

INTERPRETATION AND SOCIAL KNOWLEDGE

On the use of THEORY IN THE HUMAN SCIENCES

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Introduction

I

Our understanding of social knowledge is due for a massive transformation. A generation's worth of arguments about postmodernism and science, relativism and objectivism, have obscured our view; a consistent connection has been drawn between the study of discourse and the adoption of philosophical skepticism and ironic distance; a repeated disavowal of the possibility of causal explanation has crippled the interpretation of cultures; an overwhelming tendency to frame the problem of social knowledge as the problem of how social science can be like natural science has eliminated essential questions from our minds. The space of epistemological argument in social theory, then, comes to be defined by "posts" and their opponents: postpositivism, poststructuralism, postmodernism, postcolonialism. Regardless of what the "posts" actually are or are not, thinking about social knowledge is, in this Manichean account, a dispute about whether science (and perhaps modernity) is good or bad.

For the researcher intent on actually producing some sort of social knowledge, the disputations of social theory and philosophy are often experienced less directly, sublimated into distorted communiqués

about methodology: qualitative against quantitative; unscheduled interviews against scheduled interviews; participant observation against survey research; in a phrase, depth against generality. And so, metatheoretical arguments about the construction and justification of social knowledge reappear as debates about lived social realities, and the research design that is necessary to grasp them.

The debates about method are, without a doubt, real: they denote significant divergences in the practice of gathering and colligating evidence in social research. But in their connotations, they encode the same paralyzing epistemological dilemma. From one point of view, there are clearheaded and rigorous producers of quantifiable, verifiable truths about social phenomena, and those who, for whatever reason, prefer to impose their own interpretive flights of fancy on idiosyncratic data gathered in idiosyncratic ways. From the opposite point of view, “objectivists” or “positivists” do not work for their objectivity but rather perform it. The ongoing scientism of large sectors of the human sciences is, in this latter view, the bane of a true understanding of social life, which can only be achieved through the careful humanism of attending to what people actually do, say, think, and tell their interviewer.¹

But perhaps the heavy feeling of despair and *déjà vu* brought on by such dilemmas is not necessary. We can reconsider. So let us begin by asking: are all of our problems, as researchers and as theorists, understandable in terms of differences in method, practically conceived as that which we do to establish factual statements, broad or narrow,

1. The term “human sciences” has a historical referent in the intellectual project of the *Geisteswissenschaften*, literally “sciences of the spirit,” and thus the nineteenth-century German philosophies of history that formed an important part of the intellectual context for Max Weber’s definition of sociology. But it is also intended to evoke more recent arguments about the (sometimes nefarious) effect of disciplinary self-conceptions on research into how human beings live together in social groups. In particular, the sociologist Mayer Zald has developed an incisive analysis and critique of the ways in which the social sciences generally, and sociology in particular, can benefit from recognizing their historical-intellectual debt to both the humanities and the natural sciences, and from correcting their overemphasis on the latter as a model for the construction of knowledge. His open-ended paper invites, in my view, arguments of the kind found in this book. See Mayer Zald, “Sociology as a Discipline: Quasi-Science and Quasi-Humanities,” *American Sociologist* 22, nos. 3–4 (1991). For a historical account of the conditions that led to the formation of the social scientific disciplines as we now know them in the United States, and an argument that this organization of intellectual labor is no longer appropriate, see Immanuel Wallerstein et al., *Open the Social Sciences: Report of the Gulbenkian Commission on the Restructuring of the Social Sciences* (Stanford: Stanford University Press, 1996).

about social life? Or do disputes over method, when considered carefully, reveal other problems, problems not reducible to measurement and technique, problems that point to deep divides in our fundamental conception of how a community of inquiry can expect to know something about other human communities and their various dynamics?² I am continually left to wonder whether the world of social researchers is really divisible into those who believe in the existence of social facts in some quantitative sense and those who do not, or whether this divide has lost even its heuristic utility. Is it at the level of facts, in other words, that the great controversies over the nature and purpose of research in the human sciences should be fought out? In one sense, yes: before doing anything, we have to find a way to “establish the phenomenon.”³ But in another sense, no: debates about method often carry implicit disagreements about the nature and purpose of inquiry, the structure of social life itself, and the role of the critical intellectual or social researcher in comprehending it. If we render these disagreements explicit, we find that they are not only about method, strictly understood, but also about how knowledge claims are built out of conceptual innovation, justified in publication, and criticized as inaccurate and untrue (or, to use that infuriatingly ambiguous word, “problematic”).

