Chapter One

TWO PUZZLES ABOUT SCIENCE: REFLECTIONS ON SOME CRISES IN PHILOSOPHY AND SOCIOLOGY OF SCIENCE

Science has posed a plethora of interesting challenges to several of the major philosophers and sociologists of the past half century. Indeed, trying to understand and to explain the workings of science has preoccupied several of the leading thinkers in these otherwise disparate fields. This book is an effort to help resolve a few of those challenges. But before I can expect my solutions to be taken seriously, I need to show that the problems I am grappling with are both real and as yet unresolved. I know no better way of motivating problems than by a brief survey of their recent history, a history that, in this particular instance, involves some intriguing intersections between the concerns of philosophers and sociologists.

During the 1940s and 1950s each of these disciplines developed and elaborated its own picture of how science behaves. The philosophical accounts I have in mind are those of the logical empiricists and Popper; the sociological model is associated chiefly with Merton and his followers. Although there were important differences of emphasis between the philosophical and sociological accounts of science offered by scholars of that generation, their respective pictures—now that we have some distance from them—appear to be quite similar and decidedly complementary. These similarities are less surprising than they might initially appear because, despite occasional outbreaks of rivalry between the two disciplines, both sociologists and philosophers of that era shared a basic premise and a common problem. The premise was that science is culturally unique and to be demarcated sharply from other intellectual pursuits such as philosophy, theology, and aesthetics.[1] The central problem each sought to explain was the impressively high degree of agreement in science. During the 1960s and 1970s, however, the views on these matters held by many sociologists and philosophers of science began to undergo transformation. Gone, or in serious disarray, by the mid-1970s were most of the familiar theses of logical empiricism and Mertonian sociology. In their place came accounts of science radically at odds with their predecessors. But if the newer analyses differed sharply from the old (in ways to be described below), there were still intriguing consiliences between the new philosophical and the new sociological perspectives on science. Chief among these points of common interest shared by "new-wave" accounts of science was a conviction that the central intellectual puzzle about science required explaining the periodic outbursts of disagreement in science.

In a nutshell, students of the development of science, whether sociologists or philosophers, have alternately been preoccupied with explaining consensus in science or with highlighting disagreement and divergence. Those contrasting focuses would be harmless if all they represented were differences of emphasis or interest. After all, no one can fix simultaneously on all sides of any question. What creates the tension is that neither approach has shown itself to have the explanatory resources for dealing with both. More specifically, as we shall see, whatever success can be claimed by each of these models in explaining its own preferred problem is largely negated by its inability to grapple with the core problem of its rivals. Thus, the sociological and philosophical models of science of the 1940s and 1950s adopt such strong assumptions about the consensus-forming mechanisms they postulate to explain agreement that it is difficult to make much sense of the range and character of scientific disagreements and controversies. The more recent models, for all their promise of revealing the manifold

1. Recall, for instance, Karl Popper's remarkable claim dating from this period that the most important task of epistemology was to demarcate between science and non-science.

reasons why scientists might agree to differ, leave us largely in the dark about how scientists could ever reasonably resolve their differences in the definitive fashion in which they often do terminate controversies.

The theme of this essay, in its starkest form, is simply (*a*) that existing accounts lack the explanatory resources to tackle these two puzzles in tandem; (*b*) that this is especially true of several recently fashionable approaches to science, which turn out to be at least as flawed as those they would replace; and (*c*) that we need a single, unified theory of scientific rationality which promises to be able to explain both these striking features about science. My aim in this first chapter is to diagnose how we landed in the mess of being able to explain one or the other of these puzzles, but not both. The remainder of the book delineates some machinery that explains how both consensus and dissensus can arise, and how each can sometimes give rise to the other.

THE CONSENSUAL VIEW AND THE PUZZLE OF AGREEMENT

To anyone working in the humanities or the social sciences, where debate and disagreement between rival factions are pandemic, the natural sciences present a tranquil scene indeed. For the most part, natural scientists working in any field or subfield tend to be in agreement about most of the assertions of their discipline. They will typically agree about many of the central phenomena to be explained and about the broad range of quantitative and experimental techniques appropriate for establishing "factual claims." Beyond this agreement about what is to be explained, there is usually agreement at the deeper level of explanatory and theoretical entities. Chemists, for instance, talk quite happily about atomic structure and subatomic particles. Geologists, at least for now, treat in a matter-of-fact fashion claims about the existence of massive subterranean plates whose motion is thought to produce most of the observable (i.e., surface) tectonic activity—claims that, three decades ago, would have been treated as hopelessly speculative. Biologists agree about the general structure of DNA and about many of the general mechanisms of evolution, even though few can be directly observed. One intuitive yardstick of this staggering degree of agreement emerges from a comparison of science textbooks with texts in, say, philosophy or sociology. (And such comparisons are to the point since it was primarily sociologists and philosophers who, looking carefully at science, were struck by its comparatively high degree of consensus.) Philosophers are notorious for debating fundamentals and there is little agreement between the rival schools or factions of philosophy on anything, not even on which problems are of central importance to the discipline. So it comes as no surprise that philosophy texts written by (say) Thomists have precious little in common with those written by positivists. Similarly, sociology is divided into numerous warring camps, to such a degree that there are glaring divergences among sociology textbooks produced by (say) Marxists, hermeneutists, phenomenologists, functionalists, or sociometricians. Each school of philosophy or sociology offers a fundamentally different agenda of central issues for the field, and each advocates rather different methods for testing or evaluating disciplinary claims. The natural sciences are simply not like that, or at least so many sociologists and philosophers of the 1950s and 1960s surmised.

