Social Empiricism

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Preface

Social Empiricism took shape over a long period of time, and with the influence and support of a number of people and institutions. My former colleagues at the University of Cincinnati supported my return to philosophy of science, after a useful graduate school detour in the history of analytic philosophy. A Mellon Postdoctoral Fellowship at the University of Pennsylvania in 1990–91 gave me the time and the environment to learn about new developments in the cognitive sciences and in science studies. As a participant in the NEH Summer Institute, "Science as a Cultural Process" in summer 1991, I read widely in science studies and feminist science criticism, and had the opportunity to argue with authors. It was then that I saw the emptiness of the polarized debates between social constructivism and traditional empiricism, and looked for the emergence of some new, more complex ideas.

During the early 1990s, it became clear that full development of social empiricism would require several careful historical case studies. The first case study that I did—the history of continental drift and plate tectonics—was suggestive, but not enough. I chose to spend a year on an NEH Fellowship for University Teachers (1994–5) at the Dibner Institute (MIT), which made up the difference in my salary and gave me the opportunity to learn from other Fellows working in the history of science. During that year, in my office overlooking the Charles River and the Boston skyline, I began writing this book.

Temple University, my academic home since 1991, has supported my work with several summer grants and a semester of research and study leave (Fall 1996). *Social Empiricism* was both delayed and improved by several sideline projects in cognitive science, feminist philosophy of science and bioethics. The impending arrival of my daughter, Amira (born on April 6, 2000), gave me the incentive to finish the manuscript and submit it for publication.

I could not have written this book without the help of a number of colleagues, friends and graduate students, who read portions of the manuscript, answered factual questions, debated broad issues and fine points, and provided continuous moral support. In particular, I thank (in alphabetical order) Liz Anderson, Sid Axinn, Dick Burian, Stephen Downes, Chuck Dyke, Gary Ebbs, Moti Feingold, Samuel Freeman, Ronald Giere, Alvin Goldman, Gary Hardcastle, Jonathan Harwood, Gary Hatfield, Cliff Hooker, Mark Kaplan, Donna Keren, Philip Kitcher, Hilary Kornblith, Hugh Lacey, James Maffie, Mimi Marinucci, Nancy McHugh, Linda Weiner Morris, Ted Morris, Nick Pappas, Ellen Peel, Joan Richards, Alan Richardson, Bob Richardson, Marya Schechtman, Fred Schmitt, Paul Thagard, Jerry Vision, Joan Weiner and Alison Wylie. I have benefited from the generous comments of my audiences at talks at various universities and conferences. Referees of this book made useful criticisms, corrections and comments. My husband, John Clarke, cared about this project as if it was his own, and gave me both love and time.

The material in chapters 4–7 of this book is developed from previously published work. I am grateful to the publishers of the following articles for permission to include some ideas and short excerpts from them:

Solomon, M. (1994). "Social Empiricism." *Noûs* 28, no. 3: 325–343.

Solomon, M. (1994). "A More Social Epistemology." In Fred Schmitt (ed.), *Socializing Epistemology*, pp. 217–233. Lanham: Rowman and Littlefield.

Solomon, M. (1994). "Multivariate Models of Scientific Change." *PSA 1994,* vol. 2, pp. 287–297.

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Solomon, M. (1998). "Happily Ever After with Consensus?" *Feneomenologia e societa* 21, no. 1: 58–65.

Solomon, M. (2001). "It *Isn't* the Thought That Counts." *Argumentation* 15, no. 1, pp. 67–75.

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Introduction

If you turn your back on Mishnory and walk away from it, you are still on the Mishnory road.

