To my teachers in philosophy, especially

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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-
natively rational. 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 non-evidential (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.

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 ascertainment 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. 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. Everyday knowledge is a matter 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. Peirce emphasizes the fallibility of individual knowers and offers a definition of truth that emphasizes the community of inquirers. Popper’s embrace of criticism follows from more abstract considerations: his views about induction and consequent emphasis on falsification as the method of science.

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...
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 untrammeled 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. 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 pragmaticist 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.” This definition is offered as exemplifying the pragmaticist 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.” 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-
epistemology with his emphasis on the importance of criticism in the development of scientific knowledge. But Popper vacillated in his conception of criticism. 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, 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.

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.* 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 microsociology, 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-

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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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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 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 wrongheaded 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.

19 Of course, there's more to knowledge than data and theories, but this oversimplification serves for the point.
20 Longino 1990.
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.

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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.

CHAPTER TWO

Taking Social Studies of Science Seriously

Sociologists and anthropologists of science have posed a significant challenge to philosophy. Through a number of different case studies, they have argued that scientific knowledge is not developed by the application of procedures ratified by epistemological norms of the sort recognized by philosophers. Instead, scientists use any means necessary—negotiate, borrow, barter, and steal—to get their interpretations accepted, their aim being to win or at least survive in the game of science. “Science is socially constructed” has become both a rallying cry and a banal comment, but this motto does not mean the same thing to all who use it. And the difference makes a difference.

According to the so-called Strong Programme in the sociology of scientific knowledge (SSK), good and bad, successful and unsuccessful science must be explained in the same way. One can not explain good science by appealing to rationality and bad science by appealing to distorting social factors. In both kinds of case what does the explanatory work is interests: ideological-political interests, professional interests, individual career interests. In the Strong Programme, to say that science is socially constructed is to say that it is (primarily) the congruence of a hypothesis or theory with social interests of members of a scientific community that determines its acceptance by that community (rather than a congruence of theory or hypothesis with the world). Whether that theory or hypothesis then gains general acceptance is a function of social interactions of the community endorsing or advocating it with other communities constituting the larger scientific community.

The empirical laboratory studies done by Karin Knorr Cetina, Bruno Latour, and others understand social construction differently. For them, the procedures of science are social in the sense of involving social interactions, rather than in the sense of writing social relations into (our conceptions of) nature. They focus on the procedures of the laboratory.

1 Philosophers have engaged most with the Strong Programme in the sociology of science associated with the University of Edinburgh and as represented in the work of Barry Barnes and David Bloor. See Barnes 1977; Bloor 1991; Barnes and Bloor 1982. This program has attracted the attention of a number of philosophers; see for example, a sympathetic if critical reading by Mary Hesse (1980), and much less temperate responses from Larry Laudan (1984a), among others.

CHAPTER SIX

Socializing Knowledge

The analysis continues by offering definitions of "knowledge" in each of the modalities distinguished in the previous chapter. These definitions incorporate norms applicable to social interaction as well as philosophically conventional epistemic norms. I offer three arguments for this socialized (not sociologized) account of knowledge: the socialized account overcomes the difficulties posed by the underdetermination problem without simply denying its existence or employing covert metaphysical principles; meets the challenges posed by the sociologizers and the rationalizers to one another, and, unlike other accounts, is neutral with respect to the question of pluralism or monism.

UNDERDETERMINATION AND CONTEXT

It is tempting to think that scientific knowledge is like ordinary knowledge except better. But scientists are not (or not just) better observers and more careful reasoners than the rest of us, they also observe and reason differently and to different ends. The purpose of scientific inquiry is not only to describe and catalog, or even explain, that which is present to everyday experience, but to facilitate prediction, intervention, control, or other forms of action on and among the objects in nature. Description and classification are in service to these more overarching purposes, which move the focus of inquiry away from what is present to us to the principles, processes, and mechanisms that produce or underlie what is present to us. This involves making visible (e.g., via dissection or vision-enhancing instruments) what is invisible; in other cases it involves postulating, and devising indirect methods of investigating, processes and mechanisms beyond the range of human sensory capacities—too long or too short in duration, too big or too small to be perceived, outside of the frequency ranges to which human senses are open. In most cases these activities take place in and on idealized situations, whether in the laboratory or in thought experiments, so as to be able carefully to examine one aspect of a process rather than be stymied by attempting to render its full complexity. This way of experiment is arguably what most distinguishes modern Western science from other modes of knowledge or inquiry.

The concern with processes and mechanisms that facilitate human intervention in the natural world is manifest in the kind of science first fully developed in the modern period—mechanics—and in the close ties that have developed between Western productive capacities and scientific knowledge. In spite of this concern with what underlies experienced processes, establishing theories, in the sense of large-scale systematic accounts of a range of related phenomena, is not at the center of most scientists' work. Rather they presuppose theories and defend them when challenged, but as a distraction from their central mission, which is to answer (in light of general theoretical frameworks) specific, carefully articulated questions addressed to the idealized situation of the physical or mental laboratory or to some carefully circumscribed portion of the natural world. The practice of understanding natural phenomena by reference to underlying causal processes gives rise to the logical problem of underdetermination.

