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Helen E. Longino: The Fate of Knowledge

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CHAPTER ONE

Introduction: The Rational-Social Dichotomy

THE FATE OF KNOWLEDGE explores the epistemological consequences of two shifts in current philosophy of science: the growing recognition of the social character of scientific inquiry and the increasing acknowledgment of explanatory plurality in various scientific fields. Although not universally embraced, sociality and plurality have both been advanced by philosophers concerned to elucidate current practices in the sciences. These themes have important, if subtle, bearing on one another, given the interconnections between epistemological and metaphysical thought. This book focuses on epistemological concerns related to the sociality of inquiry, but takes up, as one direction of application, the implications for scientific pluralism of a social approach to scientific knowledge.

My aim in this book is the development of an account of scientific knowledge that is responsive to the normative uses of the term “knowledge” *and* to the social conditions in which scientific knowledge is produced. Recent work in history, philosophy, and social and cultural studies of science has emphasized one or the other. As a consequence accounts intended to explicate the normative dimensions of our concept—that is, elaborating the relation of knowledge to concepts such as truth and falsity, opinion, reason, and justification—have failed to get a purchase on actual science, whereas accounts detailing actual episodes of scientific inquiry have suggested that either our ordinary normative concepts have no relevance to science or that science fails the tests of good epistemic practice. This can’t be right. The chapters that follow offer a diagnosis of this stalemate and an alternative account. I argue that the stalemate is produced by an acceptance by both parties to the debate of a dichotomous understanding of the rational and the social.

The dichotomy between the rational (or cognitive) on the one hand and the social on the other structures both (1) the disagreements between the practitioners of the social and cultural studies of science and the philosophers and (2) the constructive (or deconstructive) accounts they all offer of scientific knowledge. Cognitive rationality and sociality are dichotomized when they are treated as definitionally excluding one another. According to the dichotomous understanding of these notions, if an epistemic practice is cognitively rational, then it cannot be social. Conversely, if an epistemic practice is social, then it cannot be cog-

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nitively rational.¹ What further is meant by “rational” or “cognitive” and by “social” varies from scholar to scholar. Roughly, rational or cognitive approaches are those that focus on evidential or justifying reasons in accounting for scientific judgment. Social (or sociological) approaches, by contrast, focus either on the role of nonevidential (ideological, professional) considerations or on social interactions among the members of a community rather than on evidential reasons in accounting for scientific judgment. Part of my task is to bring out the assumptions regarding cognitive rationality and sociality that make the dichotomy so compelling to a wide range of thinkers about the sciences. Rejecting these assumptions will open a way for the social account that I claim to be necessary for an epistemology of science inclusive of the full range of cognitive processes in the sciences.²

The argument of this book is directed explicitly at scientific knowledge. There are, however, reasons to see it as encompassing knowledge in general, or at least empirical knowledge in general. First, elements of scientific cognition on which the argument rests—observation and reasoning—are elements of empirical cognition in general. And, although important differences exist between scientific cognition and general empirical cognition, these do not bear on the normative issues involved in the ascription of knowledge. Second, the interdependence of scientists that sociological researchers have demonstrated is arguably a characteristic of all cognitive agents. A number of philosophers have argued recently that reliance on testimony is pervasive throughout our ordinary cognitive lives.³ We rely on what automobile mechanics, computer consultants, plumbers, physicians, historians, designated experts of all sorts, tell us, and weave that into the fabric of our beliefs. And, as cognizers, we are inducted from infancy into a complex set of assumptions that undergird our most mundane beliefs, so much that they come to be almost constitutive of our inferential capacities.⁴ Everyday knowledge is a matter

¹ I use “cognitively rational” and “cognitive rationality” to distinguish the kind of rationality in question here from pragmatic rationality, which is not understood as excluding the social in the same way.

² Hilary Kornblith (1994) has recognized part of this dichotomizing tendency. He notices a tendency on the part of sociologists to ignore or even deride the role of the truth of *p* in explaining belief that *p*. He diagnoses this tendency as a function of their assumption that truth-linked explanations are automatically undermined by successful social explanation. Kornblith argues that there is no necessary incompatibility between these two kinds of explanation. He does not, however, comment on the converse assumption nor the hold of both on philosophers as well as on sociologists. If my analysis is correct, there is a shared deep structure to the views of the philosophers and the sociologists that must be rejected if there is to be a genuine integration of truth-linked and social explanation.