We have disagreements, that is, not only about how we establish the sheer existence of this or that social phenomenon, but also about how we can claim to correctly and effectively explain, criticize, or interpret it. In my view it is these latter disagreements, rather than disagreements about whether or not there are, in some sense, social facts, that are at the core of controversies about social knowledge. And so, it is to these disagreements that we must address ourselves if we are to move beyond the world of posts.

2. Sandra Harding has usefully distinguished between method (“a technique for (or way of proceeding in) gathering evidence”), methodology (“a theory and analysis of how research does or should proceed”), and epistemology (“a theory of knowledge”). The analysis of this book could be said to address the latter two, if we consider that Harding’s definition of methodology includes how theory is used and applied in specific disciplines and fields. But there is another way of framing this, which is to say that evidentiary method must be accompanied by a conceptual method, whereby evidence is ordered and comprehended as part of a larger knowledge claim. Sandra Harding, “Is There a Feminist Method?” in *Feminism and Methodology: Social Science Issues*, ed. Sandra G. Harding (Bloomington: Indiana University Press, 1987), 1–14.

3. Robert K. Merton, “Three Fragments from a Sociologist’s Notebooks: Establishing the Phenomenon, Specified Ignorance and Strategic Research Materials,” *Annual Review of Sociology* 13 (1987).

II

In the discourses in and around contemporary social theory, post-positivism has a somewhat clear historical meaning—the term refers to the break with certain taken-for-granted assumptions about the unity of the natural and social sciences that, like so many other breaks, happened sometime in “the sixties.”⁴ Given the academic penchant for overdramatizing our own intellectual disputations, it should not surprise us that this break is narrated in vaguely heroic terms, especially in casual academic conversation. “Positivist” is used pejoratively in certain sectors of the human sciences because the positivist is the antagonist to the heroic protagonist, the postpositivist, who broke his chains and challenged the dragon. In this frame “positivist” refers to someone whose addiction to reductive quantification causes him to miss the real stuff of social life—villains, in scholarship, are always associated with untruth.

But what could positivism mean, in some general and yet practically significant way, if it is not just a pejorative signifier, applied to work that lacks the sacred values of the researcher who casually uses the term? Probably postpositivists mean one of two things when they say “positivist.” First of all, positivism can refer to an underlying philosophical commitment to certain methods for ascertaining social facts—descriptive positivism. Here the core positivist axiom is that the social sciences are rendered scientific by the use of specific methodological techniques for handling data, and in particular by an agreement about the superiority of certain ways of establishing reliability and validity through quantification and statistical inference. In the context of contemporary social research, the most immediate objection to descriptive positivism is in terms of the methods used to produce accurate descriptions of social phenomena. Is quantification and, more specifically, correlational analysis adequate to the task of producing facts about social life? It is a quite common argument, in fact, that social research should be empiricist, but that the methods

4. This break is documented and advocated for, in classic form, in Richard J. Bernstein, *The Restructuring of Social and Political Theory* (New York: Harcourt Brace Jovanovich, 1976). See also Sherry Ortner, “Theory in Anthropology since the ’60s,” *Comparative Studies in Society and History* (1984). For a good analysis of “the sixties” as it has become a construction relevant to intellectuals in the West more generally, see Eleanor Townsley, “‘The Sixties’ Trope,” *Theory, Culture, and Society* 18, no. 6 (2001).

for achieving empirical validity differ significantly from those found in either the natural sciences or the (perhaps misguided) quantitative branches of social science. For example, one could value the descriptive production of truth above all else, but insist that this description must be qualitative and particularistic. Thus the tendency toward empiricism in social research extends far beyond quantitative methodologies taken from the natural sciences.⁵ Positivism, as an approach to method, and empiricism, as a resistance to theory, are by no means coextensive. Empiricism can be “antipositivist.” Furthermore, “positivism” can also refer to a way of building and using theory to guide research.

In particular, the second meaning of positivism refers to how one can use theory to construct explanations. Here we find the philosophical arguments of the logical positivists, the various attempts to put their covering-law model of explanation to work in the human sciences, and the increasingly sophisticated inheritors of the ambition to construct a truly universal science of the social. Explanation, in this view, is the logical result of combining a general law about social life with particular circumstances, thus enabling the investigator to predict or retrodict the resultant outcome. What we want, according to what we could call theoretical positivism, is a social physics, with theory leading the way by positing general laws of social behavior.⁶

Should we have a social physics? Can we have a successful one?