The representation of the general structure of inquiry as shaped by a theoretical framework both obscures and makes visible the underdetermination that is the condition of empirical research that seeks access to underlying states and processes. It obscures it by making questions asked in a real setting the salient feature of inquiry. Philosophical and sociological scholars of science who focus on the details of particular scientific investigations move issues of large-scale justification to the periphery of concern as they show how knowledge is produced within a framework both they and the scientists whose work they study hold constant. Thus, the issue of justification as traditionally understood, and for which underdetermination is a clear challenge, is displaced from the center of attention. But, at the same time, this representation makes underdetermination visible by referring, even if parenthetically, to the theoretical framework within which questions are formulated. The dependence on such frameworks, necessary to engage in constructive reasoning about underlying processes, produces a gap. This gap between what is present to us and the processes that we suppose produce the world that is present to us, between our data and the theories, models, and hypotheses developed to explain the data, has been at the heart of philosophical reflection about scientific knowledge. Once the logical empiricists abandoned the notion that all meaningful theoretical statements could be fully translated into observational statements, the problem of characterizing the relation between observation and theory haunted philosophy of science. Russell Hanson, Thomas Kuhn, and Paul Feyerabend emphasized the ways in which theory influenced observation. They introduced the notion of the

1 Kitcher (1993) seems to advance this position.

2 Hanson 1958; Kuhn 1962; Feyerabend 1962.
“theory-ladeness” of observation—that is, that observation is not neutral, but is permeated by theoretical commitments. The meaning of scientific terms, too, was held to be theory-laden. This closes the gap between observation and theory, but at the price, as many philosophers protested, of circularity. Philosophical discussion of theory-ladenness seems to have stabilized in agreement that observation reports both depend on and imply theoretical commitments, but that as long as the theory that loads the observation or description of data is not the theory for which the data are invoked as evidence, circularity is avoided.

Underdetermination, however, persists. As long as the content of theoretical statements does not consist of generalizations of data or the content of observational statements is not identified with theoretical claims, then there is a gap between hypotheses and data, and the choice of hypothesis is not fully determined by the data. Nor do hypotheses specify the data that will confirm them. Data alone are consistent with different and conflicting hypotheses and require supplementation. This problem is distinct from the traditional problem of justifying induction, which concerns the relation between a generalization and its instances. Pierre Duhem, the first philosopher of science to raise the underdetermination problem, emphasized assumptions about instruments, for example, that a microscope has a given power of resolution, or that a telescope is transmitting light from the heavens and not producing images internally, or not systematically distorting the light it receives.

But the content of background assumptions also includes substantive (empirical or metaphysical) claims that link the events observed as data with postulated processes and structures. For example, that two kinds of event are systematically correlated is evidence that they have a common cause or that one causes the other in light of some highly general, even metaphysical, assumptions about causality. The correlation, for example, of exposure to or secretion of a particular hormone with a physiological or behavioral event is evidence that the hormone causes the physiological or behavioral phenomenon in light of an assumption that hormones have a causal or regulative status in the processes in which they are found rather than being epiphenomenal to or effects of those processes. Such an assumption has both empirical and metaphysical dimensions. Assumptions of this kind establish the evidential relevance of data to hypotheses. They provide a model of the domain being investigated that enables particular investigations to proceed. Some philosophers have proposed principles, such as simplicity or explanatory power, as closing the gap between hypotheses or theories and their evidence. There are well-known difficulties in interpreting any of these proposed principles, as well as in applying them. So, while individuals or even research communities may rely on them in the selection of hypotheses, their epistemic status is the same as that of more substantive assumptions.

Underdetermination has sometimes been called the Duhemian problem and, as such, has been expressed as the in-principle possibility of constructing multiple empirically equivalent, mutually inconsistent theories for any given body of evidence. Quine enlisted a version of Duhem's arguments in his attack on the logical empiricist version of the distinction between observation and theory, and in support of his views concerning the revisability of any statement in a network of beliefs. In the absence of equally developed empirically equivalent theories, this possibility seems, as Hacking puts it, fanciful. But this is only one way of understanding underdetermination. The underdetermination problem, as I understand it, does not depend on wholistic views about meaning, or on a metaphysical or semantic view about observation and theory. It is an epistemological problem: the consequence of the gap between evidence and hypotheses produced by our postulation of different entities and processes in our explanations of phenomena than occur in our descriptions of those phenomena.

The background assumptions that fill that gap, then, include substantive and methodological hypotheses that, from one point of view, form the framework or proximate intellectual context within which inquiry is pursued and, from another, structure the domain within which inquiry is pursued. These hypotheses are most often not articulated but presumed by the scientists relying on them. They facilitate the reasoning between what is known and what is hypothesized. From a traditional perspective this raises major problems for justification: if the justification of hypotheses requires assumptions, then how are these assumptions in turn justified? And how is it possible to screen out biasing factors such as individual idiosyncrasies, wishful thinking, values, social prejudices, ideologies, tacit metaphysics? In some cases evidence for assumptions can be offered, but the same problem of underdetermination besets this evidential reasoning as well. Even if scientists are not concerned in their daily practice with directly justifying the assumptions that structure their reasoning, philosophers concerned with the nature of scientific knowledge must be.

1 See in this connection Hesse 1980, 63-110.
2 Duhem 1954.
4 See van Fraassen 1989; Longino 1996.
5 Hacking 1999.
The logical problem of underdetermination shows that empirical reasoning takes place against a background of assumptions that are neither self-evident nor logically true. The identification of good reasons is similarly context-dependent, whether this is accomplished by the scientist in action or by the philosopher in reflection. As we saw in chapter 1, some sociologists of science have used versions of the underdetermination problem to argue that epistemological concerns with truth and good reasons are irrelevant to the understanding of scientific inquiry. The point, however, should not be that observation and logic as classically understood are irrelevant but that they are insufficient. The sociologists' empirical investigations show that they are explanatorily insufficient. The underdetermination argument of philosophers shows that they are epistemically insufficient. Rather than spelling doom for the epistemological concerns of the philosopher, the logical problem of underdetermination together with the studies of laboratory and research practices changes the ground on which philosophical concerns operate. This new ground or problem situation is constituted by treating agents or subjects of knowledge as located in particular and complex interrelationships and acknowledging that purely logical constraints cannot compel them to accept a particular theory. That network of relationships—among other individuals, social systems, natural objects, and natural processes—is not an obstacle to knowledge, but can be understood as a rich pool of varied resources, constraints, and incentives to help close the gap left by logic. The philosophical concern with justification is not irrelevant, but must be somewhat reconfigured in order to be made relevant to scientific inquiry.