So impressed were many philosophers and sociologists by the extent of agreement in science that they often took that degree of agreement to be the central, even the defining, epistemic and cognitive feature of science. The well-known philosopher of science, N. R. Campbell, puts it quite bluntly: "Science *is* the study of those judgments concerning which universal agreement can be reached."[2] Speaking for the sociologists, John Ziman concurs: "[Consensus] is the basic principle upon which science is founded. It is not a subsidiary consequence of the 'scientific method.' It is the Scientific Method itself."[3]

What makes the broad degree of agreement in science even more perplexing is the fact that the theories around which consensus forms do themselves rapidly come and go. The high degree of agreement which characterizes science might be less surprising if science, like some monastic religions, had settled upon a body of doctrine which was to be its permanent dogma. Consensus, once reached in those circumstances, might well be expected to sustain itself for a long period of time. But science offers us the remarkable spectacle of a discipline in which older views on many central issues are rapidly and frequently displaced by newer ones, and where nonetheless most members of the scientific community will change horses in midstream to embrace a

2. Campbell, 1952, p. 27. Elsewhere, Campbell wrote: "The subject matter of science consists of those judgements for which universal assent can be obtained" (1957, p. 22).

3. Ziman, 1968, p. 9.

point of view which may never even have been mooted a decade earlier. Moreover, change occurs at a variety of levels. Some of the central problems of the discipline change; the basic explanatory hypotheses shift; and even the rules of investigation slowly evolve. That a consensus can be shaped and reshaped amid such flux is indeed remarkable. However unsatisfactory the models of scientific consensus offered by the last generation may now appear to be, it is surely easy enough to understand why their framers believed that the explanation of scientific agreement must be a central consideration for any theory about how science works. For, when one takes into account the rapid-fire manner in which new views emerge, the staggering thing about science is not that consensus is generally reached so quickly and with such unanimity; what is astounding is that consensus is ever reached at all.

Taking the high level of consensus in science as a datum, intellectuals of the preceding generation constructed models of science, and especially of scientific decision making, which were designed to explain how science differed structurally and methodologically from such ideology-laden fields as social and political theory or metaphysics. I want to describe the salient features of some of those models, for an appreciation of their strengths and weaknesses will be useful later on.

a) *Philosophers and consensus.*—Philosophers of the 1930s and 1940s, turning anew to science after a generation of comparative philosophical neglect by many idealists and Neo-Kantians in the first decades of the twentieth century, already had some sophisticated machinery in their kits for explaining how science could be a consensual activity. Indeed, for a very long time philosophers generally have been inclined to accept what I call the Leibnizian ideal. In brief, the Leibnizian ideal holds that all disputes about matters of fact can be impartially resolved by invoking appropriate rules of evidence. At least since Bacon, most philosophers have believed there to be an algorithm or set of algorithms which would permit any impartial observer to judge the degree to which a certain body of data rendered different explanations of those data true or false, probable or improbable. Philosophers have expressed varying degrees of optimism about whether we now know precisely what those evidentiary rules are. (Mill, for instance, believed that we already had them in hand. Others, more pessimistic, believed that we had yet to develop the full kit.) But whether optimist or pessimist, rationalist or empiricist, most logicians and philosophers of science from the 1930s through the 1950s believed, at least in principle, in the Leibnizian ideal. That they did so had immediate relevance to their views about consensus in science, for science was regarded as consisting entirely in claims about matters of fact. Since scientific disagreements were thought to be, at bottom, disagreements about matters of fact, and since disagreements of that sort were thought to be mechanically resolvable, philosophers had a ready sketch of an explanation for consensus formation in science.

Specifically, they argued that there are rules of scientific methodology which are responsible for producing consensus in a rational community such as science was thought to be. If scientists disagree about the merits of two rival theories, they need only consult the appropriate rules of evidence to see which theory is better supported. Should those rules fail to decide the issue immediately (e.g., should both theories prove to be equally well supported by the available data), then all that was required to terminate disagreement was the collection of new and more discriminating evidence which would differentially confirm or disconfirm one of the theories under consideration. In this view of the matter, scientific disagreement was invariably transitory and unstable. Disagreements about the facts could arise among rational men only when the evidence in a particular domain was relatively thin or incomplete. Once identified, the disagreement could be brought to rational closure by collecting more evidence and by insisting on following the appropriate rules for assessing evidential support. In sum, philosophers preached that science was a consensual activity because scientists (insofar as they were rational) shaped their beliefs, implicitly if not explicitly, according to the canons of a shared "scientific methodology" or "inductive logic," and those canons were thought to be more than sufficient to resolve any genuine disagreement about matters of fact. To this end, many prominent philosophers of science of this period (e.g., Carnap, Reichenbach, and Popper) saw their primary task to be precisely that of explicating the rules of evidential inference which scientists use implicitly in making theory choices.

