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Theory for the Moral Sciences

Crisis of Identity and Purpose

THOMAS A. SCHWANDT

Current discourse on social science methodology reveals conceptual confusion and misunderstanding surrounding what theory is, and how it is and should be manifest in qualitative or, more broadly, interpretive studies.¹ Listening to students and colleagues talk about theory brings to mind a story about St. Augustine. When he was asked, "What is time?" he said: "If no one asks me, I know what it is. If I wish to explain it to him who asks me, I do not know." The situation surrounding our understanding of theory may not be quite so enigmatic, but it is worrisome; not so much because we cannot get on with our work as inquirers without a clear understanding of theory, but because we would like a shared working vocabulary of key notions that shape the nature and purpose of our practice as interpretive social inquirers. Our confusion about theory is characteristic of the spirit of our times that is marked by a variety of epistemological, political, and moral critiques of the methods and aims of the natural sciences that have long served as the model for the rationalization of the social sciences. Within this broad critique of the naturalistic interpretation of the social sciences, epistemological concepts like knowledge, truth, and theory that lie at the very heart of this model are being scrutinized and redescribed.

In this chapter, I sort through some of the intellectual activity surrounding our efforts to define the nature and meaning of the concept called "theory." My purpose is to clear the ground in such a way that we can grasp what I believe fundamentally is at issue in arguments about the role of theory in interpretive studies. This discussion begins with a review of several relatively uncontested claims as well as some misunderstandings about theory, partially to make it clear what the current controversy surrounding theory is *not* about. I then take up an examination of the

thesis that debates about theory are proxies for debates about the nature and purpose of social scientific inquiry. Theory is a standard-bearer in the dispute over the definition of what it means to inquire into human meaning and activity.

ON MISUNDERSTANDINGS AND RELATIVELY UNCONTESTED TERRAIN

Given the current tendency to thoroughly deconstruct just about any claim to know, I realize I am inviting criticism by claiming that there are some *relatively* uncontested senses of the term *theory* and some common misunderstandings as well. Nonetheless, I will take that risk in order to try to clear away the underbrush, so to speak, so that we might get to the theory thicket that is the source of our discomfort. I make no subsequent claim to relieve that discomfort; my objective here is only to identify its locus. A broad examination of writing about theory in philosophy of science and social science would reveal that there is general agreement among theorists, methodologists, philosophers, researchers, and the like on the meaning of several senses of the term *theory*. To be sure, there are debates that swirl around these ideas and particularly their implications, but for the most part these debates do not seem to be at the heart of the matter for our understanding of theory in interpretive studies. With this caveat in mind, I offer the following as generally accepted understandings about theory:

1. Observation statements presuppose theory.
2. Atheoretical research is impossible.
3. Theory in the social sciences and the humanities is currently being rehabilitated in this era of postpositivism and the interpretive turn.
4. Theory in the social sciences is multivalent, that is, it has various meanings and there are different ways of theorizing about human affairs.
5. Theory plays a role in both fieldwork and deskwork in social science investigations.

Observation Statements Presuppose Theory

The claim that observation statements presuppose theory has been so thoroughly discussed in the philosophy of science literature (e.g., Garrison, 1986; Hesse, 1980; Phillips, 1985; Suppe, 1977) that it requires little further elaboration here. Only the most naive inductivist still clings to the notions that scientific investigations start with observations and that observation statements form the unimpeachable bedrock of scientific theory.

A. F. Chalmers (1982) provides us with a neat little illustration of how observation statements of all kinds must be made in the language of some theory.

Consider the simple sentence in commonsense language, "Look out, the wind is blowing the baby's pram over the cliff edge!" Much low-level theory is presupposed here. It is implied that there is such a thing as wind, which has the property of being able to cause the motion of objects such as prams, which stand in its path. The sense of urgency conveyed by the "Look out" indicates the expectation that the pram, complete with baby, will fall over the cliff and perhaps be dashed on the rocks beneath, and it is further assumed that this will be deleterious for the baby. Again, when an early riser in urgent need of coffee complains, "The gas won't light," it is assumed that there are substances in the world that can be grouped under the concept "gas," and that some of them, at least, ignite. . . . When we move towards statements of the kind occurring in science, the theoretical presuppositions become less commonplace and more obvious. That there is considerable theory presupposed by the assertion, "the electron beam was repelled by the North Pole of the magnet," or by a psychiatrist's talk of the withdrawal symptoms of a patient, [or by a teacher's talk of a student's inability to read, for that matter], should not need much arguing. (pp. 28-29)