SOCIAL NORMS FOR SOCIAL KNOWLEDGE

Elsewhere I have argued that an individualist bias in philosophy stands in the way of solutions to problems, such as underdetermination, confronting the possibility of scientific knowledge. Some see the solution afforded by a social account of knowledge as worse than the problem. I claim, in contrast, that this judgment is a result of accepting a false dichotomy between the rational or cognitive and the social and that scientific knowledge is produced by cognitive processes that are fundamentally social. An adequate normative theory of knowledge must then be a normative theory of social knowledge, a theory whose norms apply to social practices and processes of cognition.

Critical discursive interactions are social processes of knowledge production. They determine what gets to remain in the public pool of information that counts as knowledge. Thus, a normative account of knowledge must rest on norms governing such interactions. Criticism must be epistemologically effective—by helping a community avoid falsehood and by helping to bring its accepted content into alignment with its cognitive goals and its cognitive standards. Effective critical interactions transform the subjective into the objective, not by canonizing one subjectivity over others, but by assuring that what is ratified as knowledge has survived criticism from multiple points of view. I reiterate, with some modifications, the criteria I take to be necessary to assure the effectiveness of discursive interactions.

1. Venues. There must be publicly recognized forums for the criticism of evidence, methods, and of assumptions and reasoning. This means that criticism of research ought to be articulated in the same standard and public venues in which "original research" is presented: journals, conferences, and so on. In addition, critical activities should be given the same weight or nearly the same weight as is given to "original research": effective criticism that advances understanding being as valued as original research that opens up new domains for understanding; pedestrian, routine, or uninformed criticism being valued comparably with pedestrian, routine, or incompetent "original research." As Mill argued, criticism not only spurs evaluation and reevaluation of hypotheses, but also leads to better appreciation of their grounds and of their consequences. Readers may wonder whether the availability of venues is not so trivial a requirement as not to be worth mentioning. On the contrary, a complex set of processes in the institutions of contemporary science in the industrial and post-industrial world works against satisfaction of this requirement. The limitations of space, the relation of scientific research to production and commerce whose consequence is privatization of information and ideas, and an understanding of research as the generation of positive results, all contribute to the marginalization of critical discourse.

2. Uptake. There must be uptake of criticism. The community must not merely tolerate dissent, but its beliefs and theories must change over time in response to the critical discourse taking place.

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* For a reaffirmation of the problem of underdetermination against a variety of recent attempts to defuse it, see Potter 1996. For another recent discussion, see Hempel 1988.

* I am grateful to C. K. Waters for his helpful suggestions regarding this formulation. It puts philosophical concerns into relationship with actor-network theory rather than seeing them as antagonists.


* The transformation in question is one of status, not one of kind. For additional discussion of subjectivity and objectivity, see Longino 1990, 1994; Lloyd 1995a, 1995b.
within it. This standard does not require that individuals or research groups capitulate to criticism, but that community members pay attention to and participate in the critical discussion taking place and that the assumptions that govern their group activities remain logically sensitive to it. The change may comprise the acceptance of different beliefs, the modification of beliefs, the development of new data, reasons, and arguments. Uptake is what makes criticism part of a constructive and justificatory practice. Uptake cuts both ways: not only must the community be responsive, but the claims of advocates of a line of criticism must take account of those responses.

3. Public Standards. There must be publicly recognized standards by reference to which theories, hypotheses, and observational practices are evaluated and by appeal to which criticism is made relevant to the goals of the inquiring community. First, in order for criticism to be relevant to a position, it must appeal to something accepted by those who hold the position criticized. Similarly, alternative theories must be perceived to have some bearing on the concerns of a scientific community in order to obtain a hearing. Participants in a dialogue must share some referring terms, some principles of inference, and some values or aims to be served by the shared activity of discursive interaction. Thus, shared elements are necessary for the identification of points of agreement, points of disagreement, and what would count as resolving the former or destabilizing the former. Second, relevance to the concerns of a community is not a function of the whim of individuals but rather of public standards or criteria to which members of the community are or feel themselves bound. The point is not so much that individuals spontaneously act out their allegiance to these standards as that they acknowledge their relevance to the evaluation of cognitive practices in their community of inquiry. A community's standards are themselves subordinated to its overall cognitive aims, which will be implicit in its practices even if not fully explicit.

The point of requiring standards that are public is that by explicitly or implicitly professing adherence to those standards individuals and communities adopt criteria of adequacy by which they may be nonarbitrarily evaluated. The satisfaction of goals of inquiry is not ascertained privately, but by evaluation with respect to shared values and standards. This evaluation may be performed by anyone, not just by members of the community sharing all standards.