Ursula LeGuin, *The Left Hand of Darkness*

Two disputed claims have been at the core of discussions about scientific change for the last forty years. The first is that scientists reason *rationally;* the second is that science is *progressive.* For about thirty years, the controversies were cross-disciplinary, led by philosophers on one side and sociologists of scientific knowledge on the other. Discussions were polarized, with philosophers defending traditional Enlightenment ideas about rationality and progress while sociologists of scientific knowledge (often abbreviated SSK), who extrapolated some themes in Kuhn's work (1970 [1962]), espoused relativism and constructivism.[1] Historians and others in science studies were sometimes guided by philosophers, sometimes by sociologists of scientific knowledge and sometimes, in two-voiced narratives, by both.[2]

Recently—over the past ten years or so—the debate has become less polarized. On both sides, careful examination of case studies has led to new ideas about rationality and progress. Creative ideas have come from science studies disciplines in addition to philosophy and sociology of scientific knowledge, such as history of science, feminist science criticism, psychology of science and ethnographic studies. The result is not a compromise or a midpoint between two extreme positions; it is the beginning of a fresh understanding of scientific change.

Social Empiricism is inspired by and addressed to this new community of ideas. The goal is a systematic new epistemology of science. This new epistemology is not intended to be the last word on scientific change. Rather, it claims to move beyond piecemeal new insights and remnants of past unworkable positions, advancing the debate to the next stage.

This introduction begins with an analysis of the immediate post-Kuhnian debate between philosophers of science and sociologists of scientific knowledge. Then it surveys the more recent suggestions, showing how they reject the framework of the original debate. The introduction ends with an outline of central themes in this book, organized by chapter contents.

In my view, philosophers of science and sociologists of scientific knowledge had *common* assumptions about the nature of scientific rationality and scientific progress. They disagreed about whether scientists reason rationally and whether scientific change is progressive, but they had shared standards of evaluation. For all, the framework of debate was Enlightenment epistemology. Descriptions of this framework and the opposed positions of traditional philosophers of science (e.g., Hempel, Lakatos, Laudan, Glymour and some early work in naturalistic philosophy of science such as Giere 1988, Thagard 1988) and sociologists of scientific knowledge (e.g., Barnes, Bloor, Collins, Pinch, Woolgar) give evidence for this interpretation.

Some of the shared assumptions about the nature of rationality are as follows:

- *Individualism* Rational thinking is thought of as an ability attributable to individual human beings and a property of their decisions. Scientific rationality, which is one kind of rationality,[3] is thought of as a property attributable to the decisions of individual scientists and typically exercised by those scientists (e.g., Galileo, Newton, Darwin, Einstein) historically credited with progressive scientific change. The term "rational" is not applied to groups or communities, except derivatively (a rational community is composed of rational individuals). The debate between traditional philosophers of science and sociologists of scientific knowledge concerns how *frequently* individual scientists reason rationally; the former think irrationality is unusual, while the latter think that rational reasoning is unusual. For example, Larry Laudan's well-known "arationality assumption" (see, e.g., Laudan 1987, p. 202) recommends that non-rational causes of belief be looked for only in the (according to him) rare case when there are insufficient rational reasons for a scientist's decisions. Naturalistic philosophers (e.g., Giere 1988) continued to argue for individualism by claiming that it is cognitive (i.e., individual psychological) factors, rather than social ones, that produce scientific change. Sociologists of scientific knowledge, on the other hand, have inherited Barnes and Bloor's (1982) "equivalence postulate," which claims to find that all beliefs—plausible and implausible, apparently well justified and not, true and false—have similar causes in human interests. All look at the causes of individual beliefs and draw conclusions about individual rationality.