³ See Coady 1992; Hardwig 1985, 1991; Kornblith 1987, 1994; Schmitt 1988.

⁴ Amanda Vizedom (2000) briefly discusses an artificial intelligence (AI) project whose

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of using our common sense in making judgments and inferences concerning aspects of experience that matter to us. Scientific investigation is a matter of extending the knowledge we have, of testing new proposals, of sometimes overturning common sense. The assumptions of common sense and the institutions that certify certain sorts of experts are of such long standing as to be past the kind of critical scrutiny that is required for assumptions that support inferences in the sciences. Nevertheless, the logical structure is parallel and, if those who emphasize the role of mundane background assumptions in our ordinary cognitive lives are correct, arguments about the nature of scientific knowledge are relevant to understanding the character and possibility of ordinary empirical knowledge.

PREDECESSORS

Contemporary philosophers who are exploring social dimensions of science or of knowledge are not the first to affirm the sociality of knowledge. Both John Stuart Mill and Charles Sanders Peirce in the nineteenth century and Karl Popper in the twentieth emphasized social aspects of knowledge or science that form part of the account developed in this book. Mill and Peirce wrote extensively on science, logic, and method, but they were also concerned to write of knowledge and truth in ways reflective of the actual situation of those who seek knowledge. The fundamental premise of Mill's discussion in the chapter "Liberty of Thought and Discussion" in *On Liberty* is based on the fallibility of human knowers.⁵ Peirce emphasizes the fallibility of individual knowers and offers a definition of truth that emphasizes the community of inquirers.⁶ Popper's embrace of criticism follows from more abstract considerations: his views about induction and consequent emphasis on falsification as the method of science.⁷

Mill's arguments occur in the context of his essay on the social and political topic of freedom rather than in the context of his logical and methodological writings, and his examples concern primarily religious belief and social and political beliefs. But a passing reference to earlier

aim is to model everyday inferences. Cyc® proposes that previous AI projects have failed because they have not taken into account "the store of 'commonsense assumptions' on which the everyday inferences depend" (Vizedom, 187). Part of the Cyc® project involves identifying those assumptions and entering them into the Cyc® Knowledge Base. For more information one can go to the Cycorp website: <www.cyc.com>.

⁵ Mill 1859.

⁶ Peirce 1877, 1878. All quotations are taken from the volume of selected writings by Peirce (1958) edited by Philip Weiner.

⁷ Popper 1968.

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controversies in astronomy and in chemistry and the very general character of his arguments suggest he intended his remarks to apply to any kind of knowledge or truth claim. His four reasons for recommending complete freedom of thought and unrestricted freedom of expression all rest on the assumed fact of individual human cognitive frailty: those who would restrict thought and its expression might be wrong and the truth lie with those whose thought is condemned; even if those who would restrict thought are in possession of the truth, (1) it is not likely to be the whole truth, and no one is, in any case, justified in claiming certainty, and (2) the reasons for and meaning of even true doctrines will be lost in the absence of contrary opinion. Mill, that is, is arguing that untrammelled opportunity for and practice of critical discussion of ideas is necessary for assuring us of the justifiability of the (true) beliefs we do have and for overcoming falsity or the incompleteness of belief or opinion framed in the context of just one point of view.⁸ At numerous other points, for example, the form of empiricism we each embrace, our views differ. Nevertheless, on the necessity of critical interaction for the integrity of knowledge Mill's view and the one I defend do coincide.

Charles Sanders Peirce's arguments concerning the social dimension of knowledge have a somewhat different basis than Mill's. While he too believes in the frailty of individual human knowers, his pragmatist theory of meaning also lies behind the apparently consensual theory of truth and reality that he offers. I say "apparently" because his precise view seems to me subject to different interpretations depending on which other of his remarks are juxtaposed to those on truth. In his essay, "How to Make Our Ideas Clear," he famously said that: "The opinion which is fated to be ultimately agreed to by all who investigate is what we mean by truth, and the object represented is the real."⁹ This definition is offered as exemplifying the pragmatist principle of defining a concept by reference to those "effects, which might conceivably have a practical bearing, we conceive the object of our conceptions to have."¹⁰ Whatever opinion succeeds in the long run in obtaining the consent of all who consider it has had the effect of so succeeding. Should we read Peirce as stating that truth (and the real) depends on our agreement, that it is, as some might say, manufactured by that agreement?[?] Or should we read him as stating that it is the inevitable effect of that which exists independently of us that it will in the end produce agreement? A clarificatory passage that follows suggests the first interpretation. "[R]eality is inde-

⁸ Mill's arguments illustrate the moral complexion of much writing on objectivity discussed by Lorraine Daston (1992).