Finally, standards are not a static set but may themselves be criticized and transformed, in reference to other standards, goals, or values held temporarily constant. Indeed, as in the case of observation and the assumptions underlying justificatory reasoning, the presupposition of reliance on such standards is that they have survived similar critical scrutiny. There is no particular act of adopting or establishing standards. Rather they come to operate as such in the same ways that content is accepted as knowledge: by being let stand through multiple acts of microcognition and microcriticism.¹⁰

4. Tempered Equality. Finally, communities must be characterized by equality of intellectual authority. Put this way, the criterion is too crude.¹¹ The experience of most people reading this passage is that, even when we limit our consideration to adults, members of our communities differ in intellectual capacity. The difficulty is that some may differ because of innate endowment: some of us are better at certain kinds of things than others. And some may differ because of schooling and other opportunities. So, equality must be qualified or tempered. Specification of the kind of tempering to which equality must submit requires specifying the role and purpose of this criterion.

A diversity of perspectives is necessary for vigorous and epistemically effective critical discourse. The social position or economic power of an individual or group in a community ought not determine who or what perspectives are taken seriously in that community. Where consensus exists, it must be the result not just of the exercise of political or economic power, or of the exclusion of dissenting perspectives, but a result of critical dialogue in which all relevant perspectives are represented.¹² While this criterion has obvious affinities with Jürgen Habermas's account of truth, it is not offered as a criterion of truth, but as a criterion distinguishing legitimate from illegitimate consensus. The requirement of (tempered) equality of intellectual authority implies both that the persuasive

¹⁰ For the role of letting stand in collective belief, see Gilbert 1987.
¹¹ C. K. Waters has persuaded me that equality simpliciter is too crude. He proposes "appropriate distribution of intellectual authority" as a more sensitive criterion. I have instead opted for "tempered equality," borrowing from the title of one of his papers, but not at his suggestion.
¹² Here is where this criterion, as an epistemological criterion, differs from equality as a moral or political criterion. The latter would require only that different perspectives have an equal chance of being included, whereas epistemic effectiveness requires that all (relevant) perspectives be included.
effects of reasoning and argument be secured by unforced assent to the substantive and logical principles used in them, rather than by properties, such as social or economic power, of those who are propping them; and that every member of the community be regarded as capable of contributing to its constructive and critical dialogue.\footnote{There can be mixed cases. The substantive principles may include socially, economically, or otherwise valenced content. And there may be occasions when dialogue selects several possibilities, community selection among which is effected by mimicry of the most powerful. In such cases, delegation to a subgroup should be regarded as a practice or standard that must survive criticism in conditions characterized by the four criteria.}

The point of the requirement is to ensure the exposure of hypotheses to the broadest range of criticism. An example of the consequences of this condition is the kind of judgment it supports of exclusionary practices. The exclusion of women and members of certain racial minorities from scientific education and the scientific professions constitutes not only a social injustice but a cognitive failing. Similarly, the automatic devaluation in Europe and North America of science from elsewhere constitutes a cognitive failing. Feminist scholars have demonstrated how assumptions about sex and gender structure a number of research programs in biological, behavioral, and other sciences.\footnote{Cf. Keller 1985, 1992; Blier 1984; Hubbard 1990; Jordanova 1993; Haraway 1989, 1991, among others.} Historians and sociologists of racist practices and ideologies have documented the role of racial assumptions in the sciences.\footnote{Cf. Lewontin, Rose, and Kamin 1984; Proctor 1988; Gould 1981; Paul 1995, among others.} The long-standing devaluation of women’s voices and of those of members of racial minorities means that such assumptions were for a long time protected from critical scrutiny. Even if the absence of women and members of ethnic minorities was self-chosen, rather than imposed, it would constitute a flaw, as their absence reduces the critical resources of the community. Thus a community must not only treat its acknowledged members as equally capable of providing persuasive and decisive reasons and must do more than be open to the expression of multiple points of view; it must also take active steps to ensure that alternative points of view are developed enough to be a source of criticism and new perspectives. Not only must potentially dissenting voices not be discounted; they must be cultivated.

Equality is, nevertheless, tempered in several respects. While the criterion imposes duties of inclusion and attention, it does not require that each individual, no matter what their past record or state of training, should be granted equal authority on every matter. The public standards mentioned in the preceding condition are intended partly to protect inquiry from such cacophony, for their obligations cut two ways. Subscription to them does impose obligations on members of a knowledge-productive community to attend to criticism that is relevant to their cognitive and practical aims. But it also limits the sorts of criticisms to which a community must attend to those which affect the satisfaction of its goals. The point of the present condition is that such criticism may originate from an indeterminate number of points of view, none of which may be excluded from the community’s interactions without cognitive impairment. Nevertheless, the advocates of a point of view, and through them the point of view itself, may lose or even forfeit intellectual authority if their discursive interactions do not satisfy the second condition of uptake. That is, reiterating the same old complaint no matter what response is offered eventually disqualifies one as a member of a discursive community of equals.

The foregoing remarks are both gestures toward spelling out how equality must be tempered and indications of the sorts of questions that must yet be addressed. For example, in determining what counts as inappropriate exclusion of dissenting perspectives, does it matter what kind of issue is involved? Are the duties of inclusion different when the question is, Should we be trying to learn about such and such, for example, atomic fission? than when it is, Is atomic fission a controllable or uncontrollable process? What is the range of matters to which the criterion of tempered equality applies? Does it extend, for example, to whether and how to use nonhuman animals in experimentation? Furthermore, the condition speaks of intellectual authority, not cognitive authority.\footnote{In this distinction, cognitive authority has to do with the amount of knowledge one has, and is thus fairly domain-specific. One can be cognitively authoritative with respect to matters in astrophysics but ignorant of cell biology. Intellectual authority is less a matter of having knowledge than of having cognitive or intellectual skills of observation, synthesis, or analysis, which enable one to make cogent comments about matters concerning which one knows less than another.} It is compatible with according greater cognitive authority on some matters to those one regards as having acquired more knowledge concerning those matters than others. What bearing should greater cognitive authority have on the attribution of intellectual authority, understood as the capacity to participate in critical discussion and thus to contribute to critical understanding? The tempered equality condition also raises complex questions of community membership. It requires both that scientific communities be inclusive of relevant subgroups within the
society supporting those communities and that communities attend to criticism originating from “outsiders.” It makes us ask who constitutes the “we” for any given group, and what the criteria are for providing an answer. Are “we” those actively engaged in producing knowledge of a certain kind for a certain aim, as members of a laboratory group are, or should “we” encompass also all those potentially affected by that knowledge? Do our educational practices exclude certain perspectives or discourage criticism? The social approach to scientific knowledge places such questions at the center of philosophy of science. While some of them will be broached in later chapters, they require far more discussion than is possible in this book.