- *Objectivity (including "cold" cognition)* It is assumed that scientific rationality is thinking that is free of motivational and ideological bias. During the 1980s, some philosophers of science (e.g., Giere 1988, Thagard 1989) put a naturalistic gloss on this assumption, claiming that scientific rationality is "cold cognitive processing," i.e., information processing motivated only by the desire for "truth" or other purely cognitive ends (e.g., good models, predictive success, explanatory success). They looked to the newly flourishing science of cognitive psychology to find out what the processes are. Sociologists of scientific knowledge countered that all scientific decision making is motivated by personal or social goals (i.e., is "hot" cognitive): they find that factors such as self-interest, the influence of authority, peer pressure, pride, conservatism, ideology, enthusiasm for new technology and funding practices determine individual scientists' decisions. Because they also identify rationality with "cold cognition" and consequent freedom from motivational and ideological bias, they conclude that scientific decision making is not rational. Latour's ten year moratorium on cognitive studies of science (1987, p. 247) was an attempt to focus attention on the importance of other causes of belief.
- *Method* It is assumed that scientific rationality requires following explicit or implicit rules of reasoning—such as probabilistic reasoning, rules of confirmation and disconfirmation, measurements of problem solving ability or explanatory power, and heuristics such as the principle of parsimony. Traditional philosophers of science (e.g., Glymour, Laudan) still

aim to produce such rules, while sociologists of scientific knowledge argue (following Kuhn and Feyerabend) that the project is hopeless because successful scientists do not follow rules.

• *Generality* It is assumed that all scientifically rational work has in common the *same* scientific method. That is, scientific method is a *general* property of rational scientific decision making. The generality assumption lies behind the usual reading of Feyerabend's (1975, p. 23) provocative statement that "Anything goes": Feyerabend finds cases where suggested rules of reasoning fail, and then declares that there is no scientific method. This reading of Feyerabend's position[4] was taken up by SSK. Traditional naturalistic epistemologists, on the other hand, continued with their positive elaboration of the generality assumption, e.g., when Kitcher (1993, p. 10) devotes himself to "the task of recognizing the general features of the scientific enterprise."

There is also a trend among philosophers, encouraged by the writing of senior philosophers of science such as Hacking (1983) and Fine (1986), to eschew questions about scientific method and focus instead on ontological questions. For example, Hacking writes, "The 'rationality' studied by philosophers of science holds as little charm for me as it does for Feyerabend. Reality is more fun . . ." (1983, p. 16). Their reasons for this preference for the SSK reaction to Feyerabend are not obvious. While there are good reasons to abandon the hope for a general, algorithmic scientific method, there is every reason to expect the same kinds of insights for epistemological questions as Hacking and Fine expect for ontological questions, i.e., piecemeal, domain specific and historically contingent.

• *All-or-nothing* The traditional assumption about rationality is that it is an all-or-nothing property: one failure to exercise proper scientific method jeopardizes the rationality of the entire decision process of which it is a part. If the causes of a decision are 90% "scientific" and 10% "social," the jury on rationality is still out. Rationality is not measured in degrees. The insistence of the sociologist of scientific knowledge that *all* scientific change is social is, really, a claim that social (= non-rational) factors substantially influence all decision making and, therefore (assuming the traditional all-or-nothing view about rationality), that there is no scientific rationality. The traditional philosophical defense of rationality is the claim that social influences on decision making rarely or never affect the outcome (e.g., Laudan 1997, Thagard 1989). I call this the "Ivory Soap" model of scientific rationality, because the claim is that reasoning is pure enough to be considered epistemologically pure (99.44 percent purity is advertised as pure enough for bathroom soap).

Some shared assumptions about the nature of scientific progress are as follows:

• *Truth* The most widespread traditional view is that truth is the goal of science, and that science progresses by accumulating truths (or, at least, partial or approximate truths, or representations, or models resembling the world). Sociologists of scientific knowledge who find that there is scientific change without increased truth are skeptical of any claim that there is cumulative progress in science. (To avoid a pointless equivocation about the word "truth" here, I do not use it for beliefs which are socially constructed in a manner that is epistemically arbitrary.)

• *Objective* One way in which the progress of science can be objective is if it consists of the accumulation of truths. Such truths are not socially constructed or negotiated, nor are they relative to the theory under consideration. Other measures of progress (e.g., predictive success, explanatory success) can also be objective, so long as they do not result in judgments that are relative to person, social group or theory under consideration. The minority of traditional philosophers who are not realists (e.g., Laudan, Van Fraassen, even Kuhn on some readings) typically claim this kind of measure of objective progress. Sociologists of scientific knowledge are skeptical of *any* measures of objective progress. (Note that the term "objective" is also used in a related sense—interchangeably with "rational" and usually meaning "cold cognitive"—when describing the methods of science, above.)