Of course, the collapse of a strict empiricism or of an absolute theory-observation distinction does not mean that observation no longer plays an important role in scientific investigations. Just exactly what role it does play and how we should interpret the theory-dependence of observations and the consequent undetermination of theories by facts or evidence are still debated. For example, there is controversy over whether separating the context of discovery from the context of verification solves the problem of theory-dependence of observation; over whether falsificationism can be sustained in light of the fallibility of observation statements, and over whether it is reasonable to accept some observation statements as true. But these matters need not concern us here, for the primary consequence of our understanding that observations are theory dependent—namely, the abandonment of a strict foundationalist epistemology (the idea that theory can and must be absolutely justified by an appeal to facts, observations, or experience)—is generally acknowledged, regardless of one's particular nonpositivist epistemological or metaphysical preference.

Atheoretical Research Is Impossible

Likewise, it is generally accepted as both a consequence and an extension of the dissolution of the absolute theory-fact distinction that atheoretical investigation is impossible. A simplistic Baconian view (something like,

first gather the facts through what we see, hear, touch, etc., and then form knowledge claims by induction) simply does not adequately describe the activity of scientific inquiry. We know that inquiry does not start with pure observation because some kind of theory precedes the collection of observations. (We also know that observations do not provide a secure foundation for theory because all observations are fallible.) In a paper exploring this claim, James Garrison (1988) uses the *Meno* paradox to illustrate the point here: The paradox says that all inquiry is impossible because either we know what we seek, in which case why search for it, or we have no idea what we seek, in which case how would we recognize it? The way out of this paradox is to recognize that we have partial foreknowledge of the phenomena we inquire into. Prior conceptual structure (theory and method) provides the foreknowledge necessary to initiate and guide the observations we make as inquirers. Garrison further argues that those who claim to do atheoretical inquiry actually do one or more of the following:

- 1) They hold their theories tacitly, in which case they need to reflect upon them and state them explicitly; 2) They hold them explicitly but deliberately withhold them from public view; 3) They pack structural concepts that properly belong to theory into their methodology where they are hidden from their view as well as ours. (p. 24)²

Two misunderstandings are evident in the discourse on interpretive inquiry when these generally accepted ideas about theory are ignored. The first, naive naturalism, is a variant on naive inductivism. Naturalism is based on the presupposition of fidelity to phenomenon. That is, it assumes that the inquirer must remain faithful to the object under study (the social world must be understood as it is in its "natural" state) by not imposing some prior commitment to scientific method. As Martyn Hammersley and Paul Atkinson (1983) have pointed out, this doctrine has served as a much-welcomed antidote to the central features of logical positivism; however, it can be wrongly taken to mean something very much like observation and description of the way things really are, free of any prior conceptual scheme or theory. A little reflection reveals why this is not possible: The doctrine of naturalism (as manifest in symbolic interactionism, phenomenology, ethnomethodology, naturalistic inquiry, and the like) is itself a cognitive theoretical frame that orients the inquiry. Further, it is impossible to observe and describe what goes on in "natural" settings without some theory of what is relevant to observe, how what is observed is to be named, and so on. This is readily evident when we compare, say, the efforts of materialists and idealists to understand and

portray the same "culture." Furthermore, even seemingly simple or mundane typological schemes for organizing and classifying data, for example, as advocated by Robert Bogdan and Sari Biklen (1982), are theoretically informed.

A second similar misunderstanding surrounds the practice of grounded theory. Again, the fallacy seems to be that the inquirer enters the inquiry *tabula rasa* and collects data, and then theory actually emerges inductively from the data. In fact, grounded theory is a complex process of both induction and deduction, guided by prior theoretical commitments and conceptual schemes. In this means of analysis, as well as in any other attempt to move from fieldnotes to concepts and interpretations, the task is far from purely inductive and inferential (Van Maanen, 1988; Woods, 1985). To be sure, the grounded theory approach emphasizes fidelity to the phenomenon under study by arguing against grand or speculative theories; however, grounded theory is not simply a methodological scheme for initiating and guiding inquiry; it requires prior theoretical understandings as well, something very much like what Northrop Frye (1963) called an "educated imagination." Consider, for example, Anselm Strauss's (1987) emphasis on the importance of a "coding paradigm" in grounded theory. This paradigm helps the inquirer to go beyond simple naming (itself requiring a theory) to construct conceptually dense codes that identify the conditions, interactions among actors, strategies and tactics, and consequences associated with what is named. To engage in such coding activity the inquirer must have what Strauss calls "theoretical sensitivity" that facilitates the identification and interrelation of these conditions. Or consider the central notion of theoretical sampling in grounded theory: One decides what to sample on the basis of its contribution to the evolving analytical scheme. This scheme arises from prior knowledge of potentially relevant concepts, ideas, and other schemes continually tested for adequacy against the data at hand.