This explanation of scientific consensus had some very tempting features. In the first place, it accorded neatly with the account that scientists themselves were prone to give of their activity. For decades, scientists had been extolling the virtues of the scientific method and they tended, like the philosophers, to see that method as the engine driving scientists to agreement. This approach also represented the traditional philosophical wisdom in these matters, since regarding science as a rule-governed activity for the generation of beliefs about the physical world had long been the norm among philosophers.

During the 1940s and 1950s, then, most philosophers of science were of one mind in holding that science was characterized cognitively chiefly by its high level of agreement and also in attributing that degree of consensus to the willingness of scientists to submit their opinions to arbitration by an impartial logic of theory appraisal. If there were any nagging worries to upset this picture, they chiefly grew out of the fact that, as many philosophers knew, scientific disagreements did not always or quickly evaporate in the face of new and discriminating evidence. After all, Copernicans and Ptolemaists fought it out for well over a century. Advocates of the wave and particle theories of light were at loggerheads for half of the nineteenth century. Pro- and anti-atomists churned up physics and chemistry more or less steadily from Dalton's *New System of Chemistry* (1810) until the beginning of the twentieth century. The existence of such long-term controversies in science, even recent science, did not by itself refute the Leibnizian ideal, as several face-saving options were open to its defenders. For instance, one could, and some did, argue that scientists are sometimes irrational in the face of the evidence and refuse to recognize the better theory. Such labels were frequently applied, for instance, to Priestley and the phlogistonists or to the opponents of atomism or to the post-Copernican followers of Ptolemy.

If the persistence of some controversies could be attributed to the stubbornness of scientists rather than to the indeterminacy of the rules for theory choice, then the Leibnizian ideal continued to look attractive. Alternatively, and more commonly, it was open to defenders of the Leibnizian ideal to suggest that these long-term controversies were merely *querelles de mots.* According to this view, there was no real difference between the theories of the contending parties (i.e., the theories were empirically equivalent); the disputes persisted only because the contenders failed to recognize the equivalence of their models. Precisely this view was taken in the 1950s by a number of philosophers and historians with respect, for instance, to explaining the prolonged debate between the Ptolemaic and Copernican hypotheses.[4] Elaborate proofs were set out to show that the two systems were "observationally equivalent"; the latent function of these proofs was apparently to show

4. See, for instance, Price, 1959.

that this long-standing controversy was not the refutation of the reigning consensual models and the Leibnizian ideal which it appeared to be. Similar claims were made about the observational equivalence of matrix and wave mechanics and about corpuscular and wave optics. (As we now know, most of these arguments were bogus, for they depended on showing that two theories were equivalent so long as their formal structures—i.e., their mathematical representations—could be shown to be homologous. Unfortunately, these proofs of "empirical equivalence" work only if we divest these theories of most of their substantive claims. But more of that in chapter 5 below.) Thus the philosophical advocates of consensus as the scientific norm could explain away the apparent exceptions to that consensus by insisting that, when consensus was not reached as quickly as one might expect, it was either because the decisive evidence was not sought, or because the scientists concerned did not realize that their rival theories really amounted to the same thing, or (in the last resort) because scientists were not behaving rationally.

Other prominent elements of the philosophy of science of logical empiricism contributed to the impression that science should indeed be a consensual activity. It was commonly asserted, for instance, that one core rule of scientific method was that acceptable new theories must be able to explain all the successes of their predecessors and some new facts as well. Science, in short, was thought to be strictly cumulative. With this strong constraint in place, it became possible to explain how scientific change could be effected fairly quickly. After all, if a new theory emerged which managed to account for everything its predecessor could, and some other things besides, then it would seem that no sensible person could resist the appeal of the new theory. So long as theory change could be said to be strictly cumulative, the philosopher had a ready explanation for the staggeringly swift changes of loyalty which accompany many so-called scientific revolutions. And it is for just this reason that the post-1960s discovery that theory change in science is generally noncumulative and nonconvergent created such acute difficulties for the logical empiricists and for Popper.[5]

b) *Sociologists and scientific consensus.*—If philosophers had a long tradition of expecting and explaining the existence of agreement about

5. For a fuller discussion of the significance of the noncumulativity of scientific theories, see chapter 5 below.

matters of fact, sociologists did not. Indeed, prior to the 1930s there scarcely was a sociology of science worthy of the name. The following two decades, however, saw a significant flowering of sociological studies of science. Central to much of the research of that era are our dual problems of consensus and dissensus. As with the philosophers, sociologists tended to regard the former as the natural state of the physical sciences, whereas the latter required special explanation as a deviation from the expected norm.

Whereas philosophers located the source of the consensual character of science in the scientist's adherence to the canons of a logic of scientific inference, sociologists argued that science exhibited so high a degree of agreement because scientists shared a set of norms or standards which governed the professional life of the scientific community. Robert Merton, for instance, argued that scientific subcultures shared the norms of "universalism, communism, disinterestedness, and organized scepticism."[6] These norms, which are "held to be binding on the man of science, . . . are expressed in the form of prescriptions, proscriptions, preferences and permissions."[7] It is, in short, because scientists share the same values or standards that they are able to form stable patterns of consensus. Merton was later to find what he regarded as strong support for the hypothesis of shared scientific norms and standards in research he did with Harriet Zuckerman. Specifically, he and Zuckerman discovered that journals in the humanities and social sciences have a consistently higher rejection rate for submitted articles than do journals in the natural sciences. (In the Merton and Zuckerman study, for instance, the physics journals sampled rejected only 24 percent of submissions, whereas sociology and philosophy journals rejected more than 80 percent.) Merton took these divergences as evidence that philosophers and sociologists could not agree about what constituted significant or solid research, whereas natural scientists could agree about the merits of specific contributions by virtue of their shared norms and values. As Merton and Zuckerman wrote in 1971, "This suggests that these fields of learning [i.e., sociology and philosophy] are not greatly institutionalized in the reasonably precise sense that editors and referees on the one side and would-be contributors on

6. For a detailed account of these norms, see Merton's classic "The Normative Structure of Science," reprinted in Merton, 1973.

