mahoney and Rueschemeyer 2002:18). It is not that natural science actually does reach such ultimate conclusions and correspondences: as often noted, the sheer fact of massive theoretical change over the past centuries immediately cautions us against any finalistic fantasies (see Bhaskar [1975] 1997:ch. 4; Kuhn [1962] 1970; Rorty 1979). But sociologists often return to this caricature of science.

Methodological positivism as defined here emerged as the leading position within U.S. sociology between the 1940s and the mid-1960s, particularly in those departments regarded as leaders in the discipline (Smelser 1986; Steinmetz forthcoming a, forthcoming b). This settlement involved a clustering of several components:

- (1) *Empiricist ontology*, the belief that sociology should concern itself only with observables;
- (2) *Positivist epistemology*, according to which sociology should seek invariant laws or “constant conjunctions of events” that link two or more phenomena (or, in a widespread positivist-realist variant, linking a conceptual/theoretical mechanism to phenomenal events in a universal conjunction of the “if A, then B” sort); and
- (3) *Scientistic naturalism*, which holds that sociology should model itself on the natural sciences. Scientism has several specific implications:
 - (3a) It assumes the *concept independence* of social mechanisms, that is, the idea that social structures exist “independently of the agents’ conceptions of what they are doing in their activity” (Bhaskar 1979:48). The methodological implication is that hermeneutic or interpretive activity is *not* intrinsic to all social research;
 - (3b) It takes for granted the *space and time independence* of social mechanisms; that is, it assumes that causal mechanisms in the social sciences are like

those in the natural sciences insofar as they are (pretty much) invariant across time and space;

- (3c) Another aspect of sociology's scientism was its self-image as a sort of *Hilfswissenschaft* for industry and the state and one whose explanations are value free, even as its problem selection is determined arbitrarily from the outside (Caillé 1986:55–56);
- (3d) A preference for large quantitative data sets and statistical methods, or qualitative simulacra of the latter; and
- (3e) A preference for “scientific” modes of presentation (nonnarrative, nonvisual, effacement of authorial voice, and so on).

This is not an ideal-typical definition. The combination of positivism, empiricism, and scientism is better described as methodological positivism's center of gravity rather than its common denominator. Some of the individual elements can be removed without fundamentally changing the logic of the whole. Different elements have been emphasized in different periods, subfields, departments, and texts. The core of the formation, however—that which cannot be subtracted without effecting a fundamental change in it—is positivist epistemology itself, hence my inclusion of the word positivism in the name of the composite formation, and my shorthand reference to this as an *epistemological* position. Only the assumptions of the time and space independence of mechanisms seem to be as central as the positivist notion of constant conjunctions. By contrast, sociologists can break with empiricism and quantitative methods, and even with the assumption of concept independence, and can still remain positivist, as we have seen with the “domestication” of historical sociology (Calhoun 1996) and the partial containment of other critical turns in sociology since the 1960s, including Marxism, feminism, and cultural sociology (Steinmetz forthcoming a, forthcoming b). Despite these differing accents, however, all of the dominant positions in American sociology during the long postwar period had strong family resemblances with one another at the level of epistemology.

POSITIVIST CRITIQUES OF SMALL-N COMPARISON AND CASE STUDIES

Positivist conceptions of comparison have been influenced by the scientific assumption that causal generative mechanisms are invariant across time and space. Under this assumption, historical research can be construed as just another form of cross-sectional comparative research.²¹ It is always legitimate, then, to apply concepts universally across spatiocultural divides. As Connell (1997) and Stoler (2001) point out, comparative social science research in the 19th century often reflected a context of colonialism and slavery. Classical social comparison was rooted in the allochronic denial of coevalness (Fabian 1983)—that is, of the contemporaneity of colonized, subaltern, and peripheral peoples. The same theoretical concepts were used to explain social processes in peripheries and metropolises (see, for example, Durkheim 1915; Forster [1778] 1996), and the non-Western colonized were considered simply to have “less” of those concepts—less civilization, less modernity, less development, and so forth. If generative mechanisms cannot vary across space, comparative research can look for differences and similarities only at the level of “outcomes” or “events.” As a

²¹ Indeed, some recent examples of “historical sociology” have treated history as a source of additional cases rather than as an opportunity to study the transformation of social structures or the effects of causal mechanisms different from those operative in the present (see Calhoun 1996; Sewell 1996).

result, there is little interest in asking whether it is ontologically legitimate to apply categories like democracy, civil society, gender, race, or class in non-Western settings (I will return to this problem in the next section). Critical realism, by contrast, insists on the variability of social-causal structures across time and space and thus rejects the assumption that social patterns are universal.²²

The dominance of methodological positivism in sociology and political science has meant that “scientific” (or “analytic”) *qualitative* research as such has come to be identified with comparison. Mahoney and Rueschemeyer (2002), for example, define a research area they call *comparative-historical analysis*, marking a distance from merely historical sociology. The authors endorse *macro-causal analysis*, as defined by Skocpol and Somers (1980), a position that defines explanation as involving constant relations of dependence between (clusters of) determining causal factors and specific outcomes (see also King, Keohane, and Verba 1994; Orloff and Skocpol 1984). Mahoney and Rueschemeyer (2002:19, 17) are especially interested in distancing comparative-historical analysis from mere case studies or “*idiographic*” (sic) research studies. For mainstream positivist sociology, the only sort of qualitative research that deserves to be called scientific is a very particular sort of comparative analysis. Since the case study is focused almost inevitably on a complex and overdetermined conjuncture of causal processes as well as events or processes to be explained, it is rejected by positivist sociologists.