⁹ Peirce 1878; 1958, 133. All quotations are from the Wiener edition of selected writings of Peirce (1958).

¹⁰ Peirce 1958, 124.

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pendent not of thought in general, but only of what you or I or any finite number of men may think about it.”¹¹ This does seem to make reality thought-dependent. On the other hand, in the very next paragraph, he states that “it is unphilosophical to suppose that, with regard to any given question (which has any clear meaning), investigation would not bring forth a solution of it, if it were carried far enough.”¹² And in “The Fixation of Belief,” he affirms the following as the “fundamental hypothesis” of science. “There are real things, whose characters are entirely independent of our opinions, . . . [that] affect our senses according to regular laws, and by taking advantage of the laws of perception we can ascertain by reasoning how things really are.”¹³ Here, reality seems independent of all thought, general or individual.

These apparent conflicts may be reconciled in a single coherent interpretation of Peirce. But closer readers of Peirce than I have differed on just how to interpret his theory of truth and reality.¹⁴ Certainly, it is clear that whatever truth is, Peirce thinks it both attainable and beyond the cognitive reach of any single individual. In another essay, a critique of Cartesianism, recommending what he takes to be the practice in the empirical sciences, he states, “We individually cannot hope to attain the ultimate philosophy which we pursue; we can only seek it for the community of philosophers.”¹⁵ Inquiry is a community, a social, activity, and its success is achieved by community practices, not by solitary ones. In an echo of Mill’s discussion, he says of the “Critical Common-sensist” that he “attaches great value to doubt. . . he invents a plan for attaining to doubt . . . and then puts it into practice.”¹⁶ Doubt is inculcated by the contrary opinions of others. So critical interaction is necessary for the improvement of individual opinion and also for the attainment of the (ultimate) truth by the community (consisting of all investigators who have ever lived). There are enough tantalizing remarks scattered throughout these essays to make the prospect of interpreting Peirce an attractive and rewarding, as well as challenging, one. But what follows is not in any way an interpretation of what he might, could, or should have meant. I do, however, join Peirce in emphasizing the role of the community in the production or attainment of knowledge.

The challenge to philosophy of reconciling the cognitive rationality of the sciences with the conditions of their practice is vividly illustrated in the work of Sir Karl Popper, often considered to have originated social

¹¹ *Ibid.*, 133.

¹² *Ibid.*, 134.

¹³ Peirce 1877; 1958, 107–8.

¹⁴ Cf. Rorty 1982; Haack 1993.

¹⁵ Peirce 1868; 1958, 40.

¹⁶ Peirce 1905; 1958, 214.

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epistemology with his emphasis on the importance of criticism in the development of scientific knowledge. But Popper vacillated in his conception of criticism.¹⁷ At times he wrote as though criticism is wholly a matter of logical relations in what he came to call World Three, a world of propositional content, which is the objective world of knowledge. What sets this world apart from World Two—the subjective world of human belief states and human interactions—is its determinate character. That a theory is or is not a solution to a problem situation, that it does or does not contradict another theory, that it does or does not have a particular empirical consequence, are all matters of determinate logical relations, independently of their being thought. Criticism is just correctly identifying the consequences of a theory and comparing them to the empirical basis of science. This kind of criticism, hypothesis testing that can be accomplished by any individual with the power of correct ratiocination, is at the heart of his falsificationist methodology for science.

At other times, he writes as though criticism is a social matter—an affair of competing scientists trying to demonstrate the inadequacies of one another's theories by means of alleged observational discrepancies or conceptual and metaphysical shortcomings. This social activity is necessary for Popper's picture, at least as an engine of the hypothesis-testing criticism that identifies the logical relations that are the substance of critical interaction, but it is also its undoing. It is necessary because Popper's Third, objective, World is static. Something must generate the confrontation of theory with theory or of theory with experience that animates criticism, and the only candidate is the attitudes and actions of persons—scientists—who, for whatever reason, subject theories to criticism. It is the undoing of Popper's picture because, while he treats the logical relations of World Three as determinate, he minimizes the fact that there may be many logical trajectories between any given hypothesis or theory and a set of data. Given the gap between theory and data, there are many different assumptions that can fill it, and hence many possible trajectories between the elements of any given problem situation and theory pair. Back in the real world of real scientists, Popper's World Two, there are many constraints operating to limit the range of assumptions that will mediate perceived relations between theory and data. Which of the myriad sets of logical trajectories that can be followed from theory to the empirical basis that is the concern of any particular scientist or of any given scientific community at any given time is a matter not only of logic but also of contingent factors operating in the world of human affairs.