• *Consensus* The traditional view about scientific progress is that scientific change is marked by consensus; significant change happens when one theory, or a set of competing theories, is replaced by *one* theory. Traditional philosophers talk of *rational* causes of consensus (e.g., overwhelming evidence for one theory) while sociologists of scientific knowledge talk of the *social and political* causes of consensus (e.g., the effect of the

Restoration on the acceptance of experimental methods in seventeenth century England).

- *Linguistic* The linguistic products of scientific activity are: explicitly stated explanations (which may be set out in traditional deductive form), propositions that claim to correspond to the world, derived predictions that claim to match observation reports. Traditional philosophers of science (e.g., Hempel, Laudan, Kitcher, Van Fraassen[5]) measure scientific progress in terms of these linguistic products. Sociologists of scientific knowledge argue that these products are not the objective achievements they appear to be, and go on to be skeptical about scientific progress altogether.
- *"Pure" science* Traditional philosophers of science claim that there is a clear demarcation between "pure" science and "applied" science (technology). There can be progress in one enterprise without progress in the other. Typically, "pure" and "applied" science are distinguished by having different goals of inquiry (e.g., truth versus building bridges). Sociologists of scientific knowledge deny that this clear demarcation is possible, and give up *any* distinction between science and technology. Latour (1987) coined the term "technoscience" to signify this.

Thus, shared assumptions about the nature of rationality and progress led to mirror image views of the nature of scientific change. Traditional philosophers of science and sociologists of scientific knowledge are closer to one another than they often believe. Their relationship is like that of authoritarian parent to rebellious child. The child (SSK) reacts against the parent (traditional philosophy of science) without changing the framework assumed by the debate.

A good deal of this post-Kuhnian exchange continues. Indeed, Pickering complains that "much of the work currently being done in science studies has remained stuck in the place where SSK left it in the early 1980s" (1995, p. 27). Since the late 1980s, however, new ideas that reject Enlightenment assumptions about the nature of rationality and progress are being widely produced. In fact, almost every assumption mentioned above has been jettisoned by at least one philosopher, historian, sociologist, feminist critic or anthropologist, and most by more than one. A quick survey gives examples of these developments.

First, on the nature of rationality:

- *Individualism* Social epistemology—a delayed response to both Kuhn and Wittgenstein (see Solomon 1996)—rejects the assumption of individualism. Philosophers of science such as Giere (1988), Hull (1988), Kitcher (1993), Solomon (1992) and Thagard (1993) have assessed scientific rationality during times of dissent from a social perspective. Instead of assessing the rationality of individual reasons and causes of belief, they assess the resultant distribution of cognitive effort by considering whether competing theories are each getting a fair share of investigation. Feminist critics, such as Haraway (1991), Harding (1991), Longino (1990) and Nelson (1990), have urged a more thoroughgoing social assessment of rationality. Longino in particular has specified four social requirements for rational scientific decision making (see 1990, chapter 4).
- *Objectivity* Giving up the assumption of individualism helps make possible the rejection of the assumption of objectivity in reasoning. This is because what matters for science is not the individual causes of belief (which may have motivational, ideological or even cognitive bias) but the resultant state of the scientific community, especially the distribution of research effort. Indeed, Kitcher (1993, chapter 8) acknowledges the role of decisions motivated by the desire for credit in rational scientific change. As he writes (p. 305), "particular kinds of social arrangements make good epistemic use of the grubbiest motives." I argued (1992) on similar grounds that "cold" cognitive processes are ipso facto no more "rational" than "hot" cognitive or even non-cognitive processes. Some feminist critics go further here, redefining the term "objectivity" (for clarity, Haraway and Harding call it "strong objectivity") in terms of particular political points of view, or specified social conditions that are especially conducive to a wide-ranging distribution of research effort.
- *Method* Kuhn (1977) proposed a non-algorithmic account of scientific method. Kuhn explicitly rejects the assumption of both traditional philosophers of science and sociologists of scientific knowledge that if there is a scientific method, it will be precise enough that there can be agreement on the promising direction(s) of research to take when following the method. Kuhn thinks that there can be room for disagreement among experts, but not so much disagreement that decisions are

