SOCIOMETRY:
PROSCIENCE OR ANTISCIENCE?*

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Criticisms of the scientific status of sociology possess some validity when applied against narrowly positivist interpretations of sociological methods and metatheory, but do not undermine the scientific project of formulating generalized explanatory models. (1) Critics allege that sociology has made no lawful findings; but valid general principles exist in many areas. (2) Situational interpretation, subjectivity, reflexivity, and emergence are alleged to undermine explanatory sociology, but these topics themselves can be explained by a widened conception of science that allows informal procedures in theorizing aimed at maximizing explanatory coherence. (3) The fact that intellectual discourse itself is a historically changeable social product does not invalidate objective explanatory knowledge. (4) The historicist claim that there can be no principles that hold across particular times and places is invalid and rests upon a confusion of underlying generative principles with the complexities of the empirical surface of history. (5) Metaheoretical criticism of the concept of causality does not undermine a sophisticated conception of scientific sociology. Sociological knowledge can and does advance, but it depends upon building the coherence of theoretical conceptions across different areas and methods of research.

In recent years there have been a variety of assaults on the conception of sociology as a science. These include the following themes. (1) Sociology has failed to produce valid findings or lawful generalizations. (2) Deterministic laws do not exist because social action consists of situational interpretation, based on human subjectivity, reflexivity, and creativeness. (3) We are locked in a world of discourse; society itself is a kind of text that we read in different ways at different times. (4) The foregoing position is often connected to historicism, the claim that only historical particulars exist, and no general laws can be found that apply at all times and places. (5) Finally, there are various technical criticisms of scientific methods and metatheory, especially of the conception of causality. The philosophy of science today is in a postpositivist mode; and a scientifically oriented sociology, it is said, is intellectually out of date.

The various criticisms are not necessarily united. Some of them include important points that contribute to the widening of sociological knowledge. But the overall thrust of the criticisms, that sociology has and can have no scientific validity, I believe is wrong. To be sure, science is not the only valid mode of discourse, or of knowledge. Sociology, like many other intellectual disciplines, can be concerned with empirical descriptions including both contemporary social conditions and historical sequences; it can discuss moral issues, propose or excoriate policies for practical action, and compare existing conditions against ideals; its can discuss foundational, methodological, and other metaheoretical issues. But the core activity that gives the field of sociology its intellectual justification is the formulation of generalized explanatory principles, organized into models of the underlying processes that generate the social world. It is these that determine how particular conditions result in particular kinds of outcomes. It is these generalized explanatory modes that constitute a science.

I will attempt to show that none of the difficulties raised by the antiscience arguments prevent sociology from formulating valid generalized explanatory modes. We have the basic structures of several such models already, in areas ranging from microsociology, through formal organizations, to macrosociology. It is not inevitable that

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sociology must be a scientific failure, nor has it failed. Those who attack scientific sociology typically fight against a caricature of "positivism" at its narrowest. On the other hand, many practitioners of allegedly scientific sociology have assumed just those narrow conceptions of method and substance that make them vulnerable to the antipositivist attack.

In what follows, I will deal in turn with the major criticisms of sociological science but also suggest what we can learn from the antiscience critiques. The flux of situational interactions, human subjectivity and reflexivity, the dynamism and episodic movement of macrostructures, are all part of sociology's subject matter. It has been the merit of some of the antiscience positions to bring these to our attention, and even to explore their pattern; but this exploration has made it possible to formulate these processes more generally and hence to widen the realm of explanatory models which are the core of scientific sociology.

THE ALLEGED FAILURE OF SOCIOLOGICAL RESEARCH

One line of attack dismisses sociology because we have no findings. After almost 100 years of research, we still have come up with no valid generalizations, no laws of sociology. This criticism is often made by outsiders to our discipline; for instance, Alasdair MacIntyre (1984) uses it as grounds for his argument that there can be no secular, nontraditional basis of morality; Alexander Rosenberg (1980) argues that since the social sciences have, and can have, no laws, any sociological explanation must come from a sociobiological level of determinism. Sociologists themselves sometimes also make the same dismissal, usually in the context of a discussion of alternative metatheories (e.g., Spencer 1987).

But the charge that sociology knows nothing, that we have no valid generalizations, is patently untrue. Let us review a few of these, starting from the microlevel and moving up to the macrolevel.

(i.) The longer, more intensely, and more exclusively persons interact with each other, the more that they will identify with one another as a group, and the more pressure they will exert and feel for conforming to local patterns of behavior and belief, provided that they are not unequals in power or competitors for scarce resources. Variations on this principle have been formulated numerous times, based on a wide range of research. Homans (1950) stated the basic point, drawing upon studies of informal groups in industrial settings, as well as anthropological research and experimental small groups. Durkheim ([1912] 1954), in analyzing the central dynamic of religious rituals, seized on the similar principle that intensely focused interaction produces moral solidarity and conformity to the group's symbols; Goffman (1967 pp. 1–136) extended this model to sociable conversations. The famous Asch (1951) experiments demonstrated the effect of group cohesion upon pressures for conformity even in visual perception. Symbolic interactionist theory converges on the same point: if a person's concepts are derived from the stance of a generalized other based on his or her social experience, then what individuals think must be influenced by their patterns of interaction. Research on self-conceptions (M. Rosenberg 1979; R. Turner 1978) can be regarded as a variant on this principle, demonstrating the influence of group membership and solidarity upon beliefs about oneself; in another application, expectation states research (Berger, Wagner, and Zelditch 1983) shows the effects of group pressures upon task performance (as did W.F. Whyte's famous Street Corner Society, 1943). Studies of networks (Bott 1971) provide the equivalent formulation: network cohesion results in homogeneous attitudes. The coherence among these various kinds of theory and research constitutes strong evidence that the interaction-density/solidarity/conformity principles are true.