5. John Goldthorpe has written that ethnography, in so far as it is not “irrationalist,” shares many of the epistemological assumptions of survey research. He then argues for a “common logic of inference” across both qualitative and quantitative research in social science, and indeed across the social and natural sciences, in John H. Goldthorpe, “Sociological Ethnography Today: Problems and Prospects,” in *On Sociology, Volume 1: Critique and Program* (Stanford: Stanford University Press, 2007). Goldthorpe’s own vision could hardly be called empiricist, however. Rather, he proposes a compelling synthesis of rational action theory and the quantitative analysis of data, which would fit the second meaning of positivism (“theoretical positivism”) I discuss below. For an analysis, see Isaac Reed, “Review Essay: Social Theory, Post-post-positivism, and the Question of Interpretation,” *International Sociology* 23, no. 5 (2008).

6. Jonathan Turner, drawing on the original vision of August Comte, argues that the goal of social physics “is to uncover abstract laws, and the fewer laws the better,” and thus that the goal of “science is to seek understanding of the universe, and the vehicle through which such understanding is to be achieved is theory. Sociology has allowed poor philosophers to usurp theoretical activity and ‘statistical packages’ to hold social science hostage.” (Jonathan Turner, “Returning to ‘Social Physics’: Illustrations from the Work of George Herbert Mead,” *Current Perspectives in Social Theory* 2 [1981]: 188, 187). For Turner’s classic defense of theoretical positivism in sociology, see Jonathan Turner, “In Defense of Positivism,” *Sociological Theory* 3, no. 2 (1985).

Successful or not, we have it: there are, in the contemporary social sciences, extended and well-articulated attempts to develop a hyper-generalized approach to social explanation. In theoretical positivism, then, we find highly general hypotheses and propositions that are to be verified or falsified through empirical testing, and, simultaneously, a strong argument that social research must find a way to move beyond merely producing correlations and instead produce theory-informed explanations. Theoretical positivism, in other words, has deep suspicions about descriptive positivism. Perhaps the goal for theoretical positivism is a set of social sciences, all of which look like neoclassical economics, in which the basic assumptions are agreed upon, and metatheoretical debate about human nature, social structure, and the problems inherent in the very possibility of knowing something about either is left to philosophers and the occasional book on microfoundations.

It must be said: if one wants to understand, on its own terms, the philosophical underpinnings of positivism, or logical positivism, or the combination of the quantitative analysis of data with the testing of universal propositions, the resources are ample. The empiricist philosophy of science has developed in radically new directions since Carl Hempel ruled the scene,⁷ and if one wants to grasp the presuppositions and conceptual how-to of strictly “objectivist” quantitative research, myriad books on research design in social science put previous generations’ manuals to shame. And yet, simultaneously, such research has been subjected to ongoing, relentless attack for what is seen as its tendency to distort and reduce human social life—to sacrifice understanding for a form of mathematical precision that is, in the end, illusory. These arguments are, by now, exceedingly well known.

Thus this book is not another attack on positivism, either in the substance of its text or in the goals that it outlines for how we use theory in research. Rather, it is a reconsideration of two fundamental

7. Consider how Wesley Salmon’s work extending the analytic tradition in the philosophy of science has moved beyond the logical positivists’ initial aversion to theorizing causality outside Humean confines, and thus brought theories of causality and (neopositivist) theories of scientific explanation together. Indeed, the entire conversation between Salmon and his critics reveals how the analytic philosophy of science has had its own positivist/postpositivist conversation. See Wesley Salmon, *Scientific Explanation and the Causal Structure of the World* (Princeton: Princeton University Press, 1984), and Wesley Salmon, *Causality and Explanation* (New York: Oxford University Press, 1998).

and interrelated questions: how do, and how should, theory and evidence interact? For there is much objection, in postpositivism, to the positivist answer to these questions (in short, that theory generates hypotheses and evidences tests them), but little agreement as to how this intersection should happen outside the positivist mold.⁸ What is this so-called dialogue between theory and evidence? How are we to construct and use theory if we are not formulating hypotheses? This is the curiosity that led to this book: I want to know how nonpositivist research works, and to build a constructive theory of knowledge claims made by nonpositivists. To do this, I have constructed a text that works at the intersection of practice and prescription: in-depth textual analysis of actual truth claims in social research is combined with epistemological reflection.