¹⁷ I draw here on Popper 1972 and freely admit to glossing over many of the intricacies of his argument in the interest of the particular observation I wish to make.

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The objective world is a world of multiple possibilities. Selection among, or activation of, one or more of those possibilities lies outside the scope of Popper's epistemology. Unlike the discussions by Mill and Peirce, Popper's theory of scientific knowledge deliberately bypasses the connection to science and inquiry as practiced and remains an epistemology of the ideal. By treating the rational and the social as so exclusive of each other as to occupy different worlds, Popper falls victim to the dichotomy in spite of having had some of the tools to escape it.

PREVIEW

The first section of *The Fate of Knowledge* examines the operation of the dichotomy between the rational and the social in the work of contemporary scholars in science studies. Chapter 2 engages with work in social and cultural studies of science, which can be roughly divided into two main streams. One has focused on the relationship between scientific knowledge and relatively large-scale professional and ideological social formations. Members of the so-called Edinburgh school or Strong Programme have been at the forefront of this line of analysis, producing books like Andrew Pickering's *Constructing Quarks* and Steven Shapin and Simon Schaffer's *Leviathan and the Air Pump*.¹⁸ This tradition has focused on scientific controversy, demonstrating the professional and political interests bound up with different sides in a controversy, and arguing that the resolution of the scientific face of controversy is determined by a resolution of the political or professional face—that is, that knowledge or what counts as knowledge is determined by social interests.

The other stream, often identified as laboratory studies or micro-sociology, has focused on the interactions within and between laboratories and research programs and on the efforts required to export laboratory work into the nonlaboratory world. Here, the explanatory weight falls on the multiple small decisions taken in the course of research. These decisions, according to practitioners like Karin Knorr-Cetina or Bruno Latour, are diverse in nature and have multiple dimensions. The overall aim for any researcher is being able to continue her or his research, and any considerations related to that goal—funding, connection to other research programs or to industrial applications, ease of representation, as well as empirical considerations—are legitimate constituents of research decisions. Knowledge is produced by an amalgam of heterogeneous acts and not by a particular kind of truth-producing activity guided by logic. Knorr-Cetina and others emphasize the contin-

¹⁸ Pickering 1984; Shapin and Schaffer 1985.

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gency of scientific decisions, implying that contingent relations escape the purview of the normative philosopher. Both kinds of scholarship in social studies of science have produced brilliant and provocative historical and sociological analysis. What concerns me is the claim that these analyses show the irrelevance of normative philosophical concerns to real science. I will show how this claim is the product of the rational-social dichotomy.

The work in social and cultural studies has stimulated a range of responses from philosophers. Some, like Larry Laudan, have simply rejected the relevance of this work to philosophical concerns, or, like Alvin Goldman, have seen it as empirically and conceptually misguided. Some, like Philip Kitcher or James Brown, have tried to take the sting out by sifting through the claims of sociologists and sociologically oriented historians, attempting refutation of those they deem extremist, and then incorporating a sensitivity to history or sociological analysis into their constructive accounts of inquiry. In chapter 3, I argue that these efforts, too, are vitiated by a commitment to the dichotomy of the rational and the social.

In the central chapters of the book, I offer an account of scientific knowledge that not only avoids the dichotomy but integrates the conceptual and normative concerns of philosophers with the descriptive work of the sociologists and historians. This involves first disambiguating three senses of knowledge (as content, as practices or procedures, and as state), analyzing the constituents of the dichotomy in order to remove the aura of self evidence that attends it, and finally offering nondichotomizing accounts of each sense. Knowledge as content is the product of the exercise of basic cognitive capacities that conforms to its intended object. So far, the account is within the mainstream of philosophy. But I argue that these cognitive capacities are, in science, exercised socially, that is, interactively. I also argue that “conforms” encompasses a family of semantic success terms and that such a latitudinarian view is required by the varied character of scientific content.