(ii.) Human cognitive capacity is limited; accordingly, the more complex or uncertain a situation, the more that participants fall back upon a taken-for-granted routine and focus on the particular area that presents the most dramatic problems. There has been a great deal of convergence on this principle from very different points of view. Herbert Simon presented this as the principle of "bounded rationality," which explains why members of organizations engage in "satisficing" in most areas while troubleshooting the most pressing problem at any given time (Simon 1957; March and Simon 1958, pp. 173–71). From a very different angle, Garfinkel's (1967)
ethnomethodological experiments demonstrate that individuals cannot cope with the full complexity of social arrangements and their justifications (especially since questions about these justifications are in principle endless); accordingly, people actively resist whenever they are forced to question more than a very few taken-for-granted routines at once. Corroborating evidence comes from experimental research on judgments under uncertainty (Kahneman, Slovic, and Tversky 1982).

There are many ramifications of this general insight regarding human strategies for operating with bounded rationality in a complex world; one might say it is one of the common themes of the social sciences in the late 20th century. It affects our efforts to construct models of human cognitive processing. It has implications for understanding the nature of organizations and of the social shaping of markets (Williamson 1975; White 1981). It explains why a source of power within organizations and occupations is occupying a position that has access to a crucial area of uncertainty, whose occupants are able to define to the rest of the organization what kind of nonroutine reality they are facing (Crozier 1964; Wilensky 1964). The cognitive limitation principle also implies that change on the macrolevel would follow a pattern of long periods of routinization broken by sudden episodes of restructuring. I would suggest, in this light, that the microprinciple of cognitive limitations is implicated in Perrow’s (1967, 1984) macromodel of organizational systems, in which the combination of nonlinearity of organizational processes and tight coupling among them results in episodic “system accidents.”

(iii.) On the macrolevel of the state, an important principle is: A political crisis arises when a state’s apparatus of military control is broken down by internal dissension among elites: this breakdown is especially likely when there is military defeat and/or the economic strain of long-term military expenses beyond the organizational capacity of the state to collect revenues. This principle, as stated is limited: it tells us when a revolt will break out, not who will win, nor what kind of social transformation, if any, will follow. This pattern has many variations: when the factors causing breakdown occur in a centralized state and coincide with popular class conflict mobilized by changing property relations, the result is a major social transformation, a “revolution” in the fullest sense of the term (Skocpol 1979). The interaction of demographic growth, money supply, and inflation can be a prior determinant of the state’s fiscal crisis (Goldstone 1986, 1987); the position of a state within the world system affects its ability to bring in revenues ahead of its military expenses (Wallerstein 1974, pp. 133–47); geopolitical patterns determine which states will become overextended and hence unable to sustain themselves militarily (Collins 1981b, 1986, pp. 145–209). In particular kinds of military/state organization (such as most premodern empires), the result of geopolitical or fiscal crisis is disintegration into smaller coercive units. A more complete theory of state crises, both revolutionary and nonrevolutionary, would have to take into account these kinds of considerations. But I believe we can accept with confidence the basic principle of state military/fiscal crisis leading to disintegration of the apparatus of coercive control, and that in turn leading to a revolt of subordinates.

My purpose in citing these principles is merely to disprove the argument that sociology knows nothing, and hence that a social science is impossible. I have not attempted to pick out our most important principles, to set

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1 Power can also be based on resource dependence in a network structure (Cook, Emerson, Gillmore, and Yamagishi 1983; Willer 1987); and on coercion, exercised with varying degrees of effectiveness in different network structures (Willer 1987; Schelling 1962). Power also depends on the organizational distribution of control resources (Etzioni 1975) and on the conditions of mobilization and conflict among opposing groups, both civilian (Tilly 1978) and military (Collins 1988b). Power is a complex phenomenon; we have made progress on a series of partial theories, without yet pulling them all together.

2 A fiscal and/or military crisis is not the only route to the dissension among elites that leads to the disintegration of the coercive apparatus. The fiscal/military crisis theory is not a complete theory of all revolutions and other revolts; but it appears to be true, as far as it goes, and it covers a very important portion of events. As Paul DiMaggio points out (personal communication), this theory is related to a more abstract explanatory principle, applicable in many contexts, about the disintegration of an organizational system.
forth a systematic theory, or to assess the overall state of our knowledge in sociology (an effort to do so is in Collins 1988). Hence these principles may look eclectic, lacking the elegance of an overarching image of the social world. But I have given some hints how these principles, although they are chosen almost at random from different parts of the field, may be coherent with one another, and I have suggested that such principles are not trivial but lead to sociological insights into a wide range of important questions. There are a good many other principles of this sort, especially in organization theory but also in other areas of sociology. There is of course plenty of room for improvement in our precision and in our understanding of the scope of these theories, but enough evidence has built up that we can be confident we have good approximations of how some important processes work. Many different social scientists have contributed to this knowledge; we do have a core we can build upon, and a discipline we can be proud of.

Is there anything salutary in the criticism that sociology knows nothing? It is not true, but it should serve to remind us that sociology has a serious problem in professional self-presentation. And within our own ranks, we need to pay more attention to the explicit cumulation of what we do know.

SITUATIONAL AND REFLEXIVE INDETERMINISM

It is sometimes held that deterministic explanations are impossible because social action consists of situational interpretation, subjectivity, reflexivity, and emergence. This is an old criticism, going back at least to Dilthey’s distinction between the *Geisteswissenschaften* and the *Naturwissenschaften*, and ultimately to the German Idealists’ revolt against the Enlightenment. In recent years this line of criticism has become very prominent, so much so that one might characterize the late 20th century as a time of neo-Idealist revival.