arbitrary. This positive view of Kuhn's has been reiterated recently, at least in general terms. For example, Pickering (1995, p. 32) writes, "On my analysis of practice, it is far from the case in science that 'anything goes'." Surprisingly, no one has developed Kuhn's account of objective but non-algorithmic theory choice.

• *Generality* Naturalistic philosophers (e.g., Goldman 1992, Kitcher 1993, Laudan 1984 and Boyd 1985) often claim that successful methods change as domains change or as science progresses. In other words, they claim that there is not a universal, general scientific method.[6] This philosophical position has been reinforced by Wittgensteinian arguments and by the recent interest in disunity of science claims (e.g., Dupre 1993). Actual studies of locally successful methods are rare, but have been produced recently. For example, Darden (1991) and Bechtel and Richardson (1993) explore and assess heuristics and reasoning strategies in the history of genetics, and Wylie (2000) explores a variety of heuristics in archeological reasoning.

• *All-or-nothing* Longino (1990, p. 76) claims that "Objectivity . . . turns out to be a matter of degree." Specifically, she acknowledges that her four conditions for objectivity—recognized avenues for criticism, shared standards (including, for science, empirical adequacy), community response and equality of intellectual authority—are typically imperfectly or only partly realized. The upshot is that "transformative criticism," which is at the heart of objective practices, comes in degrees of depth and range. For Longino, perfect objectivity is an ideal, and actual practices that do not attain this are still valuable for their partial objectivity. Wylie and Nelson (1998) also argue that objectivity comes in degrees.

Next, new ideas about the nature of scientific progress have come from the pragmatic tradition in philosophy, actor-network theory in sociology of science, and feminist critics. In general, there has been a shift from focus on theory (and thus on linguistic representation, truth, etc.) to a focus on experimental practice (where the emphasis is on what "works," where "works" is, unfortunately, never specified).

• *Truth* Hacking (1983) argues that knowledge about the existence of entities can be robust (firm, dependable) when we can successfully manipulate them. He remains skeptical about truth or even partial truth of theories. Increase in the knowledge of existence of entities, and how to interact with them, constitutes, for Hacking, progress in experimental science. This is "know how," which is not representational knowledge. Sociologists of science and feminist critics have expressed insights similar to Hacking's, in a more democratic voice: entities become known through their actions and resistances to our manipulations as much as through our successful manipulations of them. Latour (1987) and Callon (1986) use the semiotic terminology of "actants" in nature, Pickering (1995) writes of "material agency" and the "resistance of nature" to which we must "accommodate," Galison (1997) finds coordinated action in "trading zones" and Haraway (1991) celebrates the ability of nature to surprise us. For each of these writers, we need to adapt to nature (which they construe in various ways), and sometimes also to coordinate our interactions with one another, to get empirical success. Success, then, is not constructed by social interactions alone.

• *Objective* Apart from the claim that some practices "work," where "work" is not further specified, nothing is said. Even Kuhn's idea (1962) that there is "progress through revolutions" has not been taken up in any new way. As recently as Cartwright (1999) and Psillos (1999) the idea of "empirical success" is taken as unproblematic, and used as a proxy for "objective success," even though no account is given of it.