I call the different ways of bringing theory and evidence together “epistemic modes.” The term is intended to indicate the relative autonomy of these modes from substantive commitments to theoretical programs, research agendas, or specific methods. Epistemic modes dictate the *conceptual* method by which theory is brought into contact with evidence, structure the expectations about what such contact can accomplish, and provide more or less well-formed criteria of validity that are used to evaluate the knowledge that is thereby produced. Epistemic modes can also be articulated in abstract argumentation about the possibilities and limits of social knowledge, and can be interrogated about their premises. But they do not map neatly onto substantive research programs, or onto any particular intellectual history of social theory or any particular account of paradigms of social research.⁹

Thus, while the epistemic modes I examine—realist, normative, and interpretive—have elective affinities with great thinkers and grand traditions of social analysis (affinities some readers will immediately recognize, and which I discuss throughout the text and especially

8. Isaac Ariail Reed, “Epistemology Contextualized: Social-Scientific Knowledge in a Postpositivist Era,” *Sociological Theory* 28, no. 1 (2010).

9. There is one work, however, whose mapping of social thought and social research seems particularly relevant to this text. That is John R. Hall, *Cultures of Inquiry: From Epistemology to Discourse in Sociobistorical Research* (Cambridge: Cambridge University Press, 1999). Certainly this book is written in a similar interpretive spirit to Hall’s project and shares some of its arguments, as will become clear. I argue herein, however, for a return to epistemology. I do not see this as incommensurate with Hall’s map per se, but rather as a theoretical project for which Hall’s map of sociohistorical investigation reveals the need, or calls forth.

in the footnotes), their truth and utility as terms for understanding what is going on when a researcher constructs a knowledge claim stand or fall with what makes up the core of each chapter in the book: the analysis of, and reflection upon, actual truth claims made in the human sciences. Here I briefly introduce the three epistemic modes that I examine.

III

In the realist epistemic mode (chapter 2), theory points to the fundamental forces and relations of social life that lie beneath the surface phenomena that we observe, narrate, experience, and/or measure. Thus when the investigator uses theory, what she does is connect an underlying social reality to the social phenomena or outcomes that she desires to explain. The entities referred to by theory give order to her data and a causal force to her conclusions. Revolutions are explained by the fundamental processes that bring them about; conversations and interactions, in which identities are established and dissolved, are explained by the underlying tendencies of human cognition; moves in a marriage market are explained in terms of the ways in which individuals maximize utility; the emergence of a cultural phenomenon, such as *Star Wars*, is explained by the way in which it hews to the fundamental binaries that structure the human mind.

Realism reignites the possibility that social science, post-Kuhn, is still an embodiment of a generalized scientific rationality. That rationality, however, is defined and guaranteed by the way in which social theory directly references social reality, revealing the social forces that structure the world. The core conceptual problem of realism, then, is this: how can the efforts of the experimental scientific laboratory be reconfigured and transformed for the science of society and history? The opening text of the contemporary critical realist intellectual movement, Roy Bhaskar's *A Realist Theory of Science*, begins with a philosophical analysis of what must be true about the world if natural scientific experiments are intelligible and rational.¹⁰ Bhaskar's next book, perhaps even more important for that movement, considers how social science can and cannot imitate the logic of natural science that he originally derived from the analysis of experi-

10. Roy Bhaskar, *A Realist Theory of Science* (New York: Verso, 2008), chap. 1, esp. 23–26.

ment.¹¹ The problem, of course, is the way humans bring a certain “concept-dependence” to social reality, which is a nice way of saying that subjectivity messes everything up. Human subjects, moreover, have a rather irritating tendency to aspire to ideals, be motivated by outrageously unrealistic ideas, and pass judgment on each other. This issue—that human social life is full not only of concepts but of judgments—is what brings the argument of this book to the second epistemic mode I consider.

In the normative epistemic mode (chapter 3), theory is what enables research to be a dialogue between investigator and investigated—between an ethnographer and her subjects, between a historian and the lives he recreates, between a critic and the text she reconsiders and reframes. Theory brings to bear the critical force of well-articulated utopia upon the empirical world, but in doing so, it brings together the intellectual’s political philosophies with the utopian capacities immanent in social life as lived and experienced. In normative social knowledge, then, two modes of consciousness come to inform each other, and thus theories of the good get better—while at the same time striving for some sort of practical effect. The possibilities for resisting a hegemonic cultural formation are revealed; the nascent democratic instincts of volunteers are articulated and made sharper; the deeply pathological implications of a seemingly liberatory set of cultural practices are brought to light. And thus the conversation about the good and the should is expanded.