It is important to recognize that subjectivist and interpretive schools of thought in recent sociology have made positive contributions to sociological knowledge. On the methodological side, these approaches have fostered microresearch in naturalistic settings, empathetically entering into the processes, feelings, and thoughts of real people as they enact society. Such work includes the participant observation practiced by symbolic interactionists, as well as Goffman’s efforts to map out the nature of everyday life. Without such research, we would be left studying the fundamental realities of sociology’s subject matter only indirectly, at methodological arm’s length. In the last few decades, there have been further innovations in microresearch, including ethnomethodologists’ breachings, and culminating in perhaps the closest empirical analysis ever done in the social sciences, using audio and video recordings of natural interactions as a basis for developing formal models in conversation analysis.

Most of this work would have been ruled out by textbook canons of research 30 years ago. The sense of alienation form the sociological “Establishment” felt by many interpretive sociologists is no doubt due to memories of having lived through that time. The vituperation against “positivism” is partly the expression of an oppressed intellectual minority against their long-standing oppressors after finally gaining a foothold in respectability.

But we need not assume that all connection between interpretive and scientific sociology is now severed, and that microresearch with interpretive methods should be institutionalized as a kind of “separate but equal” enclave. On the contrary, the achievement of the interpretive microsociologists should broaden our sense of acceptable methods for sociological science. Clearly, a scientific method for our field cannot rule out studies of the subjective; sociological science cannot be founded on an exclusionary behaviorism (although we should avoid going to the opposite extreme of ruling out the importance of behavior, including unconscious behavior). A science does not have to be built out of “hard data” in the narrow sense. What makes it scientific is its ability to explain the conditions under which one kind of pattern holds rather than another, in whatever realm those patterns may be found.

Similarly, sociological science cannot be equated to a rigid operationalization of all of its concepts. Not only is it legitimate for explanatory theories to include nonoperational concepts at some levels; even a very positivistic model must include general orienting concepts within which its specific hypotheses and operationalized variables are located.
We always need a model of what the world is like, a mental picture of what are the fundamental processes and entities and how they link together (see J. Turner 1988; Willer 1987). Specific hypotheses make sense only in terms of some such background assumptions about what kind of world we are dealing with. The narrower traditional forms of positivism, in demanding total operationalization of all concepts, merely took for granted an unconscious conception of the world in which its explicit hypotheses were lodged. Such researchers could easily lock themselves into commonsensical or ideological assumptions, which their focus on the foreground of research techniques kept them from seeing. The interpretive sociologies do us a service in forcing the issue, so that we are made explicitly aware of those background models and thus able to theorize them.

The Place of Informal Concepts and Intuitions in Theory

The notion of a complete and rigid formalization, operationalization, and measurement of everything in a scientific theory is a chimera. There are informal concepts and intuitive leaps at several points. There is always a metatheoretical stance about what we are doing intellectually in the first place. Scientific theory sketches a model of the aspect of the world under consideration; hypotheses are derived from this, by a process of derivation that itself involves intuitive leaps. When operationalizing concepts for empirical test, we always make another intuitive leap in deciding that particular measurements or other observations actually bear upon the theory. These intuitive or informal leaps are areas in which theoretical discussions can take place (or, in many cases, ought to take place). But they are not illegitimate. That is simply the way the world is. They do not undermine our ability to have a science, for all sciences have these places where there are intuitive leaps. If physical scientists sometimes forget this and talk in crude positivistic terms as if they report "nothing but the facts," that is because they have been successful at making the right intuitive leaps as their scientific procedures have cumulated, so that they now have workable models that they know intuitively how to apply to most of the things they study.

Everything whatsoever has "fuzzy edges," so to speak; even numbers and logical relations have some areas of indeterminacy. We encounter this when number systems are extended to infinity or to infinitesimaliy, and in nonconverging algebraic series. Many systems of equations are unsolvable mathematically. Even rather restricted formal systems of logic always encounter Gödelian incompleteness; more complex systems of multivalued, modal, and other nonclassical logics have even greater areas of disagreement in interpretation (Lewis 1986). I have expressed this elsewhere (Collins 1988, Appendix A) in the following formula: mathematics is always embedded in words. But notice what conclusion follows: not that mathematics and mathematical science is impossible; on the contrary, a successful science is possible even incorporating areas of fundamental uncertainty, dealt with by tacit and informal understandings. Tacit knowledge is knowledge too, as long as it works.

Whatever our kinds of explanatory models, we still need to be concerned about validating our theories. The fact that we are always involved in interpretations (and at many levels) does not mean that we can accept every interpretation offered at face value. Typically we cannot decide these issues by a simple operationalization, measurement, and one-shot test. But the physical sciences face most of these same problems, and their success in many areas shows that some research programs and theoretical models do prove themselves in the long run over rival ones; there can be convergence on models that work, that capture the central ways in which the world is, even if these scientific models are inevitably vague and cluttered around the edges. Successful heuristics and intuitions are possible, and unsuccessful ones which lead us into blind alleys can be ruled out.

The crucial criterion is that the best theory (with its ancillary assumptions and heuristics) is that which maximizes coherence; it brings together the most successful explanatory models into a consistent overall picture of how the world operates. Methodologically, empiricism can be part of the coherence criterion; the best validated theory is the one maximally grounded to the empirical world via the various explanatory submodels it incorporates. An extreme, all-or-nothing empiricism is impossible; but a flexible empiricism, working with imprecisions and intuitive
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cancepts where necessary, and making a great deal of room for theoretical work that ties things together, is a central part of science. One needs to work nonpositivistically, so to speak, to be a successful positivist.