It may come as a shock, then, that not even positivist philosophers equate explanation with comparison. For Popper ([1934] 1992:59), to “give a *causal explanation* of an event means to deduce a statement which describes it, using as premises of the deduction one or more universal laws, held together with certain singular statements. A single event, in other words, can be explained causally, for Popper. Similarly, Nagel ([1961] 1979:15) writes that “explanations may be offered for individual occurrences” and offers an example involving a particular event (552 *passim*). Explanation, in other words, needs to be distinguished from the *logic of justification*. Nagel and Popper are echoed by a large group of philosophers who attempted during the 1960s to translate historical narratives into chains of events that were explained by chains of covering laws (Hempel 1966; Mandelbaum 1961). By extension, the same logic would hold for analyses of individual lives (“biographies”) or events, which could be disaggregated into a series of discrete causal statements of the “if A, then B” variety. The sociological critics of the case study thus are more “positivist” than even the leading figures in the positivist philosophy of science. Their adoption of this stance, which is only apparently positivist, is a function of scienticism.

Of course, the genuine positivist understanding of “explanation” is not the version I am defending in this article. Critical realism provides a nonpositivist refutation of the Millian “Method of Difference and Agreement,” demonstrating that it rests on a flawed social epistemology and ontology.²³ The Millian method can be faulted first of all for its empiricist “variablism”: it is unconcerned with the powers or tendencies of underlying causal mechanisms and is interested only in correlations of empirical indicators. Even if the Millian method is supplemented with a realist ontology, however, it can still be rejected for its epistemological positivism, that is, its bias

²²This does not mean that critical realism denies the existence of any and all universal features of human nature or society; see Lawson (1999). What is rejected is the idea that any causal structures, whether ubiquitous or geohistorically variable, will be related in a universal way to empirical events.

²³My ideas about the methods of difference and agreement have been developed partly in response to an unpublished paper by Baris Büyükkutan, “Revolutions, the Millian Method, and Critical Realism,” University of Michigan, Department of Sociology, Autumn 2002.

toward universal conjunctions. Shifting constellations of causal mechanisms rather than universal conjunctures are the norm in open systems like the social. In contrast to artificially closed systems like the scientific experiment, or to naturally closed systems like astronomy, empirical events are always and necessarily overdetermined by a plurality of conjuncturally interacting mechanisms.

Because true experiments cannot be conducted in the human sciences, one is compelled to study complex, overdetermined constellations. Generative mechanisms or structures have to be studied “in the wild,” as it were, and to complicate matters even further, they often appear in “impure” forms, mixed up with other mechanisms (Bhaskar 1986:110). The elements that constitute causal constellations may vary from case to case. And even if we do find recurrent empirical patterns, these can never be assumed to be universal or to be determined by the same set of mechanisms in each case. Comparison thus can focus on the differing empirical effects of a single mechanism, or on the differing conjunctures leading to similar outcomes. The model emphasized by positivist comparativism is one in which both the array of causal mechanisms and the outcomes (whether positive or negative) are the same in all cases.²⁴

Critical realism also distinguishes among comparison, explanation, and theory and suggests that social scientific explanation need not involve comparison at all. Indeed, the ontological peculiarities of social life mean that the “case study” is a precondition for any comparative assessment of theory. It needs emphasizing that theory and explanation are all we have—the ontological middle ground of “middle-range theory” is chimerical (Steinmetz and Chae 2002; Steinmetz forthcoming b). In a field like psychoanalysis, for instance, the case study is just as important as more abstract theoretical interventions in driving theoretical development forward. Within literary criticism, the interpretation of particular texts is as central to theoretical development as comparative studies (or abstract theoretical interventions). By the same token, the case study of a specific social event, process, or community is as important a part of the overall sociological enterprise as comparison or sustained theoretical reflection. The plausibility of a given theoretical argument can be assessed only by studying complex, overdetermined, empirical objects (particular individual psychobiographies, specific practices, and so forth). Case studies are thus the indispensable building block for all sociology.

This does not mean that case studies are “idiographic” raw data waiting to be processed by “nomothetic” theory machines. In the more positivistically dominated social science fields such as sociology and political science, however, the case study has been demoted to subaltern status, with less scientific capital than comparative or large-sample quantitative studies.²⁵ A revealing sign of this is the continuing use of the term *idiographic*, a theoretically incoherent survival from the 19th-century German *Methodenstreit*, which nowadays is used exclusively to assign low scientific capital to case studies. I am not objecting to the distinction between comparative and case studies; the problem is that *idiographic* was defined in the German debates of the 19th century as meaning interpretive *and* nonexplanatory. These adjectives therefore automatically get attached to any research labeled idiographic. But a case study (like

²⁴In other words even the Millian Method of Difference focuses on a single outcome; the “difference” in question refers only to the presence or absence of a given outcome.

²⁵It is telling that some of the main examples of alternatives to methodological positivism within American sociology have been “case studies,” from the classic ethnographies of the so-called Chicago School (e.g., Shaw 1930 [1966]); to Garfinkel’s (1967) ethnomethodological study of “Agnes” and Burawoy’s (1979) Marxist analysis of shop-floor production in a Chicago-area factory. Another interesting example is Wallerstein (1974), who unapologetically insists that his work, like astronomy, concerns a single case—the capitalist world-system.

an interpretive analysis) can be fully explanatory, even if it is not decisive judgmentally.