The features I have enumerated are features of an idealized epistemic community. In practice, then, they are criteria of knowledge-productive capacity that take the critical interactions within communities and institutions as their domain of application. As such they constitute norms applying to the social practices and processes of cognition described in the previous chapter. Satisfaction of these norms assures that theories and hypotheses accepted in the community will not incorporate the idiosyncratic biases (heuristic or social) of an individual or subgroup. We might call them conditions of effective or transformative criticism. Insofar as a community satisfies these conditions, that is, insofar as a community maintains means of disseminating and responding to criticism, and is constituted of members who hold themselves answerable individually and collectively to a set of standards and reach consensus as a result of discursive interactions including all relevant perspectives and uninhibited by political or economic power, it will qualify as a knowledge productive community. Because the norms represent ideals that can be partially satisfied, this qualification is a matter of degree. Communities may be more or less dependable and effective as producers of knowledge.

Whereas the previous chapter offers accounts of the senses or modes of knowledge which made salient their social aspects, this chapter offers norms of (discursive) interaction that apply to these social aspects. These norms enable us to distinguish those social interactions which are productive of knowledge from those which are not. I have argued that cognitive practices are social and that the located character of empirical subjects and the underdetermination problem together mean that scientific justification must be social. To the degree that cognitive processes and practices are social, they are subject to evaluation according to those norms, as well as according to the basic empiricist norms of (experiential) evidence and (valid) reasoning.

Socializing knowledge and epistemology in this way has consequences. First, knowledge does not have to be regarded as a fixed content but can be seen as constantly changing. The content changes not just by expanding, but by replacement as cognitive criteria and aims themselves change as a consequence of the application of knowledge to the solution of practical problems. Knowledge can be understood as dynamic, producing the conditions of its own eventual surpassing. Second, even though a community may operate with effective structures that block the spread of idiosyncratic assumptions, those assumptions that are shared by all members of a community will not only be shielded from criticism, but, because they persist in the face of effective structures, may even be reinforced. One obvious solution is to require interaction across communities, or at least to require openness to criticism both from within and from outside the community. Here, of course, availability is a strong constraint. Other communities that might be able to demonstrate the non-self-evidence of shared assumptions or to provide new critical perspectives may be too distant—spatially or temporally—for contact. Background assumptions, and public standards generally, then, are only provisionally legitimated; no matter how thorough their scrutiny given the critical resources available at any given time, it is possible that scrutiny at a later time will prompt reassessment and rejection. Such reassessment may be the consequence not only of interaction with new communities but also of changes in values or other assumptions within a community. Thus, the changes in a community’s knowledge can have multiple sources: new data generated within research, new values, reassessment of basic assumptions.

DEFINING KNOWLEDGE

The analysis of the previous section can be integrated with that of the previous chapter to generate definitions of knowledge in the three senses distinguished. To do so it is useful to summarize the proposal of the preceding section as follows:

Some content A is epistemically acceptable in community C at time t if A is or is supported by data d evident to C at t in light of reasoning and background assumptions which have survived critical scrutiny from as many perspectives as are available to C at t, and C is characterized by venues for criticism, uptake of criticism, public standards, and tempered equality of intellectual authority.

Rick Creath (personal comment) asks whether this might imply a role for history of science and of philosophy. Certainly, contact among different cultures and traditions has facilitated mutual critical interaction in this century; there’s no reason specialists in reconstructing the thought of another time should not generate further bases for critical interactions.
The notion of “epistemic acceptability” (akin to justifiability) incorporates both the traditional empiricist norm of justification by empirical data and the social norms applying to those discursive interactions constitutive of reasoning. The analysis it makes possible differs from traditional philosophical views, but it also differs from the sociological view by introducing norms both of success and of procedure. The concept of epistemic acceptability and that of conformation discussed in the previous chapter can be brought together to provide definitions of knowledge in each of the senses of knowledge distinguished in the prior chapter. One can think of these definitions as applying only to scientific knowledge, if one takes the restrictive view of the arguments for sociality, or as applicable to knowledge generally, if one takes the unrestricted view of those arguments.

Knowledge as Content

A given content, A, accepted by members of C counts as knowledge for C if A conforms to its intended object(s) (sufficiently to enable members of C to carry out their projects with respect to that/these object(s)) and A is epistemically acceptable in C.

The content may be embodied and expressed in many ways. It may be embodied in internal (mental) representations (however these are understood) as well as in representations in books, journals, and other media of content storage. It may be propositional in form, but also visual or kinesthetic, as in diagrams and other images, three-dimensional models, instruments, and the products of visualizing instruments, such as electron micrographs or PET scans. Knowledge is social in two ways: the status of epistemic acceptability is acquired through social interactions concerning the content carried out in contexts characterized by the conditions of effective criticism; and the projects or goals of C with respect to a given set of objects or processes will be determined in the same way. Conformation is a matter of being in one or another of the semantic relations included in the family. We find out whether some content A conforms by carrying out experiments, making predictions, and the like—that is, by finding out whether accepting A enables us to carry out our projects in the domain A is about.