It is in just this vein that the interpretive schools have contributed substantively important theories. Among these are the symbolic interactionist theory of the self (part of which is coherent with well-established principle [i.] mentioned above); the ethnomethodological theory of everyday rationality (which is coherent with the limited cognition principle, [ii.] above); as well as other existing and potential contributions to sociological knowledge. Goffman’s (1959) model of dramaturgy in everyday life is a model, in the sense of “what the world is fundamentally like” that I mentioned above; from this base, one can go on to develop specific explanatory principles.

I have argued, for instance, that it provides a base for understanding the differences in class cultures, between those wielding power and those subjected to power (Collins 1988, pp. 203–14).

Many sociologists in the interpretive camp, however, claim that their major substantive finding is the impossibility of deterministic theories (e.g., Blumer 1969, p. 60). In their empirical investigations, they discover, above all, emergence, unpredictability, situationality, human capabilities for subjectively reflecting upon and changing social conditions. Here we have an argument about what kind of model we arrive at, not about whether it is possible to have any model at all.

But is it true that the main feature of the social world is unpredictability, overwhelming any determinate processes? I suggest that it is not true, and that this perception comes from selectively focusing on a limited portion of the social world. Although much (but not all) content of sociology consists of human subjectivity, it does not necessarily follow that this cognition and feeling are indeterminate. Without pursuing this point into the status of theories of cognition and emotion, let us think of Goffman, the acknowledged genius of micro-interpretive sociology. Goffman used soft methods, but he believed the world he was studying is hard. His social theory of language (Goffman 1981) grounds cognition in the social ecology of interaction. The complexity and reflexiveness of human subjective worlds come from the many possible “reframings” that actors can carry out (Goffman 1974), but Goffman did not regard this framing activity as free-floating, and he rejected the suggestion that it reduced the world to a kind of psychedelic fantasy. Transformative reinterpretations of subjective reality are linked together into orderly transformations, among “adjacent” frames, so to speak. For Goffman, the bottom level frame, out of which all the others arise, is the physical interaction of human animal bodies, an ecological baseline that links Goffman theoretically to Durkheimian theory of the ritual basis of solidarity and of symbol construction (for elaboration, see Collins 1988, pp. 188–203, 291–97, 320–34).

It is possible, then, to have a structured understanding of subjectivity. In that perspective, the explorations of the subjective side of human life in the last few decades, notwithstanding their sometimes extreme pronouncements, have contributed elements towards a much more sophisticated explanatory theory of mind than would have been possible before.

How Unpredictable is the Social World?

Let us confront the issue of unpredictability head on. How much of the social world is unpredictable? A great many things are highly predictable. It is the pattern of people going to work over and over again to the same jobs that makes up much of formal organizations; repetitive patterns make up households and families; networks of friends and acquaintances similarly are constituted out of behaviors, cognitions, emotions, and communications that are heavily patterned. For regularity and predictability to exist, it need not even be the same persons who repeatedly interact. Most stores have only episodic relationships with particular customers, but it is the predictability of a certain number of people coming in to shop that allows businesses to stay open at all. Although microsociology is the theoretical bastion of indeterminacy, it should be apparent from these examples of everyday life that the microlevel has a high degree of predictability.

The theory of indeterminacy seems to rest upon two suppositions. One is that this kind of predictability is banal. It is true, but it is too boring for sociologists to pay attention to it; we should focus on something that everyone does not already know. So one might say there is a built-in bias toward studying the dramatic and unpredictable. But I would deny that what is banal from a participant’s point of view is
necessarily banal for an explanatory theory. On the microlevel, Garfinkel took the banality of everyday life as a product to be explained, and uncovered cognitive mechanisms that allow it to happen—and that show us the pressure points where these mechanisms are interfered with. On the macro- and mesolevels, good sociological work consists in reframing the banality of taken-for-granted patterns. Although it seems natural to a particular person that she or he does a job and chats with friends every day, there is much that sociologists have uncovered as to why jobs are structured in this way rather than that, why these persons are friends rather than others, and so forth; these are the contents of, for example, organizational theory, exchange and network theories, and stratification theory.

The other supposition leading us toward theoretical indeterminacy is more valid. It is the recognition that situations can sometimes change very rapidly: that there are negotiations, conflicts, sudden insights, decisions, and, on the macrolevel, movements, revolts, and revolutions. All this is true. But do we take this as the end of analysis, or as a beginning point, a challenge to develop theories to explain when such sudden shifts will occur? I have already noted that on the macrolevel, we know some of the crucial features that make revolutions predictable. On the microlevel, indeterminacy is typically grounded in some version of the Thomas Theorem. But if situations are determined by subjective definitions, we can still ask what determines what the definition of the situation will be. What sometimes makes situations seem to have an unpredictable, emergent quality is that one looks at them from the point of view of a single actor who knows only his or her own intentions. But if we know enough about all the actors in the situation, and the structure of their interac-

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people deliberately go into an encounter group or similar group-dynamics situation, they are implicitly making use of the microprinciple ([11], above) regarding how group solidarity is generated, because they want its emotional payoff. Their mistake typically is to overestimate how long such solidarity and emotional energy will last, after a temporary group of this kind is disassembled. Knowing the principle does not undermine it.4

It would be rash to predict that sociological science will explain everything. There may well be a considerable residue of indeterminism even if sociology is successful far into the future. But our intellectual impetus comes from pushing back the frontier. To preach indeterminism and nothing further seems to me a parasitical strategy, since it is intellectually of interest only if there is some body of theory already existing that one wishes to counter. The constructive job is to build as good an explanatory theory as we can.

I have tried to establish that a science can operate flexibly, and on all kinds of subject matters. Against this backdrop, I will comment more briefly on the remaining criticisms of sociological science.