The terms *nomothetic* and *idiographic* are embedded so thoroughly within a false distinction between generalizing explanatory theoretical science and individualizing, nonexplanatory interpretation that they probably cannot be rescued for postpositivist social science. If they must be retained, however, it is important to counter that social explanation is necessarily idiographic, insofar as our sole access to causal mechanisms is through the study of particular events or specific individuals. Excluding case studies from social science would mean excluding explanation from social science.

NONPOSITIVIST OBJECTIONS TO COMPARISON

A universal rule of judgment between heterogeneous genres is lacking in general.

Jean-François Lyotard (1988:xii)

There is a common measure, which is not some one unique standard applied to everyone and everything. It is the commensurability of incommensurable singularities.

Jean-Luc Nancy (2000:75)

Although positivism is still dominant within the discipline, sociologists also are increasingly confronted by critiques of comparison based mainly in other fields, and centered on the concepts of *incommensurability*, *translation*, and *incomparability*. These objections to comparison come from disparate sources: (1) philosophical empiricism and nominalism; (2) Saussurian and Whorfian linguistics, and semiotics more broadly; (3) Romantic and Kuhnian ideas of incommensurability; (4) critics of the extension of Western or European cultural categories to the non-West or to dominated social groups more generally; and (5) reflection on the uniqueness of the Holocaust.²⁶

Empiricist and Nominalist Critiques of Comparison

Empiricist critiques of comparison have an ontological focus. As noted already, empiricists do accept comparison across observables. But they refuse any reference to underlying causes and therefore reject the idea of commensuration across “generative mechanisms.” The idea that two empirically distinct phenomena might be compared because they have the same source thus is unacceptable. A psychoanalyst, by contrast, might suggest that dreams, tremors, slips of the tongue, and other parapraxes are caused by the same set of unobservable and unconscious structures. Like empiricists, poststructuralism objects to the theoretical deployment of generative mechanisms on what are essentially empiricist grounds, rejecting “depth metaphysics,” “hermeneutics of suspicion,” and any distinction between surface appearances

²⁶In addition to the literature cited following, see also Yengoyan (forthcoming), Jucquois and Swiggers (1991), Liu (1999), and D’Agostino (2002). Obviously I can touch here only on those aspects of the large specialized literatures on Romanticism, linguistics, hermeneutics, Kuhn’s theory of scientific incommensurability, poststructuralism, global cultural difference, and the Shoah that are directly relevant to sociological comparison.

and underlying realities (e.g., Nancy 2000:52–55) and stigmatizing such models as “ideological and metaphysical” (Jameson 1984:61). This stance precludes comparison across causal mechanisms.

A related critique of comparison takes a *nominalist* form. This approach argues that we are not justified in classifying two individuals or groups as examples of the same thing if they understand themselves under different descriptions. (By the same token, nominalism suggests we should accept actors’ self-definitions if they see themselves as categorially identical.) Sociological nominalism takes the self-definitions or “standpoints” of actors as the starting point and endpoint of analysis (although this certainly is not true of all “standpoint theory”).²⁷ A comparison of, say, Islamic and Protestant fundamentalism (or positivists and nonpositivists in social science), would be rejected from a nominalist position to the extent that the groups in question refuse this definition of themselves.

Critical realism rejects nominalism for its actualism. To recall Kant’s critique of Locke, the nominalists *sensualize* comparative concepts of understanding, interpreting them as nothing more than empirical concepts.²⁸ The nominalist critique assumes, unconvincingly, that subjects are self-transparent and self-reflexive. Even if certain social “standpoints” present *opportunities for knowledge* (New 1998), there is no guarantee that those who occupy these social locations will take advantage of their cognitive opportunities.

Critiques of Translation

A more serious set of criticisms begins with Saussure, who argued that signs always are defined relationally vis-à-vis all other signs within a linguistic system or *langue* and that words do not represent concepts “fixed in advance” but rather carve conceptual differences out of an initially amorphous, “chaotic” realm of thought (Saussure [1915] 1986:114, 110–11). Translation is then, at the very least, a problematic enterprise ([1915] 1986:115). The definition of similarities and differences by social scientists then might be seen not as apprehending some intransitively existing social facts, or “social-natural kinds,” but rather as the imposition of differences on a social world that is not always already divided up by its members.²⁹ Saussure’s suggestion that problems of comparability arise as soon as we step outside of a given linguistic community was taken up by Benjamin Whorf and was codified in the Sapir-Whorf or “linguistic relativity” hypothesis (Gipper 1972; Lucy 1992; Whorf 1956).

This basic challenge to translation is relevant for all social analysis. Once we reject the claim that “social facts” are thing-like and assume instead that they are always inextricably meaningful and practical, cultural and material, the Saussurian and Whorfian doubts about translation immediately apply. Many of the contrasts that interest social researchers involve differences that cut across linguistic systems. All cross-national research on colonialism and postcolonialism, globalization, and transculturation, for instance, is subject to these objections. But issues of translation arise even within a single linguistic field, given the multiaccentuality of language (Bakhtin 1981; Volosinov 1985).

²⁷ See Harding (1991, 1999), New (1998), and Lawson (1999) for nuanced discussions of standpoint theory and social science epistemology.

²⁸ Kant ([1787] 1965:283) (from the Appendix “Amphiboly of Concepts of Reflection” in the *Critique of Pure Reason*).