Knowledge-Productive Practices

Processes and practices of content construction and acceptance, such as those comprised in the categories of observation and reasoning, are knowledge-productive practices in C, if, when engaged in by members of C, they tend to result in the production or adoption of epistemically acceptable content that conforms to its (intended) objects sufficiently to enable members of C to carry out their projects with respect to those objects.

Processes and practices that repeatedly fail to result in content that conforms sufficiently will be rejected by a community whose interest is in accounts of natural and social processes that enable dependably successful interaction with those processes. And if a community does not reject them, then this is grounds for saying that, in engaging in those practices, they are seeking something other than knowledge.

Knowledge as Attribute (or Relation)

Philosophy of science has traditionally been concerned with analysis of the practice and content senses of knowledge just defined, and not with knowledge as an attribute or relation of some individual or collective of individuals. One reason for this disjunction is that the content of knowledge in the attributive sense is usually something that can be expressed in a single statement or small number of them. It is the sort of thing one tests in an examination. Someone can be said to know that the boiling point of water is 212 degrees Fahrenheit or that the pressure and volume of a gas at constant temperature are inversely related. But, in a scientific context, we are less interested in this kind of knowledge than in knowledge of how these parts of an account relate to other parts of an account. When ascribing knowledge to others in science, it is a different kind of knowledge than singular propositional knowledge that is usually at issue. One attributes knowledge of a theory or model, as in “She really knows gauge theory, she’s the one for your team” or sometimes of an organism or kind of system, as in “She really knows C. elegans” or “She really knows symbiosis.” The diehard may insist that all this amounts to lots of
singular propositional knowledge. But it is more than knowledge that \( p \) for many \( p \), that is involved here. What is important, and what is valued, in this individual's knowledge is her appreciation of the relationships between all the components of her knowledge. What we value in knowing *qua* scientific knowing is less knowing that something is the case than understanding it.\(^7\) Nevertheless, a definition of the ascriptive or relational sense can be given that follows the traditional pattern but incorporates the kinds of norms of success and procedure invoked here. The version I provide utilizes the notion of responding to criticism.

\[ S \text{ knows that } p \text{ if} \]

\[ \begin{align*}
\text{i. } & S \text{ accepts } p, \\
\text{ii. } & p \text{ (or } p \text{ conforms to its intended object sufficiently)} \\
\text{iii. } & S's \text{ response to contextually appropriate criticism of } p \text{ or of } S's \text{ accepting } p
\end{align*} \]

is or would be epistemically acceptable in \( C \) (i.e., \( S's \) response does or would satisfy standards adopted by \( C \), and would itself be so evaluated by the relevant subgroup of \( C \), in situations characterized by the conditions of effective criticism).

These definitions do relativize the various concepts of knowledge to communities, but those communities must themselves satisfy certain conditions in order that the cognitive activities occurring within them qualify as knowledge or as knowledge-producing. This relativization is not equivalent to traditional relativism, which (1) relativizes knowledge and epistemic acceptability not just to individuals, but to individuals' opinions at any single moment, and, consequently, (2) treats all beliefs as epistemologically equivalent. It constitutes instead a contextualization of knowledge. From a perspective of monism and individualism, contextualism is difficult to distinguish from relativism, but from a nondichotomizing perspective that accepts plurality and provisionality as features of the knowledge of empirical subjects, contextuality is the nonrelativist position.

Both the notions of conformation and the norms of epistemic acceptability distinguish these definitions from their dichotomizing counterparts, whether sociological or philosophical. Conformation introduces an element of constraint by and responsiveness to the world, but not so tight as to exclude nonreconcilable alternative accounts. The norms of effective criticism introduce constraints on procedure but locate the grounds of these constraints in certain kinds of social interactions. The challenge of the sociologist was that philosophical epistemology was concerned with idealized knowers and hence irrelevant to empirical knowers, who are socially, historically, and geographically located. The challenge of the philosopher was that sociological attention to empirical knowers obliterated the distinction between knowledge and opinion, between sound and unsound science. These challenges are two sides of the same dichotomizing coin. Disable the dichotomies, and both can be met. The challenge of the sociologist is met by showing that the epistemological concerns of philosophers can be articulated for the empirical subjects that the sociologist studies without idealizing the process beyond recognition. The challenge of the philosopher is met by showing that norms *can* be articulated for social processes. Satisfaction of these norms does not mean that knowledge is purged of the social (as required by the philosophical accounts discussed in chapter 3) but rather that social processes and practices can be distinguished as to their success in producing knowledge. So, it is possible to distinguish between epistemically good and bad science, without denying that good science is or can be significantly affected by a variety of social factors.