SOCIETY AS DISCOURSE

Another criticism goes as follows. We are locked in a world of discourse; society itself is nothing more than a kind of text that we read in different ways at different times. This is a popular theme now, deriving from French structuralism and its offshoots. It has created a veritable revolution in the world of literary criticism. This is understandable as a professional ideology, elevating literary theorists' own field by the assertion that all reality is a piece of literature. "Discourse" has thereby won a wide influence in the larger intellectual work (including the publishing business). It fits well especially with the particularistic, descriptive themes of anthropology and also makes inroads into the cosmopolitan side of sociology (e.g., Brown 1987; Giddiener 1985).

But the upsurge of sociological research on culture is not always merely relativistic. It has also been carried forward in a deterministic mode: we have rather good researchers and theories about the material and organizational base of the production of culture, both as it is distributed among social classes, as well as in more specialized culture-producing institutions (Bourdieu 1984; DiMaggio and Useem 1982; Coser, Kadushin, and Powell 1982). Culture does not simple organize itself; it is organized by social processes.

Foucault (1969), the most significant of the French "discourse" school, has documented the social bases of ideas in such practical fields as psychiatry and law. But Foucault does not attempt to undermine the validity of his own discourse as a historian. Moreover, the historical patterns that he emphasizes as determinative of the field of discourse—the bureaucratization and specialization of social control agencies, the shift in public/private boundaries, the move from ritual punishments to a reflexively conscious self—are highly congruent with Weberian (Weber [1915]1946) and Durkheimian/Maussian (Carrithers, Collins, and Lukes 1985) theories of modernity. The best of this European work reinforces rather than departs from the central cumulative traditions of sociological knowledge.

The intellectual popularity of "discourse" as a master worldview is bolstered by the success of the sociology of science in showing how knowledge is a social construct. This success is indeed something to cheer about. But we should not forget that the sociology of science is an empirical research discipline, which has made great strides in the last 30 years in cumulating sociological models of what determines the kind of knowledge produced by particular kinds of organizational conditions (see Whitley 1984, for recent summary and synthesis). Notice what this does for the claim of undermining scientific knowledge. There is a determinism at the very heart of this alleged indeterminism; sociology of science itself is becoming a scientific success.

This raises some interesting issues of reflexive self-consciousness. Some sociologists of science (e.g., the British school around Michael Mulkay, Harry Collins, Steve

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4 It is sometimes said that knowing too much about how social relationships operate makes them go flat. Can an exchange theorist or someone who applies Durkheimian and Goffmanian ritual theory fall in love? Does the theoretical self-consciousness destroy the situation? I can attest to you it does not; robust social processes have a quite wonderful power, overriding a weaker process like momentary reflectiveness.
Woolgar, and others; see Knorr-Cetina and Mulkay 1983) go so far as to argue that science is merely a set of competing power claims. The only democratic path is to allow no single voice to have a privileged position; hence, Mulkay (1985) and others have taken to writing and presenting papers in a “New Literary Form,” in which the author steps aside and lets many competing voices appear. The result is entertaining, but it is not clear why reflexiveness should prevent scientific knowledge. Bloor (1976, 1983), who powerfully applies a Durkheimian theory, argues in his “strong program” that a sociology of science can and should explain not only false knowledge claims but also true knowledge.

From an organizational perspective, the power claims in scientific discourse that Mulkay and others document are themselves part of fairly predictable patterns. Different kinds of intellectual discourse (i.e., particular scientific disciplines) are embedded in organizations, and they themselves can be understood as organizational forms. Hence, organizational theory (especially the theory of how various kinds of task uncertainty and of resource dependence affect organizational behavior and structure; see Whitley 1984; Fuchs and Turner 1986) shows scientific discourse is not a free-floating construction but appears in predictable ways under given circumstances. Furthermore, we know that organizational structures are at least partly determined by the task environments in which they work (Collins 1988, pp. 467–85). This means that the objective nature of the subject matter is one of the determinants of the social activity (including the discourse) that makes up science.

Arguments that exclusively emphasize discourse are one-sided; although there is a culturally constructed component in any knowledge, it also can be knowledge of something. Indeed, any argument about the social basis of knowledge is self-undermining if it doesn’t have some external truth-reference as well—otherwise why should we believe that this social basis itself exists? We need to get beyond polemically one-sided epistemologies, of either the subjectivist or the objectivist sort; a multidimensional epistemology can take account of the way we live in a cultural tunnel of our own history, but still we can cumulate objective knowledge about the world.

HISTORICISM

Historicism is the claim that only historical particulars exist, and no general laws can be found that apply at all times and places. This argument arises to some extent in conjunction with other antipositivist criticisms, as a kind of oppositional united front. But it also has its autonomous basis in the actual practices of historical sociology. It is a price we are paying for a very good thing. The last 20 years, since the publications of Barrington Moore and Charles Tilly in the 1960s, have been a golden age for historical sociology. We have learned a great deal about macroprocesses by looking at historical materials with a sociological eye and by making comparisons across societies and times. This, for instance, is how Moore (1966) showed the connection between the forms of capitalist agriculture and the differing political structures of modern states. But although this shows us something about particular states (17th-century England, 19th-century United States, etc.), the underlying theoretical conceptions have a more universal application; it is for this reason that a family of models related to Moore’s (Paige 1975; Skocpol 1979; see also Stinchcombe 1961; Weber [1923] 1961, pp. 81–94) have been powerfully applied to other times and places.

Historical sociologists are under two sorts of pressure to announce themselves as historicists. One is that they come into a good deal of contact with historians. Historicism is a kind of professional ideology for historians; they make their living by describing particulars, and furthermore the pressures of intellectual competition within their field lead them to specialize and to resent intruders on their turf. Hence, historians tend to dislike any suggestion that there are general processes, and particularly that such processes might be uncovered by comparing across times and places (i.e., across the boundaries of their research specialties). Historians often espouse an ideology that would make it impossible to consciously develop a general explanatory theory; and many historical sociologists respond to criticism from historians by giving in to their ideology.