²⁹ Whether or not this world is carved up inherently into natural kinds or should instead be characterized as a chaotic flux is an ontological question as well. See White (1987) for the Nietzschean argument that narrative historiography imposes order on a more fundamental historical chaos.

Romanticism and Kuhnian Incommensurability Theory

Within the contemporary philosophy of language, *incommensurability* (an idea popularized by Kuhn [1962]1970; see following) refers to “a state in which an undistorted translation cannot be produced between two or more denotational texts” (Povinelli 2001). Stepping outside of linguistic analysis, it can be argued more generally that objects, events, or practices have to be commensurated in order to be contrasted coherently. *Commensuration* is defined by the *Oxford English Dictionary* as the measurement of things against or in comparison to one another. Because comparison necessarily involves not only differences but also similarities, it requires the “transformation of different qualities into a common metric” (Espeland and Stevens 1998:314).³⁰

A major strand of ontological critique of commensurability and comparison is rooted in the idea of the uniqueness and unrepeatability of events. The longer-term antecedents of this line of reasoning reach back to 18th- and 19th-century Romanticism, historicism, German historical economics, and ethnology. German Romanticism was the source of the upgrading of folk cultures and the emphasis on cultural uniqueness, with the Grimm brothers playing as important a role as Herder, Humboldt, and Georg Forster (Koepping 1983:43, 88). Boas’s historicist emphasis on unique geographic-cultural configurations carried this German glorification of the unique and nonrepeatable into 20th-century American anthropology. Keane (2005) argues that this neo-Romantic assumption of the irreducibility and incomparability of particular cultures has been a common feature of much 20th-century cultural anthropology. The Romantic theory of the *sublime*, discussed in the next section, excluded certain aspects of reality from conceptual ordering and representation altogether.

The most influential formulation of this thesis from an explicitly epistemological perspective is Kuhn’s ([1962] 1970) theory of the incommensurability of scientific theories.³¹ This is presented as a purely epistemological argument that does not impinge directly on the question of the referent. In Kuhn’s earliest formation, in the *Structure*, incommensurability was illustrated with the model of a Gestalt switch, which allows the observer to inhabit two different cultural worlds sequentially without ever being in both at once. Later Kuhn (1999:34) adopted a more linguistic approach to incommensurability, describing himself as “an unregenerate Whorfian.” He admitted the possibility of bilingualism but insisted in an essay written just before his death that “there are things than can be said in one language that cannot be articulated in another” and that this problem could not be solved (as per Donald Davidson’s suggestion) simply by enriching the native language “by adding the missing words to it” (Kuhn 1999:35).

Collier (1994) suggests that Kuhn and his followers are conflating two different arguments. One concerns “objectless incommensurability.” If two theories (or discourses, paradigms, or speakers) actually are talking about different underlying mechanisms (“objects”), there is no incommensurability but rather two different conversations about two different things. For realists, this is not really a form of incommensurability at all. Belief in “objectless incommensurability” thus seems to rest on acceptance of the idealist denial of the existence of objects separate from descriptions of those objects. The more difficult case is “subjectless incommensurability.” This is the hypothetical situation in which two theories actually do have a common

³⁰ Chang (2002) distinguishes between *incommensurability* and *incomparability* but acknowledges that the two terms are used interchangeably in most of the literature.

³¹ The literature on Kuhn is immense; for a recent overview of his incommensurability thesis, see Favretti, Sandri, and Scazzieri (1999).

referent and in which “understanding one of the incommensurable theories would preclude understanding the other” (Collier 1994:92). There is no single subject, in other words, who can occupy both worldviews. The realist “is not committed to denying that there could be complete incommensurability” of this sort; the realist simply doubts that anyone could ever be *subjectively* confronted with this state of complete incommensurability (Collier 1994:93). Similarly, Bhaskar (1986:74) does not preclude the possibility of two epistemic communities focusing on the same intransitive object and traveling along on “semantic world-lines which never meet and know nothing of each other.” Because the two theories actually do not clash in this case, however, they again cannot cogently be called “incommensurable.” The claim that individual subjects cannot switch back and forth between seeing the world according to two different theories is also belied by phenomena such as multilingualism, code-switching, and summarizing one’s opponent’s argument in order to criticize it—as Kuhn himself acknowledged.

Incommensurability theory of this sort is a challenge to comparative researchers, who must face the likelihood that the objects they study are embedded within differing linguistic (or cultural or paradigmatic) systems. If we accept that social objects are simultaneously material and meaningful (concept dependent), this means that they cannot be stripped of their meaning within their respective linguistic systems without distortion—without transforming them into something fundamentally new.

The Imperialism of Comparative Concepts and the Asymmetry of Power Between the Observer and the Observed

Another set of arguments against comparison is concerned with the application of parochial and geohistorically limited categories beyond their core field of genesis and circulation. For example, categories like “civil society,” “the state,” “development,” “the public sphere,” “modernity,” “civilization,” and even “capitalism” are often used in comparative analyses that ask why these conditions are absent or only partially realized in specific settings (Chakrabarty 2000). Some critics reject this form of analysis out of hand, suggesting that social mechanisms are necessarily variable across culture and space (as opposed to the critical realist argument that social structures may vary in this way). Thus, Marriott (1990:1–2) argues for Indian social science concepts derived “from the realities known to Indian people” and inveighs against the application of the “parochial” concepts of European “ethnosocial sciences” to Indian realities. The investigator who asks in rural India “about equivalents of Western ‘individuals,’ ‘social structures,’ ‘kinship,’ ‘classes,’ ‘statuses,’ ‘rules,’ ‘oppositions,’ ‘solidarities,’ ‘hierarchies,’ ‘authority,’ ‘values,’ ‘ideology,’ ‘religion,’ ‘purity,’ etc., risks imposing an alien *ontology* and alien *epistemology* on those who attempt to answer” (emphasis mine).³² A more restricted version of this line of critique acknowledges that analysis of the non-Western world cannot avoid categories whose origins lie in the core countries, due to the permeation of the globe by capitalist modernity (Chakrabarty 2000). Yet such “Western-global” concepts must be combined with local ones,³³ and their usefulness for the study of earlier precapitalist histories (both European and non-European) should be evaluated carefully rather than taken for