What normative research does is reconfigure the terrain of normative argument by producing interpretations of actual social life. Ideals for a better social order are salvaged from conversations in coffee shops, solidaristic town hall meetings, anti-imperial campaigns, and so on. Visions of dystopia also anchor normative research, sometimes in the genre of tragic irony: the normative projects of humanist reformers are revealed to be norming projects in the service of a new format of domination (for example). But the core conceptual problem, for normativism, is how this operation of critique, which *is* grounded in actuality and fact, can or cannot be grounded in a more explanatorily powerful use of theory. A town hall meeting identifies a problem, but how can the solution be tested, if there is no laboratory? Normativism, in other words, either points back to realism, or toward a different

11. Roy Bhaskar, *The Possibility of Naturalism: A Philosophical Critique of the Contemporary Human Sciences*, 3rd ed. (New York: Routledge, 1998).

way to ground truth claims in social research. And it is to discerning what this “different way” could be that the latter part of this book is dedicated, in its study of interpretive social knowledge.

In what I call the interpretive epistemic mode (chapter 4), the primary focus of the investigator is on the arrangements of signification and representation, the layers of social meaning, that shape human experience. The investigator reconstructs the meaningful context of social action, and to do so she draws upon a plurality of theoretical abstractions; these different theoretical tools need not—pace realism—add up to a coherent, general, and referential theory of social reality in the abstract. Rather, the investigator combines bits of theory with bits of evidence, and then these theory-fact pairs are brought together into a meaningful whole. This meaningful whole is the deep interpretation that the investigator constructs, and it gives coherence to her case. Cockfights are reinterpreted as a performance of status, masculinity, and what it means to be human; a revolution is analyzed in terms of how the advance guard constructed, and propagated, a worldview; the actions of a whole cohort of politicians or scientists is traced back to a formative experience and the narrative construction of collective identity that emerged from it.

Interpretive social research, I argue, must push beyond the surface reports of actors and the immediate meanings available in the investigator’s evidence to grasp some deeper set of meanings that inhere in the action under study. This is what the use of theory allows the hermeneutically sensitive researcher to do. But even deep interpretations are typically constructed—by friends and foes alike—as *interpretations and not explanations*. Overcoming this problem is the core conceptual difficulty for the interpretive epistemic mode. Consider, very briefly, an example.

An interpretivist sets out study the Salem Witch Trials. In his interpretation, he attempts to grasp the meaningful world of Puritan life and, in particular, how witches and witchcraft played an emotionally charged role in the Puritan social imaginary, encoding a socially powerful metaphysics of God, sex, and patriarchy.¹² What sort of knowledge claim is this interpretation? The point, with Salem at least, is that this metaphysics was *threatened* in 1692 by the possibility of witches meddling with the cosmos, thus turning the

12. Isaac Reed, “Why Salem Made Sense: Gender, Culture, and Puritan Persecution of Witchcraft,” *Cultural Sociology* 1, no. 2 (2007): 209–34.

trials into a countrywide crisis, ending in the murder of nineteen men and women. Who felt threatened by the witches? What did fear motivate those people to do, and what social processes were they able to trigger to get done what they were certain needed to be done? What were the unintended effects of these developments? These are causal, explanatory questions, and they force us to consider whether or not, and if so how, interpretations of social meaning can possess explanatory torque.

In other words, if realism derives from the logic of the lab, and normativism from the logic of the democratic meeting or social movement, interpretivism derives from the logic of reading and disputing different readings of a text,¹³ which raises the question—reading for what? How do symbols enter the social world, and have an effect on social action—an effect, moreover, that the investigator can comprehend and communicate by building an explanation? The idea of interpretive explanation is the subject of chapter 5. In it, I continue my attempt to recast the interpretive way of making social knowledge claims. Meaning-centered research in the human sciences, I argue, is not only theory-intensive (chapter 4) but also constructs causal explanations, albeit in a way that departs from the typical ways of thinking about social causality. By revealing the way social meanings act as *forming*, as opposed to *forcing*, causes, the interpretive epistemic mode can offer a synthetic approach to social knowledge, and enable the researcher to build social explanations and deliver social critique.