7. Merton, 1973, pp. 268-269.

the other almost always share norms of what constitutes adequate scholarship."[8] In sum, the review process in science, along with other features of its reward system, manages to institutionalize and to internalize the professional norms far more successfully (i.e., more uniformly) than the nonsciences do. The norms of science are not always explicit, but Merton is convinced that they are always decisive: "It has become manifest that in each age there is a system of science that rests upon a set of [normative] assumptions, usually implicit and seldom questioned by most scientific workers of the time."[9] Twenty years earlier Michael Polanyi had sketched a similar explanation for the high degree of consensus in science. "Each [scientist]," he wrote, "is pursuing a common underlying purpose and . . . each can sufficiently judge—in general accordance with other scientific opinion—whether his contribution is valid or not."[10] In Polanyi's view, this internalization of shared norms or standards explains the "spontaneous co-ordination [i.e., agreement] of scientists."[11]

No more than their philosophical counterparts did sociologists of this period think that agreement in science was inevitable or ubiquitous. They knew, of course, about some of the famous scientific controversies that have divided the scientific community into warring factions. But sociologists, such as Merton and his colleague Bernard Barber, tended to explain these deviations from the expected consensus by arguing that "prejudice and superstition" could sometimes serve as institutional and intellectual obstacles to scientists following the "scientific" norms expected of them. Barber, in particular, argued at length in a much cited study that philosophy and theology have sometimes intruded into science, making it difficult if not impossible for scientists to adhere conscientiously to their professional norms.[12] Thus, if it seems odd from a consensualist perspective that astronomers, supposedly sharing the same norms, should disagree for a century and a half about the merits of Ptolemy and Copernicus, then a sociologist of this persuasion accounts for that controversy by conjecturing that the

8. Ibid., p. 472. Still later in this essay, Merton makes the claim even more explicit: "the marked differences in rejection rates of journals in the various disciplines can be tentatively ascribed . . . in part to differences in the extent of consensus with regard to standards of adequate science and scholarship" (p. 474).

9. Ibid., p. 250.

10. Polanyi, 1951, p. 39.

11. Merton, 1973, pp. 268-269.

12. See especially Barber, 1961.

followers of Ptolemy were religiously prejudiced men who had only partly internalized the appropriate norms of science. The Copernicans, so the explanation goes, more fully succeeded in acting as scientists because they managed to separate secular and sectarian values. As quaint as these explanations now appear (for few historians of science would still venture to claim either that Ptolemaists were less scientific than Copernicus or that Copernicus was less "metaphysical" than Ptolemy), such explanations were commonplace throughout the 1940s and 1950s, and they went largely unchallenged by the sociologists and philosophers of the period. What is important for our purposes is that the sociological advocates of these consensualist approaches were convinced that, once the appropriate scientific norms did reassert themselves, scientific controversies would come to a decisive end. To this extent they accepted a sociologized version of the Leibnizian ideal, albeit one in which shared values, institutionalized into a collective system of rewards and punishment—rather than a methodological algorithm—provided the alchemy needed to bring harmony out of disagreement.

Rarely does Merton specifically say that the shared norms that guide and direct scientific research are the same norms that his philosophical contemporaries were taking to be constitutive of the scientific method, although he does write that "the institutional imperatives [or norms] derive from the goal and methods [of science]."[13] But it is not crucial for my purposes to insist on a virtual identity between the philosophical and sociological accounts of science dating from this period. What should be clear, however, is that both sociologists and philosophers of that era were inclined to think that agreement among scientists about the "facts of the matter" was the natural state of affairs and were disposed to explain such factual agreement by insisting that it was the direct result of agreement among scientists at a "deeper" level—at the level of procedures and methods (as the philosophers would put it) or at the level of norms and standards, incorporated into an institutional reward system (as the sociologists would have it). Both camps insisted that scientific agreement was the by-product of a prior methodological

13. Merton, 1973, p. 270. As further evidence that Merton believed that the norms were ultimately grounded in the rules of method, consider that he defines one of the core norms, organized skepticism, as "the detached scrutiny of beliefs in terms of empirical and logical criteria" (Merton, 1968, chap. 8). Such criteria are surely those provided by the rules of scientific methodology.

and axiological compact. In Polanyi's words, "the consensus of scientific opinion" changes because each of the various groups in science "agrees with respect to their standards."[14] What was said to make scientists unique, and to explain their near unanimity on most matters of fact, was a more fundamental consensus about the guiding aims of the activity and about the most effective means of implementing those ends.