The dichotomizers each propose one kind of principle as either explanatory or justificatory or both. Philosophical dichotomizers propose logic and a basic empiricism. Sociological dichotomizers propose social interaction. The former operate as norms, the latter as causal explanation. What each proposes is necessary, but neither is sufficient either as explanation or as justification. Logic and observation alone underdetermine, social interaction alone (where it determines at all) overdetermines. Both logic and observational evidence *and* social interactions are required. Reasoning, sensory experience, and social interaction are involved in the generation and justification of scientific content, and the satisfaction of the norms appropriate to those processes and practices is necessary for the ascription of knowledge. While norms of each kind fail sufficiently to constrain what can count as epistemically acceptable, in concert they are quite binding. This account, therefore, does not dismiss the work done by philosophers of science concerned with elaborating principles of reasoning or of evidence. Indeed, it presupposes basic empiricism and logical norms, while remaining neutral with respect to ongoing philosophical debates (e.g., the status of Bayesianism, multi-valued logics). Instead, it proposes to add to those norms traditionally studied by philosophers (and about whose precise nature there is ongoing disagreement) those that (ought to) govern the social interactions also partially constitutive of scientific knowledge. Thus can underdetermination be countenanced without collapsing into relativism.\(^7\) Not only is the dichotomy between the rational and the social dissolved, but the (post-

\[ \text{\textsuperscript{27} This may be one of the reasons why twentieth-century analytic epistemology and philosophy of science have seemed to have little mutual relevance.} \]}\]
or nondichotomous) definitions constitute elements of an account of knowledge that can withstand the skeptical challenge to philosophy from the social and cultural analysts of science.

KNOWLEDGE, PLURALISM, AND PROVISIONALITY

I suggested guidelines in chapter 4 that an account of knowledge might observe in order to avoid dependence on the truth of a metaphysical position or to foreclose possibilities that may be in nature. How does the social account of knowledge and inquiry I am advocating satisfy those guidelines? I restate them here, presenting them in reverse order.

1. A suitable humility requires a modest epistemology. An epistemology—as a theory of human knowledge—ought not to promise complete knowledge (or trade in other absolutes, like certainty) but ought to give sense to the distinctions and normative judgments that are a part of epistemic discourse.

The epistemological approach I’ve outlined does not trade in absolutes but it does, through the notion of criticism and the criteria for epistemically effective criticism, preserve the meaningfulness of distinctions like that between justified and unjustified belief or between knowledge and opinion.

2. A satisfactory epistemology should be open to theoretical plurality or theoretical unity being the final result of inquiry.

The approach I advocate requires pluralism for the conduct of inquiry, but a characteristic of a process need not be a characteristic of its product. This approach does not preclude the possibility of some single all-encompassing theory being the final result of inquiry nor does it preclude the possibility of multiple accounts. It is logically possible that all social and material cognitive interactions should come to an end in a single unified story, but also possible that they should end with a multiplicity of partially overlapping, but noncongruent, accounts.

3. The issue of theoretical pluralism ought not be decided by one’s choice of epistemology.

The kind of social epistemology I advocate does not commit itself to any metaphysics of nature. It is an approach compatible with pluralism or monism as views about the final outcome of inquiry. Of course, particular communities may be committed to metaphysical assumptions, but these are not the same as a general social epistemology being so committed. Similarly, as we have seen, philosophers may use metaphysical assumptions implicitly in their development of a general epistemology. This is, I have argued, a flaw in such accounts, because it requires an argument for the metaphysics as part of the support of the epistemology.

4. The plurality of representations in biology (or any other science) may be a function of how the world is or of human intellectual equipment for and interests in understanding the world. Our epistemology cannot dictate which.

The epistemological approach advocated here is neutral as to whether plurality results from human intellectual capacity and interests or from features of the world we seek to understand. As several later instances will show, some cases of plurality in biology are best understood as a function of diverse human cognitive interests, others as a function of how the world is. I take it to be a virtue of the approach to knowledge developed here that none of these substantive questions concerning pluralism is preempted by the approach but remains to be decided by other forms of analysis or by time.

The account of knowledge I have offered is neither metaphysically antirealist nor epistemologically relativist. It does not, however, suppose that there is a single way nature is that writes itself into our representations provided we get our methodology just right. It is not committed to monism. Our modes of representation will pick out aspects of nature and natural processes with greater or lesser degrees of success. Nature may be so complex that it is impossible for any single account of a given process to represent fully all the factors that make a difference to the precise course of the process. On the other hand, it may be possible that, in the long run, a unified complete representation of nature will emerge from the processes of inquiry. On the account developed here, we do not have to wait that long to claim knowledge.

One of the difficulties in discussions of realism in philosophy of science is that metaphysical realism is often conflated with scientific realism. The problem of realism in the philosophy of science has traditionally centered on the distinction between observables and nonobservables. An antirealist like Bas van Fraassen holds that empiricism does not license an inference from the truth of statements about observable entities and processes to the truth of statements about nonobservable entities, states, and processes; and that science aims not at truth but at empirical adequacy. A realist like Jarrett Leplin or Richard Boyd holds that the success of science can only be explained by the literal truth of scientific theories. These positions are not the metaphysical positions of realism or idealism but are rather positions about the semantics of theories, the aims of science, and what one’s epistemological theory says and does not license. Thus, one can be a realist in the sense of holding that

13 See Rediels 1998.
there is a world independently of our thinking that there is one, without being a scientific realist in the sense of holding that the success of our best theories consists in the world having exactly the features attributed to it by those theories. Whether the success of science demands that the world have the features attributed to it by successful theories depends on the aims of science. But there is not a single aim of science. There are multiple aims. A community of scientific realists will hold that one of the aims must be the true representation of the structure(s) of nature. A community of scientific antirealists or of pragmatists will hold that empirically adequate or useful content is a sufficient aim.\footnote{To the extent that each may be a source of methodological criticism, the arguments of the previous section suggest one might want to have representatives of both perspectives in one’s scientific community.}