Two features of scientific inquiry make its epistemology more complicated than traditional philosophy of science has appreciated. One is this social character of its cognitive, or knowledge-productive, capacities. The second is the gap between its explanatory resources (data) and its explanatory aspirations (theories).¹⁹ I reiterate and expand the argument developed in my first book, *Science as Social Knowledge*,²⁰ that individual observation and reason must be supplemented by social processes such as

¹⁹ Of course, there's more to knowledge than data and theories, but this oversimplification serves for the point.

²⁰ Longino 1990.

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critical discursive interaction and that a more complete epistemology for science must include norms that apply to practices of communities in addition to norms conceived of as applying to practices of individuals. I incorporate these norms into an account of epistemic acceptability, and use the normative concepts of epistemic acceptability and conformation to provide nondichotomous accounts of knowledge in the three senses distinguished previously. Following through on the consequences of the analysis means breaking with conventional views of scientific knowledge as permanent, as ideally complete, and as unified or unifiable. This break, in turn, means accepting the provisionality, partiality, and plurality of scientific knowledge. Many philosophers would be content to accept this as a characterization of science today, but not of science in the future, or of ideal science. Epistemology ought to be for that ideal science. I insist instead on an epistemology for living science, produced by real, empirical subjects. This is an epistemology that accepts that scientific knowledge cannot be fully understood apart from its deployments in particular material, intellectual, and social contexts.

Contemporary philosophers have both resisted the strong form of sociality in knowledge defended here and developed different articulations and interpretations of a strong social approach. In chapter 7, I take up a series of objections to the view. I use the distinction between the modalities of knowledge to solve some of the puzzles that philosophers have posed regarding the nature of individual epistemic agency in a social approach to knowledge. I show how the account, while flexible enough to accommodate pluralism, also has the resources to identify both wrong-headed content and wrongheaded practices. Finally I explore differences between my views and those proposed by other strongly social philosophers of science, such as Miriam Solomon, Steve Fuller, and Joseph Rouse.

In the eighth and final substantive chapter of the book, I apply the socialized conception of knowledge to the new pluralism in philosophy of science. I review a number of different instances of theoretical plurality in recent and contemporary biology. These instances exhibit differences in approach—that is, in methods and questions addressed to a phenomenon or different initial characterizations of a common domain or both. While some of the examples are subjects of ongoing controversy, I appeal to some recent historical and philosophical scholarship to show how even apparently settled issues like the synthetic theory of evolution and the structure of genes are better understood in the pluralist framework of the social account. In a brief concluding chapter, I indicate some of the other directions in which the argument of the book can be pursued.

SOME PHILOSOPHICAL PRELIMINARIES

The approach to knowledge I take in this book is naturalist in the sense that it treats the conditions of knowledge production by human cognitive agents, empirical rather than transcendental subjects, as the starting point for any philosophical theory of knowledge, scientific or otherwise. It is not naturalist in the sense of treating knowledge as a natural kind whose nature or essence can be discovered (by scientific or philosophical methods). Belief, as a psychological state, is a kind of some sort, but knowledge is not a separate kind to be distinguished from belief as the psychological state of doubt can be distinguished from the psychological state of certainty. If knowledge is a kind at all, it is an unnatural kind, a product of human activities. As a product of human activities, it belongs in the category of other such things, like tables and chairs, but it is not a substantial kind as these, however artifactual, are. It is, rather, a status certain kinds may have. “Knowledge” is what Gilbert Ryle called a “success term,” ascribed to exemplars of certain kinds—in this case: beliefs, claims, theories—when they satisfy certain conditions. “Knowledge” and its cognates, then, are normative concepts.

A philosophical theory of knowledge spells out the conditions that our use of the term “knowledge” indicates must be satisfied for correct ascription. On this understanding, a philosophical theory of knowledge is neither purely descriptive nor purely prescriptive. It requires a characterization of empirical subjects and of the situations in which they seek to produce (things that have the status of) knowledge. This is descriptive. It also involves a characterization of the conditions satisfaction of which they take the status to imply. This constitutes a spelling out of the prescriptions implicit in the ascription and withholding of that status. Filling in the details between the empirical conditions of knowledge production and the ideal conditions of success is the job of philosophical theory. The fate of *knowledge* lies in the consequences of those details.