But historians’ claims are not consistent. In interpreting their particular cases, they implicitly draw upon some ideas of what general structures are and how patterns of social motivation and change operate. The analysis
of historical reality can hardly be approached with a tabula rasa; historians have theories whether they know it or not. What makes someone a great historian, one whose work commands widespread intellectual attention, is typically his or her ability to create a theory, to show the more general skeleton underlying the narrative of particulars. Lesser historians are usually those who operate with naive, taken-for-granted conceptions, or with old theories that have passed into common discourse. Historians at their best have been building sociological theory, although they have not always discussed it as such, and have typically interwoven it with their particular historical description, sometimes with considerable artistry and dramatic style.

I have no sympathy with the blanket claim that historical particularism is all that is possible; on the contrary, we cannot even see particulars without general concepts. But there is a more valid reason why historical sociologists tend to work at a low level of generality with theories embedded in the understanding of a particular range of cases. Even if one's aim is to develop general theory, the macromaterials of long-term history are extremely complex. Although we may know something of rudimentary processes, getting any very abstract picture of the overall combination of conditions operating in historical change is very difficult. Theoretically oriented historical sociologists have worked with intermediate approximations to a level of explanatory generality. For instance, Weber's massive historical comparisons of the conditions involved in the rise of rationalized capitalism have yielded many general analytical points, but embedded in an account of certain concrete sequences of world history. This same halfway combination is found in modern work such as that of Mann (1986) on the conditions determining the history of social power, of Goldstone (1986, 1987) on the state crises and social transformations; and I will admit that my own work (Collins 1986), on such matters as the extension of Weberian theories of capitalism or of sexual stratification, is also quasi-embedded in accounts of particular historical developments. Such works are a challenge for theorists to attempt to departicularize what we have learned, to pull out the more fundamental theoretical structures underlying these accounts.

We will always have two levels: a theoretical level of abstract and universal explanatory principles, and a level of historical particulars. Insofar as our theories are successful, we will become better and better at showing how the myriad arrangements of historical particulars are generated by particular combinations of variables in theoretical modes. There will always be tasks for historians, and for historical sociologists, in making this kind of concrete interpretation. At the same time, exploring concrete history is one of the main ways that we make progress in building and validating our general models—although such theory is built and validated by its coherence with all kinds of sociological research, contemporary as well as historical.

It is not true that there are no explanatory principles that hold generally across history. The three examples that I gave at the beginning of this paper are completely coherent, as far as I know, with evidence from any historical epoch, and there are many more such principles. Of course, some principles may not be applicable because the independent variable does not exist in a given historical situation. One cannot predict either the existence or the nonexistence of revolutionary crises if there is no state. But there is doubtless a more abstract formulation related to principle (iii.) that would apply to sources of crises in political power in stateless societies. Macroprinciples in general are likely to be more complex than micropinciples, involving combinations of many processes. But we have at least rudimentary outlines of promising principles on the macrolevel as well. It is a mistake in criticizing the limitations of particular theories (e.g., in widening the reference from allegedly independent societies to a world system, or overthrowing unilinear evolutionism or development theory or functionalism), to go to conclude that general theory is impossible. The result of this critical development is not no theory at all, but improved theory.

THE METATHEORETICAL ATTACK ON CAUSALITY

Critics of explanatory sociology like to point out that the consensus in the philosophy of science has changed since the heyday of logical positivism. It is generally acknowledged that programs such as that of Carnap, which attempted to construct all scientific
knowledge from sensory experience organized into statements of formal logic and mathematics, have failed. There is no consensus now on an alternative epistemology for science, although most philosophers make room for theoretical preconceptions and programs, and for pragmatics in both theory formulation and research (Quine 1969; Dummett 1978; Putnam 1983). It would be generally agreed that mathematics and formal logic are not self-grounding, and there is considerable recognition of the role of nonformalized statements in any body of knowledge. Along with this, there is a loosening up in the conception of what constitutes knowledge: not merely the ideal of classical physics, but widened to include the rather different scope of knowledge in the biological and earth sciences, history, extending perhaps even as far as alternative forms of knowledge embodied in art (Goodman 1978).

What does this mean for sociology? I would suggest that it puts sociological science more on an even footing, epistemologically speaking, with the established natural sciences. For they too operate under the same sort of epistemological imprecisions. Sociology will never be a science fitting the old logical positivist ideal, but none of the natural sciences fits that ideal either. We are not aiming at the impossible; if we can reach the degree of approximate and pragmatic success the natural sciences have achieved, that would be plenty. It is true that some sociologists may continue to uphold a methodological ideal that is closer to the crude induction-plus-mathematical-formalization model of science. I would suggest that this is particularly likely in the applied end of our field, where purely descriptive information (e.g., on the success of desegregation programs) has some immediate use, and hence straightforward induction is more likely to be pursued. But this does not affect the larger issue of the methods appropriate for building a general explanatory science.

Modern philosophy of science does not destroy sociological science; it does not say that science is impossible, but gives us a more flexible picture of what a science is. This is all to the good in consolidating a science out of the materials that sociology already has available. A number of the more specific technical criticisms mounted by the antiscience position in sociology seem to me to cling to a narrowly positivist image of their opponents and do not touch a more realistic image of science.

Criticisms of the concept of causality are frequently raised in this spirit. Causal theories are dismissed on the grounds that there is no such thing as "the cause" of anything; there is always a complex of conditions, and those in turn have antecedent conditions, which can be traced backwards and outwards in an endless web. Given causes explain something only under particular conditions, which are typically taken for granted, especially in statistical analyses of survey data that attempt to causally explain all the variance in their particular sample. Some of the attack on causality, however, has come from within the scientific camp itself (Gibbs 1972) and does not dismiss the aim of testable, generalized explanatory principles.