Substantive Theory Is Being Rehabilitated

Empiricists and logical positivists argued that if we sharply distinguished specific, concrete, empirical observations from general, abstract statements, then we could successfully purge metaphysical elements from science and get on with the business of describing and explaining the way things really are.³ They assigned epistemological priority to observation statements, leaving theoretical statements (concepts) entirely dependent for their meaning on the particular observation statements into which they could be unpacked. This is represented in Herbert Feigl's (1970) visual image of theoretical concepts growing out of the "soil" of observa-

tion: Theory is meaningful *precisely because* of "an 'upward seepage' of meaning from the observation terms to the theoretical concepts" (p. 7). In effect, these thinkers elevated the importance of fact or empirical evidence in our construction, judgment, and accumulation of knowledge claims over the relevance of theoretical concerns. Certainly, general theoretical commitments of the philosophical or metaphysical kind were regarded as unimportant in the development of scientific knowledge.

The criticisms of this strict empiricist or logical positivist stance made by Kuhn, Polanyi, Feyerabend, Lakatos, and others in effect challenged this relative priority of observation statements over theoretical statements. Their efforts can be interpreted as attempts to restore theoretical considerations to an equal footing with observation statements in our picture of the development of scientific knowledge. Much of postpositivistic philosophy of science is concerned with defining and explicating the role that theoretical commitments, conceptual schemes, beliefs, assumptions, and the like play in our conduct and understanding of scientific inquiry.

There is now general agreement that the formation, testing, and success of scientific theories is not solely an empirical matter. Of course, what kind of role such alleged extra-scientific considerations do and should play is debated, but the central question is not *whether* there is a place for these concerns but *how* they operate. In this sense, we can say that post-positivism is concerned with rehabilitating the theoretical by examining how substantive or, more accurately, interpretive theoretical commitments (i.e., behaviorism, symbolic interactionism, materialism, critical theory, feminist theory, structuralism, poststructuralism, etc.) and values shape, organize, and give meaning to our observations about people and society (see, for example, Alexander, 1987; Gouldner, 1970; Hesse, 1980).

This restoration of a focus on theoretical commitments is misunderstood if it is taken to refer to ongoing efforts in the social sciences to improve, refine, and criticize *methodological* theories or theories of the instrument that help us acquire or analyze data (e.g., generalizability theory, item response theory, grounded theory, narrative interview theory, aspects of fieldwork concerned with theories of trust or social relations). Despite contemporary fascination with the recovery of interest in fieldwork methodologies and the philosophy of *verstehen*, the hallmark of social science today is *not* debates over methodological approaches. Debates over theories of method are endemic to the social scientific disciplines and date at least from the writings of Vico and Herder in the eighteenth century (see Berlin, 1976). What sets contemporary debates in social science apart are not methods debates but debates about the very substance of social science—large-scale disagreements about the nature, meaning,

and purpose of human activity, including the activity of human inquiry. I will have more to say about this claim in the second section of this chapter.

Theory Is Multivalent

It is generally recognized that there are different approaches to building theory and different theories, all of which are in some way useful in making sense of social phenomena. Of course, there is debate over which approaches are more useful, but little debate over multivalency as a general condition. Space does not permit a full explication of this claim here, but it can be sketched. For example, Jeffrey Alexander (1987) argues that there are good cognitive and evaluative reasons why the social sciences will always be characterized by multiple theoretical orientations and will never achieve the degree of consensus about empirical referents or explanatory schemes characteristic of the natural sciences.⁴ He notes that social science has a distinctive evaluative nature and invites us to consider, for example, how ideological implications accrue to the very descriptions of these social objects: calling a society "capitalist" or "industrial," claiming that there has been "individuation" or "atomization." Alexander cites Mannheim on this point: "Every definition depends necessarily on one's perspective, i.e., it contains within itself the whole system of thought representing the position of the thinker in question especially the political evaluations which lie behind this system of thought" (p. 21).