As we now know, the consensual view of philosophers and sociologists of the 1950s and 1960s will not stand up to sustained analysis. Scientists have disagreed far too often and about too many important matters for one to treat scientific disagreements as minor deviations from a consensual norm. More to the point, we have studied many of these disagreements in sufficient detail to see that the explanatory resources of classical philosophy and sociology of science are impotent to handle the broad range of cases in which disagreement arises. It is frequently true, for instance, that scientists who are doing their best to follow appropriate norms of disinterestedness, objectivity, and rationality nonetheless find themselves led to very divergent conclusions. We now understand how deeply the data in science, especially at the research frontier, can underdetermine choice between theories. We now know that the logical empiricists were simply wrong in believing that all scientists have subscribed to the same methodological and evaluative standards. We have been able to show over and again that the prolonged scientific disagreements of the past were not mere *querrelles de mots* between empirically equivalent theories, but were, rather, genuine controversies between profoundly different rival frameworks which appeared, for a time, to be equally well supported by the available evidence.[15] Much information has accumulated in the past decade to suggest that scientists often violate Merton's proposed norms for scientific behavior and, on occasion, are even rewarded for such violations. More tellingly, we can easily specify circumstances in which a willingness to break with those norms is important for the progress of science.

That said, we ought not conclude that there is nothing right in the analysis of the logical empiricists and the Mertonians. As we shall see later, these scholars put their finger on important features of the scientific enterprise. But what can be added with some conviction is that

14. Polanyi, 1951, p. 217.

15. For a discussion of the groundlessness of many attempts to reduce genuine disagreements to mere verbal disputes, see Laudan, 1968; 1977, chap. 2.

neither approach has shown itself to have the explanatory resources to account for disagreement of the degree and of the kind which science, past as well as present, produces in abundance. As scholars began to discover some of the flaws in, and exceptions to, these earlier models, they reacted in a not uncommon way by suggesting that we must start again from scratch, more or less repudiating everything in the prevailing but discredited paradigm. Writers like Kuhn, Feyerabend, and a host of younger sociologists of science have spent the past several years developing an explanation of dissensus in science. It is to some of those models that I now turn.

THE "NEW-WAVE" PREOCCUPATION WITH DISSENSUS

To make a long story short, there are four lines of argument which undermine the classical preoccupation with scientific consensus: the discovery that scientific research is much more controversy-laden than the older view would lead one to expect; the thesis of theory incommensurability; the thesis of the underdetermination of theories; and the phenomenon of successful counternormal behavior. I want to discuss each of these matters briefly.

a) *The ubiquity of controversy.*—Theories change rapidly in science; it is a cliché that yesterday's science fiction becomes today's scientific orthodoxy. But sometimes these changes can turn into drawn-out, vituperative affairs that introduce fundamental divisions of belief and loyalty within the scientific community. I have already mentioned a few such debates: Copernicus-Ptolemy, wave-particle optics, atomism versus energetics. The list can be extended more or less indefinitely to include Newtonian versus Cartesian mechanics, uniformitarian versus catastrophist geology, vis viva versus momentum mechanics, one-versus two-fluid theories of electricity, Priestley versus Lavoisier in chemistry, the debates about spontaneous generation, Einstein versus Bohr on quantum mechanics, special creation versus evolutionary biology, the recent debates about continental drift, and so on. Each involved prominent scientists on both sides, genuinely different theories, lasted several decades, and seemed to count reasonable arguments on both sides. Cases such as these seem to make it clear that, whatever force the rules and norms of science may have going for them, they were in fact insufficient to bring these controversies quickly to a definitive resolution.

There is a different way in which one may formulate the point. If the consensual model and its implied Leibnizian ideal were sound, it is very difficult to understand how mavericks or revolutionaries in the scientific community could ever get their ideas off the ground. As Kuhn cogently argued, "In short, if a new candidate for [reigning] paradigm had to be judged from the start by hard-headed people who examined only relative problem-solving ability, the sciences would experience very few major revolutions."[16] Since revolutions do not occur overnight, every scientific revolution must be inaugurated by a period when some scientists are pursuing new ideas and others are quite happy with the reigning theories. The consensual model was said by its critics to make it very difficult to understand how reasonable men could ever differ, in ways that seem to be required to permit the exploration of new ideas. As Thomas Kuhn succinctly formulated this objection to the consensual approach: the emergence of new scientific ideas "*requires* a decision process which permits rational men to disagree, and such disagreement would generally be barred by the shared algorithm which philosophers have generally sought. If it [i.e., such an algorithm] were at hand, all conforming [i.e., rational] scientists would make the same decision at the same time."[17] Kuhn maintains that it is only the existence of differential preferences and values among scientists which allows new theories to flower. Otherwise, "no one . . . would be inclined to try out the new theory, to articulate it in ways which showed its fruitfulness or displayed its accuracy and scope."[18] It is telling that Kuhn in this passage, as in much of his work, ignores the fact that scientists can distinguish between criteria for acceptance of theories and criteria of pursuit worthiness.[19] Such a distinction allows one to circumvent some of the problems Kuhn raises for the consensual view. But to this extent Kuhn is surely right: the consensual view fails to make sense of the broad range and variety of cases of scientific disagreement. Because it does, something more must be going on than meets the consensual eye.

b) *The thesis of incommensurability.*—Kuhn himself proposed to fill in a part of the picture by claiming that the advocates of rival theories simply fail to communicate with one another. This failure is no