³²On the applicability of Western categories to Chinese history, see Huang (1993, 1998). For an argument against the applicability of “Western” concepts of gender to Africa (Yorubaland), see Oyewume (1997). On differing understandings of “democracy” in Wolof and French-speaking Senegal, see Schaffer (1998).

³³For a historical example of the melding of European and Indian theoretical categories, see Guha ([1983] 1999).

granted. Social scientists have to think harder about whether it is ontologically legitimate to discuss the development of the “state” or “civil society” in the Chinese Ming dynasty or 19th-century precolonial Namibia (as in Loth 1963), to use the category “religion” in precolonial aboriginal Australia (as in Durkheim 1915), or to apply modern American conceptions of rational decision-making to medieval Europe.

Although this line of incommensurability thinking is compatible with critical realism’s insistence on the time, space, and concept dependence of social mechanisms, it does not go to the heart of the problem of comparison. It is not always problematic to export concepts from Western “ethnosocial science” beyond their zones of emergence and effectivity. Even if an idea like “feudalism” is inappropriate for precolonial African history, for example, this does not hold for all originally “Western” concepts. Nor can we rule out human cultural universals on a priori ontological grounds. Critical realists are often willing to argue for such universals (Lawson 1999:46–47).

A related line of criticism of comparison focuses on the asymmetry of power between the observer and the observed, which shapes the ways the weaker culture is translated into the metric of the observer. This is a “sociological” extension of the more general critique of translation, one that shows how translation in social research is usually unidirectional rather than reciprocal.³⁴ Non-Western societies and subaltern groups are much more likely to be analyzed in terms of the categories of the Western and dominant cultures than vice versa. Even within core countries, outside observers of poor or subaltern cultures almost always are more privileged than their informant.

This unidirectionality of translation leads to bad social science. As Lucy (1993:25) suggests, the “analysts’ own language categories may be so strongly felt that other languages will be interpreted or described in terms of them—effectively short-circuiting the possibility of developing clearly contrasting cases” and “producing serious distortion.” Others have argued that translation in contexts of colonialism or global inequality leads the observer to ignore or even to disavow the radical difference of the non-Western social order, its unique or incommensurable aspects (Bhabha 1994; Clifford and Marcus 1986; Lambek 1991). Lyotard (1988:9) theorizes this asymmetry with the concept of the *differend* between two parties, defined as a case in which the “‘regulation’ of the conflict that opposes [two parties] is done in the idiom of one of the parties while the wrong suffered by the other is not signified in that idiom.” The distortion of translation by power differentials is also problematic politically, as the conceptual language of the dominant groups may contribute performatively to the reproduction of their dominance. Analyzing women with categories originally designed to analyze men, for instance, may bolster male domination.

This is a serious challenge to projects of social comparison. To some extent, however, this line of attack conflates or equates the very real “violence of abstraction” in social practice with a less compelling argument that scientific abstraction is necessarily violent. This is not to deny that processes of symbolic commensuration may be used to justify further acts of exploitation and domination. But symbolic commensuration need not be violent in all cases. Against a poststructuralist refusal of categories that permits only empiricist nominalism or hypocritically substitutes an alternative conceptual language for the ones it rejects, we need to insist that critical social science can contribute to the “unmasking” and “refutation” (Mannheim

³⁴ It is ironic, for instance, that Quine’s (1960:28, 76) well-known discussion of the “radical translator” involves the translation of a “jungle language” spoken by “untouched” “natives” living in the “darkest archipelago.”

1952:140) of the social conditions that produce false knowledge and thus help to reproduce inequality. More to the point, if social science is unwilling to break with the categories of common culture it will be able to think its way out of the “is” into the “ought” only in arbitrary ways. Moreover, some of the social practices criticized by critical social science are themselves practices of social commensuration. The determination of value under capitalism, for example, involves a rendering of qualitative differences into a homogeneous, quantitative metric in ways that, according to Marxist critical theory, obscure the real underlying relations of exploitation. Such processes of socioeconomic commensuration should not be equated with knowledge of those same processes, even if the latter involves a form of theoretical commensuration.

The Shoah, Trauma Theory, and Incomparability

A final set of arguments for uniqueness and incomparability arises from reflection on the Nazi extermination of the European Jews. This discussion began with Adorno’s ([1955] 1981, [1962] 1982) prohibition on poetry after Auschwitz, or more specifically on his reservations about representing or displaying images of the Shoah. Lyotard (1988), Langer (1991), Agamben (1999), and others have called attention to the problems of basing knowledge on testimony in a case where the actual witnesses to the “Gorgon’s head” were killed or were exposed to horrors that were both “unimaginably true and at the same time unimaginable” (Agamben 1999:76–77). This raises doubts about whether such a historical trauma can be represented or can be understood at all (Caruth 1996). As Julia Hell (2001:95) notes: “This understanding of historical trauma as an event that shatters our symbolic frames and therefore awaits full comprehension draws . . . on a core theme of trauma theory since Freud” (see Freud 1957). Hartman (1992:321) summarizes the most radical conclusion that can be drawn from this line of reasoning: “In every realistic depiction of the Shoah . . . we describe but cannot *explain* what happened.”