IV

These reconstructions of epistemic modes are intended to be an intervention in, and a symptom of, what I view as a new epistemological era for social research—the era after the posts. After debates about culture, interpretation, and the linguistic turn; after innovative

13. The issue is how much reading a text can be fruitfully compared to analyzing social action. Ultimately, in chapters 4 and 5, I move toward an image of the interpretive social researcher as more akin to a theater critic than a reader of texts. However, the whole question of action and text is one of the key debates of the cultural turn, which opens up many of the questions that this book attempts to address. The classic meditation on text and social action is Paul Ricoeur, “The Model of the Text: Meaningful Action Considered as a Text,” *Social Research* 38 (1971). See also Susan Hekman, “Action as a Text: Gadamer’s Hermeneutics and the Social Scientific Analysis of Action,” *Journal for the Theory of Social Behavior* 14, no. 3 (1984).

recastings of standpoint theory and the politics of knowledge; after extended discussions of power/knowledge, interpellation, and subject production; after all this, what is the producer of social knowledge to do? Is it possible to believe a great deal of what is written and said in postpositivist social theory to be true, and yet to proceed with the goal of explaining social action? I think that it is, and this possibility is what I intend my account of social knowledge to render visible.

Yet if this book is a symptom of a specific epistemic moment, and the product of a certain generational experience—of being trained in social theory long after the fiercest battles over “postmodernism” had burned bright—its sympathetic engagements and reconstructions are less aimed at the august history of social theory and philosophy, and more at the inner workings of texts that have, often very successfully, attempted to say something about the world—or at least some small piece of it. For, when social theory is presented in the form of competing schools, when research programs are assessed for their fecundity, or when the basic traditions of philosophical thought about social science are set out, the result is often exiguously honest but repetitive. It is also bewildering. Not only are there a plurality of theoretical positions and research programs, but every such program of necessity involves combinations of methodological tendencies, theoretical presuppositions, and political implications or valences—along with answers to the more explicitly epistemological questions considered here. When one includes, furthermore, the codified empirical claims that often belong (or are seen to belong) to this or that research program, the result is a sprawling catalogue or encyclopedia of what are, ultimately, different subject positions in a complex and dynamic discourse.

Training in social theory involves learning how to map this discourse in useful ways, and to pay close attention to the way in which theoretical discourses encode their own historical conditions of production. Yet these maps, as they become their own extended apparatus of intersecting denotations and connotations, can be exhausting. At a certain point in my own intellectual biography, I came to view this exhaustion about mapping the field of social theory to be connected to a different problem—the problem of the metatheoretical frame within which one makes strong, empirically responsible, theoretically informed knowledge claims in social research itself. For both myself and the historians, anthropologists, sociologists, and scholars of communication I trained with, our metatheoretical frames were much

more precise in their negative aspects. We did not have an integrated Marxist theoretical structure to contain and connect social explanation, political judgment, and the interpretation of cultures. We did not have the calm and settled scientific frame of variables and their various effects, or even the frame of a truly general sociological theory (almost everyone I trained with was, in a rather disturbingly certain way, a “postpositivist”). We did not have the warrant to simply head out into the world (or into the archives) to pursue value-free social research. I interrogated my friends, colleagues, and teachers on the matter. We all knew for sure what we were not. We were not positivists, not Marxists, not “reductionists,” and so on and so forth. But what was the negation of the negation? What were we?

I have come to view this question as essential, and as answerable only in an epistemological register. That register must resist the twin temptations of formal logic, permanently distant from the messiness of social investigation, and the reductive sociology of knowledge, insensitive to how internal and impersonal imperatives, combined with informal argumentation and scholarly communication, can make communities of inquiry more than just clubs or networks.¹⁴ A theory of social knowledge should, instead, be built through critical reflection upon what it is that researchers do when they call on social theory to help them comprehend their evidence. That, at least, is the starting point for chapter 1.

14. For the classic criticism of formal logic as distant from the sorts of argument that carry weight in different areas of inquiry, see Stephen Toulmin, *The Uses of Argument* (New York: Cambridge University Press, 2003). For a critique of how strong programs in the sociology of science, and more broadly the sociology of knowledge, themselves reify social structure in a contradictory way, see James Bohman, *New Philosophy of Social Science* (Cambridge, MA: MIT Press, 1993), 40–49.