16. Kuhn, 1962, p. 156.
17. Kuhn, 1977 (italics in original).
18. Ibid., p. 332.
19. For a lengthy discussion of the difference between acceptance and pursuit, see Laudan, 1977, chap. 4.

accident, he thinks, because rival theories are radically incommensurable. We can see why he thinks so by looking at Kuhn's account of interparadigmatic disagreement. Far more than many of his predecessors, Kuhn was cognizant of the extent to which the history of science was rife with major controversies. He had himself written a very influential book about one such controversy, *The Copernican Revolution.* As Kuhn saw it, periods of scientific revolution were characterized by the (unpeaceful) coexistence of a variety of rival paradigms or world views, each with its own advocates. As Kuhn described these clashes between rival paradigms, they were always inconclusive. This is because the paradigms themselves were "incommensurable." Advocates of one paradigm literally could not understand their rivals; they lived in different worlds. They might use the same terminology, but they would typically mean fundamentally different things by it. The impossibility of full translation between rival paradigms is further exacerbated by the fact that, as Kuhn claims in his more recent *The Essential Tension,* the advocates of different paradigms often subscribe to different methodological standards and have nonidentical sets of cognitive values. Thus, what one party to a dispute views as a positive attribute in a theory may well be viewed as a liability by advocates of a different paradigm. So, there is a failure of communication with respect to both the substance of theories and the standards regarded as appropriate for their appraisal.

c) The underdetermination of theories by data.—Probably more important than either of the previous nudges toward a focus on disagreement was a family of arguments concerning underdetermination. In brief, they amount to the claim that the rules or evaluative criteria of science do not pick out one theory uniquely or unambiguously to the exclusion of all its contraries. Several separate lines of argument lead to this conclusion. One is the so-called Duheim-Quine thesis, according to which no theory can be logically proved or refuted by any body of evidence. Another route to the same conclusion hinges on the claim (associated, for rather different reasons, with the work of Wittgenstein and Nelson Goodman) that the rules of scientific inference, whether deductive or inductive, are so radically ambiguous that they can be followed in indefinitely many, mutually inconsistent ways. Pursuing a similar line, Kuhn has argued (in *The Essential Tension*) that the criteria of theory choice shared by scientists are too ambiguous to determine choice. This cluster of arguments has often been taken to entail that science cannot be the rule-governed activity that many empiricists and sociologists made it out to be.

d) Counternormal behavior.—Paul Feyerabend and Ian Mitroff have both argued that many highly successful scientists have repeatedly violated the norms or canons usually called scientific.[20] From time to time, scientists have ignored the evidence, tolerated inconsistencies, and pursued counterinductive strategies. More to the point, many of the most noteworthy instances of scientific progress seem to have involved scientists who rode roughshod over conventional methodological sensibilities. Minimally, such behavior seems to suggest (as Mitroff argues) that Merton has misidentified the norms that guide scientific practice. More radically, such behavior might lead one to conclude with Feyerabend that, where methods are concerned, "anything goes."

With such ammunition in hand, new-wave sociologists and philosophers of the last ten or fifteen years have been urging us to focus chiefly on scientific debate and disagreement, for (as they see it) such disagreement is far more likely to be the "natural" state of science than consensus is. More than that, these scholars have laid out elaborate machinery for explaining how disagreement could arise and persist (e.g., from incommensurability or underdetermination). But, as I have already hinted, these writers are ill equipped to explain how agreement ever congeals. To see how this approach comes unstuck with the problem of consensus formation, let us consider in some detail the difficulties that agreement poses for Kuhn's analysis. Because he believes that interparadigmatic dialogue is inevitably partial and incomplete, and because he thinks that the partisans of different paradigms subscribe to different methodological standards, Kuhn can readily explain why many scientific debates are protracted and inconclusive affairs. If both sides are indeed "talking past one another," if they are judging their theories against different yardsticks, then it is no surprise that they continue to disagree. In sum, Kuhn's model correctly predicts that dissensus should be a common feature of scientific life. What it cannot explain so readily, if at all, is how—short of sheer exhaustion or political manipulation—scientific disagreements are ever brought to closure. If rival scientists cannot understand one another's point of view, if they have fundamentally different expectations

20. See Feyerabend, 1978; Mitroff, 1974. Mitroff's evidence for effective "counternormal" behavior is a good deal more compelling than Feyerabend's.