Certain aspects of this debate are merely terminological. "Cause" is to come extent a metaphor, a shorthand for referring to our focus on a particular portion of a complex of conditions that are involved in producing certain outcomes. Some of these conditions may be concomitant relationships among parts of a social structure, as well as antecedent conditions that determine which kinds of outcomes will follow. (See general discussions by Klein 1987; Walker 1987; Meeker and Hage 1988. As Wallace [1987] points out, there is a variety of causal patterns—continuous, episodic, multileveled, etc.) But it is important to retain this concept, whether under the term "causality" or something equivalent, for it enables us to distinguish between explanations that work and ones that are vacuous. Functionalist analysis, for example, has turned out to be a poor mode of explanation, unless it can be translated into causal mechanisms (see Stinchcombe 1986, pp. 80–100). One cannot "explain" something by giving it a name, even if that name is "norms" or "rules" or "culture"—or for that matter a "problematique" or "discourse"—any more than one can explain gravity by referring to a "gravitationous propensity." Here "causality" is useful by giving us a mechanism, which tells us what process is operating and when particular outcomes can be expected rather than others. "Causality" saves us from reifications, as well as from ideological justifications masquerading under the guise of explanations.

As we have seen above, an explanatory theory has as its core a model of "how that part of the world works," what the parts are and how they fit together. Specific causal
propositions fit into such a model and are the object of empirical testing, but they depend upon the background conditions of the whole model. Some of the objections to "causal theory" in sociology are directed at particular kinds of statistical models (e.g., in the status attainment literature) built entirely at the level of propositions. But although such models may be too embedded in a particular body of data from a given historical period and fail to make explicit the structural conditions that frame these processes, that is not to say that such causal processes cannot be incorporated in a valid theory of the larger social universe (see Campbell 1983).

Steven Turner (1987) voices a more specific objection, to causal statements of the form "the more the X, the more the Y." He argues that such propositions are literally untrue unless the correlation is perfect; but empirically there are always exceptions, and hence such propositions have no logical foundation. Turner denies that imperfect correlation can be taken as an approximation to true causal relations. He holds that there is no path, logically, from general propositions (which are always idealized and perfect) to the messy world of inexact relationships. Statistics provides no answer to this underlying issue. Theory is always underdetermined by data, and a wide-open pluralism of theories is the consequence that assumable will always be with us.

Turner’s argument, however, leads to absurd extremes. Does anyone really believe that if we had a large number of well-validated propositions of the form "in a large proportion [fill in the range of probability] of cases, the more the X, the more the Y," we would know nothing at all? Turner’s argument cuts against physical science as well as against sociology; again, I would be quite happy with the level of approximation and pragmatic success that these other sciences have achieved, whatever a purist argument like Turner’s would say about the logical status of such knowledge. On the philosophical level, Turner seems to assume a rigidly positivist conception of theory and fails to recognize that any theory involves interpretive leaps and pragmatics, including deciding that given observations are appropriately connected to a given theory. All theories are not equally valid; the question is which theory works in the largest number of contexts that are coherent with one another.

Obstacles to Cumulation and The Organizational Politics of Sociology

I maintain that when we go looking for it, we find bits and pieces of sociological knowledge lying all over the landscape. Our problem is recognizing what we have and organizing it so as to maximize its visibility. Why is this so difficult?

One reason is the fragmentation and antagonism within our field. Sociology is divided into a large number of specialties. This is hardly surprising, since there are many thousands of researchers with an interest in cultivating their own turf; and since sociology takes the entire social world (including its causes and effects) as its target, there is an enormous range of empirical things that we can study. The sheer amount and diversity of sociology give us a practical incentive not to pay attention to work outside one’s own area. On top of this, there is a diversity of methods, whose proponents frequently regard work produced by rival methods as having no knowledge value. And there is a further split into theoretical schools, which often denigrate each other as part of their competition for hegemony. These battles are especially intense when theoretical factions have political overtones, or when it is argued that only practical knowledge or politically engaged expressions of a particular sort are worthwhile. All these conditions make it difficult for us to pick out the places where theoretical explanations come together, and to assemble the bits of evidence thrown up by different approaches into a coherent pattern.

5 As Willer (1987, pp. 43, 220) points out, physical scientists typically carry out experiments, not to achieve statistical validation, but to discover the range of conditions under which a theoretically derived relationship holds. Willer (1987, p. 220) comments: “physicists were satisfied with an experiment if its results came within a factor of ten or more of theoretically predicted values. This sort of experimental error was considered ordinary and no one even considered statistically testing the result... [W]hat can it possibly mean to say that physics is an exact science?... [E]xact meant the exact use of theory, not necessarily the exact production of clean results.”

6 One of the main advantages of the natural sciences may be that there are relatively few direct
Perhaps even more important for American sociology has been a different type of split, between descriptive and theory-oriented research. The latter is the search for general explanatory principles; the former is a much more straightforward investigation of questions that, at least initially, are fully comprehensible to a layperson. This is the class of issues such as: how much social mobility is there? (are we still the land of opportunity? were we ever?) how much racial discrimination, and what progress towards its diminution? These kinds of researches could be theory-relevant, but it is not necessary that one have an explanation in terms of genuinely analytical variables in order to produce useful information. Statistical technique without much theory finds its place most naturally here. It is likely this practically oriented search for descriptive information that anti-positivistic sociologists have in mind when they attack the methodological and antitheoretical bias they perceive in sociology.

This division of labor between our different activities need not poison the atmosphere over what is a scientific sociology. Practical/descriptive research could doubtless be done better if it were grounded in more analytical theory, but for many purposes an atheoretical description is enough. What is important to recognize is that cumulating an analytical core of explanatory propositions is a different enterprise. When we attempt to cumulate our general knowledge, we have to know what we are looking for and to cull it from the mountains of descriptive material that make up so much of the research literature. Theory-oriented research and practical/descriptive research can live together harmoniously, even if they produce an information-retrieval problem.