In an influential intervention in this discussion Lyotard associates the Shoah with the Kantian category of the sublime and gives a Kantian epistemological twist to the claim of unrepresentability. The sublime sentiment, as transported by Lyotard (1979:77–78) from Kant’s early-Romantic 18th century into the late 20th, is “a strong and equivocal emotion: It carries with it both pleasure and pain”; indeed, it even “derives pleasure from pain.” The sublime only takes place “when the imagination fails to present an object which might, if only in principle, come to match a concept.” We thus have ideas “of which no presentation is possible,” which “impart no knowledge about reality”; they “can be said to be unrepresentable.” This formula allows for a modernist or postmodernist *aesthetics* of the sublime, and hence an indirect representation of the Shoah, as long as modern art is understood as making “an allusion to the unrepresentable by means of visible presentations,” as presenting the fact that “the unrepresentable exists.” But modernism cannot close the gap between representation and the unrepresentable or cannot permit a *conceptual* understanding of the sublime. The association of the Shoah with the theory of the sublime underscores the Romantic underpinnings of the contemporary anthropological belief in uniqueness and the critique of commensuration.³⁵

But as Rose (1996:43) asks with respect to this debate, what exactly is it “that we do not want to understand?” The actual experience of the camps’ victims most brutalized cannot be reconstructed or empathically reexperienced. But numerous theorists and

³⁵On the sublime see, Kant ([1790] 1987), Part I, Division I, Book II; also Weiskel (1976).

historians in fact have debated the causes of the Holocaust. Reversing Lanzmann's dictum, one could even argue that explaining is a more plausible goal than describing the Holocaust, at least when description is understood as a recreation of actual subjective experience. In this respect, the incomparability of the Shoah may be located at the level of the experiential and empirical event, and not at the level of the forces that led to the extermination.

Summary

The discussion of the uniqueness of events helps to clarify the critical realist rethinking of the problem of comparison. Comparison can be organized around common causal mechanisms or common events. The anthropological literature on incommensurable cultures, like the discussion of the limits of representation of the Nazi Judeocide, suggests that one cannot assume that events fall into repeated types. Indeed, since events are the result of overdetermined and contingent conjunctures of causal mechanisms according to critical realism, they are always in a certain sense unique. The events that are the most significant historically, like the Holocaust (and one could list others, including the French and Russian Revolutions), are the least amenable to grouping into types. They seem to be comparable only in their incomparability.

The discussions of the unrepresentability of the Holocaust and of radical cultural difference underscore a difference between two sorts of events: the genuinely *sui generis* event and the kinds of events Lawson (1998:149) calls "demi-regularities"—partial event regularities that indicate the "occasional, but less than universal, actualization of a mechanism, or cluster of mechanisms, over a definite region of time-space." Critical realism suggests that both unique events and demi-regularities are determined causally and in principle can be explained—even if it is much more difficult to justify any given account of the former.³⁶

RESPONSES TO THE CRITIQUES OF COMPARISON

Strictly speaking, the incomparable is the unthinkable.

Peter Osborne (2002:15)

The critiques of commensurability, translation, and comparative analysis of what is said to be incomparable have started to activate alternative understandings of comparison. One response is hermeneutic. Gadamer (1975) contrasted "the comparative method" as codified by Dilthey with his own method of the hermeneutic circle. The "essence of comparison," writes Gadamer, "presupposes the freedom of the knowing subjectivity, which is in control of both members of the comparison . . . Hence we must doubt whether the method of comparison really satisfies the idea of historical knowledge" (1975:206). The hermeneutic circle, by contrast, involves a *Verschmelzung* (melding) of the horizons of the present and the past, rather than a subordination of present and past to the observer. Along similar lines, Lambek argues against the objectifying

³⁶Can we make a parallel distinction between unique and semi-regular causal *mechanisms*? This would seem to undermine part of the meaning of the concept *mechanism*. One of the traits of the mechanism is that it is relatively perduring, even across contexts in which it is suppressed rather than empirically expressed (this is the critical realist notion of "counterphenomenality"; Collier 1994:7).

form of comparison, “third-person comparison,” which implies “a privileged, detached observer, positioned at an Archimedean or Laplacian point.” Yet Lambek also insists that the alternative is not the “first-person rhetoric” of autobiographical social science. Instead, he argues for a *dialogic* form of comparison, which acknowledges the individuality and distance/difference of the Other: “It entails the interplay of our language with that of the Other; it is concerned with trying to find the resources in our language to understand initially alien phenomena without applying distortive prejudices. . . . It implies that we can only understand the Other if we understand ourselves—and perhaps vice versa” (1991:48). What is being “compared” here, then, is not just the Other but is also the observer/social scientist him/herself.