about what counts as a "good" scientific theory, it seems utterly mysterious that those same scientists should ever (let alone often) reach a point where they eventually agree about which paradigm is acceptable. But without such agreement, the onset of normal science, whose existence Kuhn went to such lengths to document, becomes utterly unintelligible. Without an account of consensus formation, we are missing a crucial link between the two central ingredients in Kuhn's picture: his theory of disagreement (incommensurability) and his theory of consensus maintenance (normal science). Kuhn has often been faulted for failing to explain the transition from "normal" science to "crisis" science (i.e., from consensus to dissensus), because he never explained why recalcitrant but unthreatening puzzles should suddenly come to be regarded as paradigm-threatening anomalies. There is some justice in this criticism, but it misidentifies the core flaw in Kuhn's approach: that he has no plausible resources for explaining the far more striking transition from crisis to normal science. Once disagreement emerges in a scientific community, it is almost impossible to see how Kuhn can put the rabbit back into the box. When one considers how central the notion of consensus is to Kuhn's picture of science (after all, a paradigm is just what there is supposed to be consensus about, and normal science is just the sort of science that ensues when consensus reigns), it seems extraordinary that he offers no detailed account of the mechanisms of consensus formation. Worse that that, Kuhn's analysis has several features built into it which seem to foreclose any possibility of accounting for the emergence of consensus. Consider the fact that, in Kuhn's view, every paradigm is virtually self-authenticating: "each paradigm will be shown to satisfy more or less the criteria that it dictates for itself and to fall short of those dictated by its opponents."[21] If paradigms do indeed have this self-reinforcing character, then it is incomprehensible how the advocates of one paradigm might ever find reasons that would lead them to change their paradigmatic allegiances. Because Kuhn cannot explain how the advocates of rival paradigms might ever come to agree about which paradigm is better, he is in the hopeless position of requiring us to accept the existence of two radically distinct species of scientific life ("normal" and "revolutionary" science) without giving us any clues as to the dynamic process of metamorphosis, by which consensus emerges out of dissensus. Periods

21. Kuhn, 1962, pp. 108-109.

of revolutionary and normal science may each make a kind of sense in its own right, but Kuhn has no convincing story to tell about how science moves from one state to the other. Nor is it difficult to see why Kuhn lacks a theory of consensus formation: his account of dissensus requires such deep-rooted divergences and incommensurabilities between scientists that there remains no common foundation upon which to shape agreement anew.

It would be misleading to give the impression that Kuhn has nothing to say about the emergence of consensus; he does address the issue on occasion. Indeed, he goes so far as to say that what is virtually "unique" to science is that consensus emerges so convincingly out of dissensus.[22] Additionally, he devotes an entire chapter of *Structure* to answering what is essentially the question of consensus formation: "What causes the group [i.e., a scientific community] to abandon one tradition of normal research in favor of another?"[23] But what he does have to say is, when taken collectively, inconsistent, and, when taken singly, unconvincing. Sometimes, for instance, Kuhn will explain the transition from consensus in favor of one paradigm to consensus for a rival by invoking purely external considerations. We have to wait, he says, for the older generation to die off before the new paradigm establishes hegemony (the so-called Planck principle).[24] But, even if true, this provides no answer to the central question; for it fails to explain (if it be so) why the *younger* scientists are able to agree that one particular rival to the orthodoxy is preferable to others. After all, transitional periods of crisis are, for Kuhn, typified by the existence of a multitude of new paradigms, each vying for the allegiance of the relevant scientific practitioners. Even if we assume (with Kuhn) that younger scientists are more open to novelty than their elders, we still have no explanation for the fact that the young Turks are so often able to agree about which dark horse to back. If Kuhn is right about incommensurability of beliefs and incompatibility of standards, young advocates of

22. Kuhn (ibid., p. 17) writes: "What is surprizing, and perhaps also unique in its degree to the fields we call science, is that such initial divergences should ever largely disappear."

23. Ibid., pp. 143 ff.

24. The Planck principle is summed up in Max Planck's famous quip (1949, pp. 33-34): "a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die and a new generation grows up that is familiar with it." For an excellent critique of the implications of Kuhn's version of the Planck principle, see Hull, 1978.

rival paradigms should have all the same difficulties their elders do in reaching agreement about the respective merits of competing paradigms. Identical objections apply to Kuhn's suggestion that hegemony and normal science reassert themselves once the advocates of a particular paradigm get control of the major journals and the prestige appointments in a discipline. Even if true, such a reduction of scientific decision making to *Realpolitik* leaves unexplained the processes whereby the scientific elite in science comes to rally around a single new paradigm.

On other occasions, sounding rather more traditional, Kuhn says that consensus eventually congeals around a new paradigm because it can be seen to be objectively better than its predecessor by such criteria as its degree of empirical support, its demonstrated fertility, and its perceived problem-solving ability.[25] But if it is possible to compare theories along these vectors so as to get all or most scientists to agree about them, then it is unclear what all of Kuhn's earlier fuss about incommensurability and the absence of shared standards amounted to. He cannot have it both ways. Either there are shared and unambiguous standards which can be invoked by the proponents of rival paradigms for deciding the issue between them (in which case Kuhn's talk about incommensurability and the nonspecificity of shared cognitive values comes to naught, thus undermining his explanation of dissensus), or else there are no such standards (in which case Kuhn's account of disagreement escapes unscathed but only at the apparent expense of his being unable to explain consensus formation).

Kuhn is scarcely unique among contemporary philosophers and sociologists of science in propounding an account of disagreement which leaves little or no scope for explaining agreement. Imre Lakatos and Paul Feyerabend, for instance, are in the same plight, if for rather different reasons. Lakatos went to great lengths to stress the role of various conventions in theory assessment. For him, the decision to treat a prima facie falsifying instance as a genuine refutation was a matter of "convention." Mindful of the Duhemian ambiguities of falsification, Lakatos argued that rational scientists could completely ignore apparent refutations for their research programs. If they do, it becomes entirely conceivable that rival theorists might conduct a controversy for years, even decades, without the disagreement issuing in any firm

25. For this side of Kuhn's work, see especially the last chapters of Kuhn, 1962.

consensus. But what Lakatos always left opaque was how a community of scientists might reasonably come to the conclusion that one research program was genuinely superior to another, thereby reestablishing consensus. On Lakatos's account, as on Kuhn's, it appears reasonable to hang onto a theory—no matter what empirical anomalies confront it—more or less indefinitely. But to say as much is, in effect, to say that there are no rational mechanisms whereby consensus about the preferability of one line of research over another can be established. Since that sort of consensus is commonplace in the sciences, Lakatos's approach leaves us with no explanation of the fact that scientists come, often quite rapidly, to regard most scientific controversies as definitively resolved.