Alexander claims that given the difficulty of establishing consensus on even the most basic of empirical referents, consensus on more abstract concepts forming the bases of substantive social theory is even less likely. The lack of agreement on empirical referents and concepts means that "the full range of non-empirical inputs to empirical perception become objects of debate" (p. 21). Because there is such endemic disagreement, moreover, social science will "invariably be differentiated by traditions and schools" that become the "bases upon which scientific disagreements are promoted and sustained" (p. 21).

Theory Plays a Role in Fieldwork and Deskwork

Theory plays a role both in orienting the field-based inquirer to the object of study and in writing about what one has researched. A moment's reflection on, for example, the existentialist posture of fieldworkers like Peter and Patricia Adler or Jack Douglas, the commitment of Paul Rabinow to phenomenology, or Shirley Brice Heath's sociolinguistic conceptual scheme reveals that one enters the field with a theoretical language and

attitude. That substantive theory is essential for making meaning out of or interpreting the data one has gathered needs little further explanation. There are, of course, disagreements, as John Van Maanen (1988) points out, on whether theory is best employed and emphasized in fieldwork or deskwork. He notes that confessionalist ethnographers unlike, say, realist ethnographers seem to find theory most relevant during the writing phase: "Theory doesn't determine the fieldwork experience, but it may provide the dictionary with which it is read" (pp. 97-98). Yet, these kinds of disagreements do not obviate the claim that theory plays some kind of role in both aspects of interpretive work.

We could spend our time wrestling with the nuances and implications of the general understandings or claims about theory that have been sketched above. We could also spend our time examining the difference between a theory and a model; exploring whether a minimalist view of theory—one that does not emphasize nomological networks and systems of postulates—qualifies as theory; investigating whether theory development or testing is more important in interpretive work, and so forth. But I do not believe that such exertion will necessarily pay off in a greater awareness of what is fundamentally astir in the controversy surrounding theory in interpretive work. The claims presented above are really not much more than background to a more profound issue, namely, is scientific theorizing, as typically defined in empiricist social science, an adequate purpose and frame of reference for human inquiry? The remainder of this chapter is a long answer to this question. I begin with an explanation of the activity of scientific theorizing as a total intellectual orientation shaping our received view of the goal and practice of social inquiry. That view is then contrasted with a different intellectual orientation. The chapter then concludes with a discussion of the implications of this new orientation for our thinking about theory.

THEORY AS A TOTAL INTELLECTUAL ORIENTATION AND AN ALTERNATIVE

When, for example, the ethnographer Martyn Hammersley (1990) argues that "sociologists have a collective duty to attempt the production of well-established theory" (p. 109) (understood as a set of conditionally universal causal claims stated in deductive form), what is at stake? Hammersley admits that not all social research should be preoccupied with this goal, but he believes that striving for this kind of theory "gives us the best hope of producing effective explanations for social phenomena and thereby a

sound basis for policy" (p. 108) and is essential to the cumulative development of knowledge. On one reading, we might say that Hammersley is seeking to get beyond the internecine quarrels over whether ethnography can properly include cultural description and theory development only by making a case for including theory verification and testing. This may be true enough, but I suggest that more is at issue. In linking the "collective duty" of social inquirers to the activity of developing theory and in claiming that this activity offers the "best hope" for generating useful explanations of social phenomena, Hammersley points to theory as the most appropriate intellectual orientation for the practice of social inquiry, and thereby introduces us to the issue that lies at the heart of our current discomfort over theory in interpretive inquiry.

In this view, the development of causal, explanatory theory is not simply some critical element in the armamentarium of the social science methodologist or researcher; it is the *raison d'être* for the practice of social inquiry. In other words, to talk of the importance of empirical theory is not to talk simply of some feature of scientific investigations, but to talk of a pervasive and dominant intellectual orientation, purpose, or goal of social and political inquiry. Richard J. Bernstein (1976) explains this thesis as follows:

At the core of the naturalistic understanding of social and political inquiry is the demand for empirical explanatory theories of human behavior. When this idea of empirical theory is fully articulated, it requires that we discover basic invariants, structures, or laws that can serve as a foundation for theoretical explanations—explanations that will take a deductive form, and from which we can derive counterfactual claims about the relationships between independent and dependent variables. It has been projected that the social sciences, as they mature, will discover well-tested bodies of empirical theory which will eventually coalesce in ever more adequate and comprehensive theories. (p. 227)