• *Consensus* Feyerabend's view, echoing Mill, that consensus is both undesirable and avoidable in scientific inquiry, has generally been ignored. Philosophers and sociologists of science typically still treat consensus as the goal of inquiry and as a natural resolution of dissent. Recently, a few feminist critics (notably Haraway 1991 and Longino 1990) have argued that pluralism, with continued dissent, is not an intrinsically unsatisfactory state for scientific knowledge. Rather, continued consideration of several theories acknowledges the partial virtues of all theories. For example, Haraway writes of "the joining of partial views and halting voices into a collective subject position" (1991, p. 196). Dupre's (1993) pluralism and Giere's (1999) "pluralistic perspectival realism" are influenced by Haraway's and Longino's work, especially.

• *Linguistic* Scientific success is not understood as a linguistic product (as a theoretical claim, explanation, explicit prediction) or even as a representation (including the pictorial models suggested by Giere 1988 and Nersessian 1992) but as a successful *practice* (see, for example, work of Galison 1997, Hacking 1983 and Pickering 1995). Of course, theoretical claims and models may play a role in *producing* the successful practice.

- *"Pure" science* There has been no recent change in this discussion.

Social Empiricism takes all these developments further. It has two parts, corresponding to the "social" and the "empiricism" parts of the thesis. Chapters 2 and 3 give a new empiricist account of scientific progress. Most of the book (chapters 4 through 8) develops and applies a new, social account of scientific rationality. Throughout, case studies, primarily in late nineteenth and twentieth century natural science, are used to develop the ideas.

Chapter 2, "Empirical Success," begins an answer to the neglected question, "what is the nature of empirical success?" and the related neglected question, "what is it for some practices to 'work'?" Chapter 2 gathers a range of examples of scientific success, distinguishes empirical success from other kinds of scientific success, and then discusses the robustness, significance and varieties of empirical success. Empirical success can be predictive success, experimental success, observational success, some kinds of technological success and some kinds of explanatory success; in each of these categories the empirical success can be more or less significant. Qualitative assessments of empirical success (such as robustness and significance) suffice for the purposes of this book. Some kinds of technological success do not come with significant empirical success; this point is the basis for a new understanding of the distinction between science and technology. The same is true for some kinds of explanation. Thus the contribution of types of explanatory success to empirical success is also reconsidered.

Truth is the most commonly stated goal of science, especially by scientists. Yet, frequently (as Laudan and others have pointed out), empirically successful scientific theories are substantially false in that most of the entities postulated by them do not exist and/or most of the claims of the theory are incorrect. It is tempting to conclude from this that the only genuinely attainable goal of science is empirical success. Chapter 3, "Whig Realism," argues that truth can still be a genuinely attainable goal of science. Whig realism states that there is typically *something* true about empirically successful theories, although such theories may not be literally true, partially or approximately true, or even good representations. Truth in theories is known, at best, in hindsight. (Empirical success, on the other hand, can be known during early consideration of theories.) Whig realism, unlike most forms of realism, is consistent with pluralism. Moreover, unlike most forms of both realism and antirealism, it has straightforward and reasonable methodological consequences.

In chapter 4 I introduce the terminology of *decision vectors,* which is used for discussions of scientific rationality in chapters 5 through 8. This terminology is epistemically neutral so that I can describe scientific decision making and scientific change without using such terms as "biasing factors," "external factors," "social factors" or "psychological factors," since these terms already come with negative normative associations. A variety of decision vectors, discovered in the various science studies disciplines, are described, in preparation for the work of later chapters. A distinction between empirical and non-empirical decision vectors is discerned, and this distinction will have epistemic importance in later chapters.

Chapters 5 and 6 explore a range of cases of dissent and consensus in science. Decision vectors influence all decisions—during dissent, during consensus formation, and during the dissolution of consensus—to the same extent, in all areas of scientific inquiry. Consensus can take place without general or central causal processes. Distributed models of decision making have recently been described in the artificial intelligence community as well as, more qualitatively, in the actor-network theories of Callon and Latour. These models are useful for understanding the cases of consensus that I describe.