If Lakatos was an anarchist in spite of himself, Feyerabend set out quite deliberately to elaborate a theory of knowledge which would favor rampant theoretical pluralism. In Feyerabend's view of the matter, it is undesirable that scientists should ever reach consensus about anything. His ideal of science is the sort of endless questioning of fundamentals which one associates with pre-Socratic natural philosophy: nothing is taken as given, everything can reasonably be denied or affirmed. Like Kuhn, Feyerabend believes in the radical incommensurability of theories. Far more than Kuhn, he denies that there are any methodological principles or norms which it is reasonable to insist that scientists follow in assessing theories ("anything goes"). Feyerabend does not deny that scientists sometimes do agree about which theories are good and which are bad, but he deplores that state of affairs as unreasonable. If scientists only appreciated the finer points of epistemology, he seems to say, they would see that no theories should ever be regarded as having displaced or discredited their rivals and predecessors.

Sociologists, too, have been quick to see that the existence of widespread controversy in science fits ill with older models of science in their field. Michael Mulkay seems to speak for many of the new-wave theorists in regarding the phenomenon of scientific disagreement as a refutation of older approaches: "If, for example, the Mertonian norms are effectively institutionalized in science, it becomes difficult to account for the frequency of intellectual resistance [which] is recurrent in science and is, indeed, an inescapable feature of the growth of scientific knowledge."[26]

26. Mulkay, 1977, p. 106.

A preoccupation with scientific disagreement has lately shown up with increasing frequency in the research of several sociologists of science, including Collins, Pickering, and Pinch. Harry Collins, for instance, has devoted much effort to studying some recent controversies in theoretical physics. In each of the cases Collins examined, he found that ingenious scientists can concoct a way to circumvent arguments and evidence against their pet theories. In effect, Collins's claim is that the experimental evidence is always so ambiguous that virtually any theory can be maintained in the face of any evidence. As he puts it, "the natural world in no way constrains what is believed to be."[27] Or, "the natural world has a small or non-existent role in the construction of scientific knowledge."[28] Since, in Collins's view, the features of the world (as we come to learn about them from experiment and observation) do virtually nothing to restrain our beliefs about the world, Collins has a ready explanation for the prolongation of scientific disagreement. But, as with Lakatos, Feyerabend, and Kuhn, that explanatory virtue quickly becomes a liability because, having severed all significant causal links between the world and our beliefs about it, Collins cannot bring the world back into the picture as a factor driving scientists to eventual consensus. Although Collins says he is interested in the mechanisms of consensus formation in science (he asserts it is important to describe the "mechanisms which *limit* interpretative flexibility and thus allow controversies to come to an end"),[29] I predict he will find that, having written off the world as a constraint on our beliefs, he lacks the most relevant explanatory resources for tackling that problem. Like the others, Collins seems to be in the awkward position of having so robustly explained how scientists can disagree that it becomes nothing short of miraculous that scientists are frequently able to reach a broad agreement about the "facts" of the world and about which theories are the most promising or plausible for explaining those facts.

In this brief thumbnail sketch of some currents in contemporary philosophy and sociology of science, I have not attempted to establish that any of the new-wave approaches are false or that their flaws are irremediable. But what does seem clear is that the newly forming orthodoxy in philosophy and sociology of science is confronted by challenges

27. Collins, 1981*a*, p. 54.
28. Collins, 1981*b*, p. 3.
29. Ibid., p. 4.

every bit as daunting as those that proved to be the undoing of empiricist methodology and Mertonian sociology. More specifically, many recent theorists, while labeling classical philosophy and sociology as impoverished, have ignored the central issues with which their predecessors were grappling. We can scarcely claim to have moved significantly beyond the work of the 1940s and 1950s unless we can make some sense of the striking facts that scholars of that generation rightly regarded as basic features of science. We either have to deny with Feyerabend that rational scientists could ever exhibit widespread agreement (and that seems to run counter to the record), or else we have to find some account of dissensus which is not so robust that it precludes the very possibility of frequent and widespread agreement. Until we manage to account for a Janus-faced science, we cannot seriously claim to have understood what we are about.

This book is an attempt to move us some steps forward in that direction. In succeeding chapters I focus chiefly on describing the various levels at which scientific disagreement can occur. In each instance we will be exploring how far one can expect disagreements to be amenable to rational analysis and rational closure. As we shall come to see, the full-blown Leibnizian ideal cannot be plausibly resurrected, for there remain many scientific controversies that cannot be rationally terminated, even with the best will in the world. On the other hand, we will discover a very large range of cases where there is appropriate analytic machinery for understanding how many scientific controversies can be brought to a reasonably definitive resolution.