More debilitating is an attitude that seems to carry over from practical/descriptive work into all of sociological research: the tendency to regard only the latest data as relevant. If we want to know what the equality of educational opportunity is, the survey with the latest date is obviously of most practical significance. But this “bias of the present,” a kind of headline-chasing in sociology, is one of the factors that turn our faces away from pull-

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political implications of their theories. This has enabled them to avoid a source of controversies that has clouded the analytical project of the social sciences, and of sociology above all.

Data from different time periods are a positive advantage in attempting to cumulate general explanatory principles. The change in empirical distributions that we often observe enables us to formulate principles of scope and to refine our theories. For example, research from industrial sociology and community studies from the 1930s through the 1950s led to general principles about how the experience of giving or taking orders produces divergent class cultures (summarized in Collins 1988, pp. 211–14). The knowledge accumulated remains relevant on the analytical level, even if (as seems to be the case) the harsher forms of authority have diminished for most working people in recent decades, which would bring about declining differences in class cultures. A comparison of historical differences would thus not merely add particularistic description, but have value for testing and extending the theory; its scope would be further bolstered by bringing in even more extreme instances, which we can find most easily by going back to another historical epoch in which extremely harsh and violent coercive controls were common. Analytical theory must be distinguished from empirical generalizations about trends over time; only the former can give us understanding of future social patterns, whether or not there is a reversal of empirical trends in the independent variables.

Another reason why we fail to cumulate is that we place too much emphasis on the most recent advances in statistical techniques, and insufficient emphasis on the connections of results across different methods. As Stinchcombe (1984, pp. 55–56) pointed out under the heading “Why are Generalizations forgotten?”: “the biggest correlation coefficients in a body of empirical materials will ordinarily give rise to the biggest path coefficients. . . . Similarly, in almost all cases, the metric coefficients in a regression equation will be biggest relative to variations in causal forces when the path coefficient is largest. . . . And, in turn, the big relations in loglinear analysis are almost always the same ones that have big metric regression coefficients . . . . What this means is that almost all of our causal knowledge from the time we were using correlation coefficients (or even eyeballing crosstabulations) is still our causal knowledge throughout the transitions to path analysis, to structural equations with metric coefficients, to
loglinear analysis.” There are of course advances, in the sense that log linear analysis or latent variable measurement models enable us to see certain difficult patterns, to model certain complexities and take account of particular possibilities of measurement error. But these are developments, so to speak, on the edges of theoretically substantive paradigms, not at their core. We should be building on the earlier results rather than dismissing them because the research lacked all of today’s technical refinements.

As current methodological sophistication (e.g., Campbell 1983; Lieberson 1985; Willer 1987) readily acknowledges, there is no automatic “methodological fix”; explanatory models are always underdetermined by any particular set of data, and theoretical considerations always enter into our assumptions as to which models can be plausibly excluded. If empirical patterns are robust enough—that is, if our theories are on the right track—we will find them with a variety of methods. If the theories have a weak grasp of the central processes, then what we end up with after great statistical refinement is likely to be at best a shadow.

All research methods have their weaknesses. We can overcome this problem by showing a coherent pattern among results found with different methods. Thus, although there are weaknesses in cross-sectional research (Lieberson 1985), the coherence of theory based on all sources of evidence can be used to validate our inferences as to what factors are operative in a given body of data. The existence of experimental evidence helps enormously, when tied to a general theoretical scheme; so do theoretical connections to naturalistic studies (e.g., of organizations or of face-to-face interaction), to historical studies of dynamics, and indeed to the entire range of sociological research. Coherence, if it is sufficiently strong, can bail us out of any local methodological jam. And this is a reason for working to theorize our findings everywhere. From a methodologist’s angle, theory might be regarded as a device for storing our understandings of this coherence. Hence, ignoring earlier research is a vice and not a virtue; the very differences in research methods give us an opportunity to validate our findings by “triangulation.”

Sociology is engaged in many different enterprises. The extreme diversity of what we do tends to keep what we already know hidden under a cloud of dust. Our problem, in a sense, is too much knowledge, especially since so much of it is on the practical/descriptive side, which becomes unwieldy if it is not boiled down into theoretical generalizations. We suffer from the limitations of our cognitive capacity, and this cognitive overload reinforces the structurally based tendencies in the intellectual world to simplify one’s sociological worldview down to one’s own research specialty, theoretical camp, or political faction, and to focus solely on the latest research data or technique.

CONCLUSION

My final point, perhaps the most important, is about the mood of sociology. It concerns our social relationships, our attitudes toward one another within our intellectual field. Much of what we express today about each other’s work is negativistic, hostile, dismissive. This factionalism is debilitating because we need multiple approaches in order to cross-validate our findings.

For sociology to make progress, we need some spirit of generosity, instead of a spirit of factional antagonism. This is not the same as a policy of “go your own way,” tolerating each other but having nothing to do with one another intellectually. Building sociological knowledge is a collective enterprise, and in more ways than one. All human activities are social, and science itself is a process of organizing thought collectives. As in other human activities, conflict is inherent within the organization of the intellectual world. This is not bad, since conflict is a main source of intellectual dynamics, including the processes by which we put forward new theories and collectively decide which ones lead to the best results. But conflict need not be extreme. In no form of intellectual life do we depend more heavily upon each other than in a science. To come together as scientists, we need to concentrate on the coherence of theoretical conceptions across different pieces of research. The personal aspect of that intellectual structure is generosity and good will, a positive feeling towards each other’s best contributions as we grope our way forward together.

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