Examples in chapters 2, 3 and 5 show that scientific success is not dependent on consensus, and examples in chapter 6 show that consensus is not always progressive for science. The move from dissent to consensus, therefore, is not in itself of much epistemic significance. The pursuit of empirical success and of truth can be consistent with both dissent and consensus.

Chapters 5 and 6 begin construction of a social epistemology for science, which is developed fully in chapter 7. It is a *more* social epistemology than those currently available. The historical case studies suggest that scientific rationality is socially emergent and not dependent on such conditions as individual clear thinking, rational decision making or reasonable inferences. Instead, it is a matter of having a particular distribution of decision vectors across the scientific community. Scientific communities are not merely the locus of distributed expert knowledge and a resource for criticism, but the site of *distributed decision making*.

To attest to the fact that the distinction between dissent and consensus is not one of much epistemic significance, and that, in particular, consensus is not a superior or a distinctive epistemic state, a normative account of scientific decision making—which I call *social empiricism*—is developed for dissent and then consensus is treated as a special case of dissent (where the amount of dissent approaches zero). *Social empiricism* is then applied to some new cases, and shown to yield useful normative recommendations. In particular, social empiricism is critical of the epistemic goals of the new institution of "consensus conferences" in the medical sciences.

Chapter 8, finally, reflects on the idea of "epistemic fairness" entailed by social empiricism. There is a parallel between epistemic fairness and new conceptions of justice, fairness and democracy in feminist political philosophy (e.g., Young 1990). This parallel reinforces my suggestions about the kinds of normative recommendations that social empiricism can realistically offer. Chapter 8 also considers the relation between social empiricism and the various feminist critiques of scientific inquiry. In my view, feminist critiques of science generally offer the deepest challenge to traditional views about scientific change. Social empiricism has much in common with them. Notably, both have a serious commitment to a naturalized approach to epistemology. I discuss several particular similarities and one important difference between feminist critiques and social empiricism.

Traditional epistemologies, from the time of Plato and Aristotle, and through the contributions of Descartes, Bacon, Newton, Mill and more lately Hempel, Laudan and others, have produced rules and heuristics for individual scientists. Social empiricism, while acknowledging the utility of *some* of these individual guidelines, develops rules and heuristics that are socially applicable. This means that the traditional focus on methods and heuristics to be individually applied by all working scientists is rejected. Instead, the normative emphasis is on science funding, administration and policy.

The case studies chosen throughout this book come mostly from late nineteenth and twentieth century science, and cover a range of sciences, from the physical sciences to the life sciences to the empirical social sciences. I have three reasons for these choices. First, I want to forestall any criticisms of the kind "what you say is applicable only to physics" or "what you say is applicable only to the 'softer' sciences" or "geology is an unusual case that does not generalize." Secondly, the scientific enterprise has changed over its long history, and it is likely that its epistemic character has also changed, especially with and since the Scientific Revolution of the seventeenth century. While it is plausible that my conclusions apply to all modern science (i.e., post-Scientific Revolution), I am being cautious and restricting my conclusions to a shorter historical period, i.e., recent and contemporary science. Finally, my purposes are practical rather than historical or abstractly philosophical. My goal is to positively affect scientific decision making through practical social recommendations. For this reason, I focus my investigations on the past 100 or so years, including contemporary as well as recent historical cases, so that there will be no questions about relevance to contemporary issues.

Quine famously regarded science as an extension of common sense (1966, p. 229). For him, and many others, answers to epistemological and ontological questions about science can come from reflection on ordinary practices. Even Pickering (1995, p. 6) stresses the continuities of science with the ordinary: "Much of everyday life . . . has this character of coping with material agency, agency that comes at us from outside the human realm and that cannot be reduced to anything within that realm." Certainly, general epistemology and philosophy of science have positively influenced one another. Yet, philosophy of science is not epistemology. Science may have developed from ordinary practices, but it has developed distinctive practices, particular goals and unique forms of social organization. Social empiricism is an epistemology for science.