A different but analogous distinction to the observable-nonobservable distinction is in play in debates between pluralists and monists. This is a distinction between the simplified systems that we construct for investigation and the natural systems they stand for. Monists hold that science aims at a unified picture of the natural world, and that the ultimate success of science will consist in the production of that picture. This assumes that the simplified systems constructed for investigation each fully captures the causal processes of a given natural system and that all models or theories of such systems can be integrated into a single coherent theory or conjunction of theories. In practice, the systems constructed for investigation by particular theoretical approaches both diverge from one another as those approaches become more empirically successful and overlap one another in their application to the natural world. Pluralists hold that the complexity of the natural world is such that a single unified picture of the world is not possible. Although monism seems more congenial to realism, both pluralists and monists are realists. Indeed pluralism only has the bite it has in the context of realism. Various kinds of realism have been proposed to accommodate pluralism: minimal realism,\footnote{Longino 1990.} tempered realism,\footnote{Waters 1991.} promiscuous realism,\footnote{Dupré 1993.} relational realism,\footnote{Rediker 1998.} and perspectival realism.\footnote{Giere 1999.} What these realisms have in common is a resistance to treating the susceptibility of our singular world to multiple adequate representations as grounds for metaphysical idealism or for epistemological skepticism.

There seem to be at least two choices one can make about the use of the term “knowledge” in a dynamic and social epistemology of this sort.

If one wants to count a community’s acceptable theories as knowledge, one can treat knowledge as provisional, partial, and context-dependent. But those uneasy with the instability this permits may propose to disqualify the provisional and the context-dependent as objects of knowledge. This might eliminate theories and models as objects of knowledge, leaving as knowable only the observational data that support them. This criterion is even more draconian than it seems, however. While the observed or measured character of data warrants a different level of doxastic confidence, there are still important senses in which the data, too, are provisional and partial. Different theoretical frameworks can make different data, different descriptions of data, or different aspects or measurements of the data salient. Different statements about observation will be meaningful and relevant in different theoretical contexts. Some descriptions of celestial bodies, while salient to sixteenth-century astronomers and twentieth-century navigators, are of no interest to twentieth-century astronomers. The measurements of contemporary astronomers may prove to be as useless to astronomers of the twenty-fifth century as the earlier measurements are to the former. Systems of measurement change, criteria of good measurement change, the kinds of relationships among data that are important change, the kinds of data that are important change. So if, as on the first alternative, we deny that the provisional counts as knowledge, then little or nothing remains as knowledge. Because we cannot predict which of our beliefs will persist and which wither into disuse, almost nothing we count as knowledge can be ascribed permanence or certainty. The temptations to legislate provisionality away and the untoward consequences of doing so suggest that our concept of knowledge contains elements that are brought into tension with each other when confronted with science and its history. This tension necessitates decisions of a semantic nature that will utilize some aspects of the concept at the expense of others.

\textbf{CONCLUSION}

The socialized account of knowledge integrates, rather than dichotomizes, the rationality and sociality that are equally aspects of knowledge. The central normative notions are epistemic acceptability and conformation (of content to its intended object to a degree sufficient to enable the realization of projects with respect to that object). These involve both traditional evidential norms and the community norms of effective critical interaction (criticism, for short). Epistemic acceptability for a community C is defined in terms of experiential evidence (data), logic, and the satisfaction by C of the conditions of effective criticism. Conforma-
tion is a general semantic notion of which truth is a special case. Given
that conformation can be achieved in many different ways, it requires
anchoring. The anchoring is provided by the community’s projects with
respect to the intended object of the representation. These projects (and
judgments regarding the degree of conformation necessary) must be (or
must be such as would be) endorsed by C in contexts satisfying the
norms of effective criticism. Sociality and contextuality are pervasive in
the account of knowledge. Plurality, provisionality, and partiality will be
features of knowledge of any degree of complexity in the short term and
possibly, but not necessarily, in the long term as well. On the account I
am offering it is not necessary to wait for the ever receding long term
correctly to claim or ascribe knowledge. While I see this as an advantage
of the account, others may be less persuaded. In the next chapter I take
up a series of questions and objections that have been raised against the
kind of analysis I am offering here.

CHAPTER SEVEN

Clarifications and Responses

The social account of scientific knowledge thus far raises several kinds of
question or objection, including the role of individuals and the relation
of cognitive individuals to cognitive communities. Another kind of objec-
tion charges that the distinctions that it is the job of epistemology to
clarify just get obliterated in the social account, at least in the kind of
social account offered here. A third question concerns the relation of the
account proposed here to alternative approaches to social (or socially
sensitive) epistemology. The distinctions and redefinitions of the pre-
vious chapters offer ways of addressing these issues.

INDIVIDUALS WITHOUT INDIVIDUALISM

The role claimed for critical interaction in knowledge has required the
articulation of norms directed at community structures and processes
rather than at individuals. One of these, public standards, stipulates that
what holds a community together must be public. Adherence to some set
of standards as regulative of cognitive endeavours constitutes a cognitive
community out of a set of individuals. These standards define a set of
cognitive goals, and of practical ends to which the cognitive are related.
In addition, they specify criteria for satisfying these goals, including
methodological procedures, tolerable error limits, as well as substantive
assumptions about the domains under investigation that must be pre-
served in any model or theory of them. All of these elements of a set of
standards can change, and there can be subsets and nested sets corre-
sponding to subcommunities within a broader community. These stan-
dards, which probably encompass everything discussed as methodology
by philosophers of science and more, are not well viewed as rules individ-
uals follow in constructing their contributions to the whole. Rather, they
are better understood as touchstones of critical interaction—principles to
which community members profess (literally or behaviorally) allegiance,
and which therefore ground their critical discursive interactions.

A cognitive community is any group bound by some set of common
goals and shared public standards regulating critical (knowledge-produc-
tive) discourse and the stabilization of representations as knowledge.
Given that every group organized for some purpose, whether for govern-