Attempting to merge phenomenology with structuralism, Bourdieu (1977) also argued for an articulation of the sociologist’s conceptual language with the standpoint of the people observed. The sociologist, according to Bourdieu (2001:222–23), “forces himself to contribute to the construction of a *standpoint without a standpoint* which is the standpoint of science; he is, as a social agent, taken in by the object that he takes as his object, and in this respect he has a standpoint that coincides neither with that of the Others, nor with the ‘flying over’ or ‘digging under’ standpoint of the quasi-divine spectator. . . . He knows that the peculiarity of the social sciences requires him to work . . . to construct a scientific truth that is *capable of integrating the vision of the observer and the truth of the practical vision of the agent*” (translation and emphasis mine). This “integration” of the two “visions” was discussed in an earlier work as a double break—with native experience and with the “presuppositions inherent in the position of the ‘objective’ observer who . . . tends to bring into the object the principles of his relation to this object” (Bourdieu [1980] 1990:27). Bourdieu’s interest in comparing the standpoints of scientist and other social groups is also illustrated by his parallel studies of scientists, Béarn peasants (in Bourdieu [1980] 1990:147–161), Frenchmen in general (Bourdieu 1984), and Kabylisians (Bourdieu 1977), all using the same theoretical framework (see discussion in De Certeau 1984:50–52). Bourdieu’s work in general was deeply comparative, involving comparisons among social classes, men and women, and the like. Yet he rarely discussed the topic of comparison directly. Not having been seduced by what he called the “positivist temptation” (Bourdieu, Chamboredon, and Passeron 1991:34, 69),³⁷ Bourdieu was not plagued by doubts about the value of working on single “cases” or “cultures” (France, Kabylia) and building up comparative knowledge over the course of a lifetime across different projects and texts.

The hermeneutic program is not antithetical to critical realism. By emphasizing the concept dependence of human action, critical realism acknowledges that social comparison necessarily involves interpretation and translation. Critical realists have drawn upon hermeneutic approaches in developing the argument that social actors’ own “proto-scientific” theories about society constitute a starting point for social theory and a “compensator” for the lack of experimentation (Collier 1994:165). Critical realism would reject both objectifying “third-person” comparison and solipsistic “first-person” variants and would see the latter as committing the “epistemic fallacy”—the belief that statements about being can be always transposed into statements about our knowledge of being (Bhaskar 1986, [1975] 1997).

³⁷ Jenkins (1992:60) accuses Bourdieu of a “residual positivism” for his frequent use of statistics. I hope that I have shown that qualitative research can be more positivist than any use of statistics, which taken alone has no identifiable epistemological nature.

I want to suggest two other useful axes of comparison, however, in addition to the “comparison” between social scientist and informant. After all, we do presumably want to say something about the external world and not just about our own relations with it. Critical realism is most helpful, I think, in understanding what exactly is at stake here by urging us to distinguish between comparisons among empirical events and comparisons among underlying theoretical causal mechanisms. While accepting that Arunta grammar very well may lack gender distinctions, for instance (Povinelli 2001; Strehlow 1944), critical realists would not see this as necessarily precluding comparison between Arunta and other languages or semiotic systems. After all, Saussure ([1915] 1986:94) himself, who initiated the critique of translation, spoke of “linguistic laws” or “general principles existing independently of concrete facts.” Comparison might be organized, for instance, around investigation of the effects of varying kinship systems on the incidence of gender distinctions in language. Comparison across theoretical mechanisms—or what I would call *depth-realist comparison*—differs from actualist versions of comparison in that it is happy to juxtapose events or individuals that seem to have little in common at the empirical level. What unites the cases, at least hypothetically, is the effectivity of some social structure or theoretical entity. The goal of explanation is to investigate the vicissitudes of this conceptual mechanism across differing concrete contexts, cases, and events (see also Locke and Thelen 1995; Marx [1939] 1973:100–08 [Introduction, section 3]; McAdam, Tarrow, and Tilly 2001).

Depth-realist comparison is foreign not just to methodological positivism but also to most poststructuralist positions. Empiricism and poststructuralism both eschew talk of unobservable structures or causal entities. As a result, the only conceivable sort of comparison is one that involves events that are similar at the concrete level. A psychoanalytic comparison of the imaginary and symbolic identifications of Europeans and Chinese in the 17th century, for example, would be rejected from a positivist standpoint not on the grounds that psychoanalysis is only valid within the modern West but because concepts like the unconscious are inadmissible in all cases. Depth realists, by contrast, are comfortable invoking theoretical and conceptual structures. But because they are not philosophical rationalists, they would argue that we cannot possibly determine the limits of psychoanalysis (or any other theory) a priori, without empirical and comparative study. Yet they would not expect the existence of hypothesized structures to be easy to refute, since mechanisms are never related to events in a universal way within open systems.

The more familiar type of comparative research, which we can call *actualist comparison*, is an alternative to comparing phenomena that have little in common at the empirical level other than the putative effect of some causal mechanism. This involves comparing a series of events construed as empirically commensurable. The positivist comparative method (“macro-causal analysis”) thus is identical to actualist comparison at the level of basic research design. There is nothing wrong with this; indeed, at least one critical realist (Lawson 1998, 1999) suggests that social science comparison should be organized around empirical demi-regularities. There also may be ethical or political reasons for organizing a comparison among phenomenally similar (or different) entities or events.

As we saw in the previous section, however, some events are unique at the phenomenal level. By confining comparative explanatory analysis to repeated events we relinquish the ambition of trying to explain some of the most significant world-historical events. But critical realism is not alone in focusing attention on structure-changing events (see Bhaskar 1979; Sewell 1996). Although some sociologists

seem to believe that their discipline has low public status because of its failure to emulate the natural sciences, it is more likely that the public is especially interested in those pivotal and unique social events that positivism leads sociologists to avoid.