The defense of this view extends from Comte through Merton to Homans and Turner in the social sciences and from Feigl and Hempel to Popper and Newton-Smith in philosophy of science. Its lineage and development need not be recounted here. What is important for present purposes is to recognize two points. First, by this definition the development of social science theory requires a disinterested scientist; hence, theory clearly excludes any normative considerations, that is, it is empirical and descriptive, not prescriptive (Bernstein, 1976). Second, an essentialist argument is implicit in this insistence on a unified model of the sciences, with a formal, deductive paradigm for developing and testing theory at its core. In other words, the inherent nature of the practice of social

inquiry—its very essence, meaning, and rationale—is thought to reside in the naturalistic interpretation. To engage in social inquiry is to pursue this kind of empirical theory. Likewise, the essence of social scientific knowledge is empirical explanations. The activity of constructing and testing empirical theory is the very heart of the social scientific enterprise.

Because the notion of a solely empirical explanatory theory signals the essence of what it means to engage in social inquiry, we can view theory as the standard-bearer in debates about the purpose of social inquiry. Thus, for example, when we question the importance of developing and testing empirical theory, we are questioning not only the rationality of the social sciences, but their very essence. We are questioning whether it is our “collective duty” to organize our social practice of inquiry so as to achieve the goal of formulating empirical theory, and we are questioning whether empirical theory offers us the “best hope” of progress toward the realization of ideals and social goals.⁵ Hence, our discontent with theory is an identity crisis of sorts, a struggle with what it means to be a social scientist and to engage in social investigation.

The pursuit of empirical theory by a disinterested inquirer is what marks the practice of the social scientist from that of the humanist. Merton, for example, claimed that although social scientists are pressured to straddle scientific and humanistic orientations, they should resist such pressures and act “wholly the scientific role,” for failure to do so denies the empirical, systematic, and cumulative nature of scientific theorizing (quoted in Alexander, 1987, p. 13). That image of the collective duty of social inquirers to act as scientists is what is currently being redescribed. Paul Rabinow and William Sullivan (1987) observe, for example, that

The time seems ripe, even overdue, to announce that there is not going to be an age of paradigm in the social sciences. We contend that the failure to achieve paradigm takeoff is not merely the result of methodological immaturity, but reflects something fundamental about the human world. *If we are correct, the crisis of social science concerns the nature of social investigation itself.* The conception of the social sciences as somehow necessarily destined to follow the path of the modern investigation of nature is at the root of the crisis. (p. 5; emphasis added)

At the risk of being redundant, “the nature of social investigation itself” is the idea of discovering and testing empirical explanatory theory by adopting a disinterested scientific attitude toward the world. It is this nature, this essence of social inquiry that gives rise to our discomfort with theory. And it is this defining feature or essence that is currently being re-examined. Clifford Geertz (1980), for example, referred to this refigu-

ration of the nature of social inquiry as the blurring of genres, a radical alteration in the sociological imagination evidenced by a turning away from dreams of social physics to the imagery, method, theory, and style of the humanities. What we are wrestling with then is defining the boundaries of a different intellectual orientation for our practice as social inquirers.

This new intellectual orientation is emerging and not yet clearly understood. Its outlines are less apparent in quarrels over methodological paradigms and more evident in discussions of the inquirer’s role; the inquirer’s obligations to self, to society, and to the people he or she studies; and the status or place of social inquiry among other efforts to understand ourselves. The call of Robert Bellah and colleagues (1985) for social science as public philosophy; Norman Birnbaum’s (1988) plea that social science develop *sagesse*—a kind of wisdom stemming from reflectiveness that is inseparable from purposive moral action; and Richard Rorty’s (1989) argument that solidarity not objectivity should serve as the regulative ideal for our activity as inquirers all point to new ways of conceiving of the meaning and goal of social inquiry. The remainder of this chapter explores some themes that characterize the effort to redefine the intellectual orientation of social inquiry.

A Focus on an Ethical Model of Reasoning

Scientific reasoning requires bringing particulars under universal rules. Scientists deductively demonstrate necessary truths from premises that themselves are necessarily true. The familiar form of explanation is

1. Laws and theories	Explanans
2. Initial conditions	
<hr/>	
3. Event to be predicted or explained	Explanandum

Aristotle distinguished scientific reasoning from ethical reasoning. He maintained that because of the complexity of human behavior there are no first principles (laws and theories) on which to construct a science of ethics. Hence, ethical reasoning requires deliberation—sizing up a situation and weighing information—and making decisions on a case-by-case basis.

What guides our ethical deliberation is the virtue of practical wisdom