Another source of confusion surrounding actualist comparison is that even where there are identifiable “types” of empirical objects we cannot assume that each instance is determined by the same concatenation of causes. Thus, while Lawson recommends that research projects should start from the identification of contrastive empirical differences and allows that the phenomena in question be singled out because they are “unusual, undesirable, or of interest in some other way,” it is not obvious that this strategy will allow one to focus on and to identify “single sets of causal mechanisms and structures” (1999:38, 40, emphasis mine). Indeed, a research project constructed solely along the lines of actualist comparison and conducted with careful attention to complex structures of causality is, ironically, just as likely to transform itself into a series of parallel case studies tracing different patterns of causality as it is to find a single “invariant causal configuration.”

A critical realist comparative research strategy combines both orientations: comparison across mechanisms and across events. Empirical phenomena may be selected for comparison for explicitly political or “interested” reasons or because we suspect them of having similar determinants. But they also should be selected because they are believed to be relevant to uncovering or illuminating the causal mechanisms and structures of interest.

AN EXAMPLE OF COMPARISON ACROSS EVENTS AND GENERATIVE STRUCTURES: THE FORMATION OF COLONIAL NATIVE POLICY

As an example of this strategy, my ongoing research on German precoloniality and colonialism (Steinmetz 2002, 2003, forthcoming c) involves comparisons organized at both levels: the real and the actual. At the empirical level, I compare three colonial states: the late-19th-century German colonies of Southwest Africa, Samoa, and Qingdao (China). More specifically, I set out to compare the radically different forms of “native policy” pursued by the Germans in these three colonies and directed at different ethnic groups within these colonies. As I argue, however, there is no single, monocausal theory of the colonial state, which like all states is construed best as a concrete and empirical object, not a causal mechanism (Jessop 1990). My initial definition of cases does not involve any theoretical translation, since all three of these political entities were described by the German and colonized actors at the time as colonies.³⁸ Similarly, *native policy* (*Eingeborenenpolitik*) was the term used by the colonizers to describe the policies I am analyzing. A more difficult question involves the word to use in referring to the addressees of native policy. The Germans generally spoke of their colonial subjects as being divided into “tribes” (*Stämme*) or “races”; the colonized typically used specific proper names (or epithets) in referring to other native groups and to themselves. I therefore use the term *ethnic group*—a theoretical rather than a historical “folk” concept—to refer to the members of a relatively coherent cultural group sharing a language, some customs, and at least

³⁸ In contrast to the other two cases, Qingdao was governed by the German Navy and not by the Foreign Office or (later) the Colonial Ministry. Yet the language of colonialism was used systematically, from the very start, in official discussions of Qingdao.

some sense of common identity. The comparison of colonial Southwest Africa, Samoa, and Qingdao is thus an actualist comparison.

At the same time, however, this is also a depth-realist comparison. I am interested especially in exploring the differing effects on colonial native policy of three social mechanisms: (1) precolonial ethnographic discourse or representations of the to-be-colonized; (2) competition for cultural distinction among different sectors of the German colonizing elite; and (3) imaginary identification of colonial officials with images of the colonized. Each of these mechanisms is linked to a broader theoretical discussion. The effects of ethnographic representations on native policy are thematized in colonial discourse theory (Said 1979; Mitchell 1988). I theorize intra-elite struggles for class distinction in terms of Bourdieu's (1984) theory of symbolic capital, reconstructed along psychoanalytical lines. The cross-identification of colonial officials with images of the colonized is theorized in psychoanalytic terms as imaginary identification (Lacan 1991:134–48; Lagache 1961; Laplanche and Pontalis [1967] 1973:144; Žižek 1989). The cases are selected not only because they were all “colonies” in the worldviews of the relevant actors but also because they can be expected to reveal the workings of these causal mechanisms. Before conducting the comparisons in ways that emphasize these particular mechanisms, I first write individual explanatory histories—case studies—of each of the three colonies and the specific projects of native regulation within them. Only after completing this stage of parallel case research is it possible to move to the level of comparison. Case studies and comparisons then make possible a third aspect of social research, the reconstruction of theory. Specifically, I am able to reconstruct the initial understandings of the structural mechanisms discussed by colonial discourse theory, Bourdieu, and theories of the “colonial mirror” (e.g., Cannadine 2001).

CONCLUSION

The production of sociological knowledge involves movement among case studies, comparisons among case studies, and theory. Any social science oriented toward explanatory accounts will be necessarily involved in the study of specific cases. Within open systems like the social, any event is the product of a multiplicity of generative mechanisms interacting in unpredictable ways. Because our only access to underlying structures or mechanisms is through the empirical event, explanatory case studies are an indispensable part of social analysis. Indeed, explanation is the privileged province of the single social event, phenomenon, or process. Even in an explicit, full-fledged comparison, the researcher will have to trace the working of a range of important generative mechanisms, even if she is interested in eventually emphasizing just one of them. And this almost always will involve telling stories about specific case histories. Given the necessity of reconstructing meaning and of studying the effects of mechanisms in overdetermined, open systems, it is implausible to expect comparisons to be anything other than small-N comparisons.

Every theory, by contrast, is concerned with a generative mechanism or structure. Elaboration of the picture or model or a given mechanism is a necessary and semi-autonomous part of the scientific process, as has been recognized by writers from Popper to Paul Feyerabend and Rom Harré to Roy Bhaskar. The three forms of sociological activity most disparaged by methodological positivism—case studies, small-N comparisons, and the semi-autonomous development of social theory—thus turn out to be the three core activities of any social science.

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