A full argument for principles (A_1) and (A_2) requires lengthier treatment than I can give here. What I shall claim, however, is that there is nothing in the usual arguments for the ambiguity of testing which would undercut (A_1) or (A_2). To that extent at least, we are entitled to claim that it seems to be entirely appropriate to talk about the appraisal of individual theories—with the proviso that such appraisals concern problem-solving effectiveness and not truth or falsity.

There is yet another important dimension of the Duhemian problem which must be mentioned here, although a thorough treatment of it will have to wait until we have developed further machinery for theory appraisal in the next chapter. The dimension in question has to do with the nature of a rational response to a so-called falsifying experiment. On my analysis, whenever a complex of theories generates an anomaly, that anomaly counts against *each* element within the complex. The fact that each of those theories has this particular anomaly does not, of course, require that they should each be abandoned; for, as we have seen, the existence of an anomalous problem for a theory is not *ipso facto* sufficient grounds for abandoning the theory. But that is not an end on it. Precisely because the anomaly exists, and because science seeks to minimize anomalies, there is still cognitive pressure on the scientific community to attempt to resolve the anomaly. Resolving that anomaly will require, presumably, the abandonment (though not by virture of its "falsification") of at least one of the theories that composed the complex that was unable to deal with the anomaly. From my point of view (and I suspect that from Duhem's too), *the real challenge of the Duhemian analysis consists,* not in showing how we can "localize" falsehood or truth, but rather *in showing what rational strategies there are for selecting a better complex.*[22] It is this point to which I shall return in chapter three, where machinery for making the relevant assessments will be described.

Chapter Two

Conceptual Problems

> *If a historian accepts the [customary] analysis of confirmation, . . . he may conclude that the course of scientific development is massively influenced by . . . nonevidential considerations.* WESLEY SALMON (1970), p. 80

Our discussion in chapter one focussed exclusively on empirical problems and on the connections between such problems and the theories which purport to solve them. It would be an enormous mistake, however, to imagine that scientific progress and rationality consist entirely of solving empirical problems. There is a second type of problem-solving activity which has been *at least as important* in the development of science as empirical problem solving. This latter type of problem, which I call a *conceptual problem,* has been largely ignored by historians and philosophers of science (though rarely by scientists), presumably because it does not comport well with those empiricist epistemologies of science which have been the reigning fashion for more than a century. The purpose of this chapter is to state the case for a richer theory of problem solving than empiricists have allowed, to explore the nature of these nonempirical problems and to show what role they have in theory appraisal.

Even the briefest glance at the history of science makes it clear that the key debates between scientists have centered as much on nonempirical issues as on empirical ones. When, for instance, the epicyclic astronomy of Ptolemy was criticized (as it often was in antiquity, the Middle Ages and the Renaissance), the core criticisms did *not* deal with its adequacy to solve the chief empirical problems of observational astronomy. It was readily granted by most of Ptolemy's critics that his system was perfectly adequate for "saving the phenomena." Rather, the bulk of the criticism was directed against the conceptual credentials of the mechanisms Ptolemy utilized (including equants and eccentrics, as well as epicycles) for solving the empirical problems of astronomy. Similarly, the later critics of Copernican astronomy did not generally claim it was empirically inadequate at predicting the motions of celestial bodies; indeed, it could solve some empirical problems (such as the motion of comets) far better than the available alternatives. What chiefly troubled Copernicus' critics were doubts about how heliocentric astronomy could be integrated within a broader framework of assumptions about the natural world—a framework which had been systematically and progressively articulated since antiquity. When, a century after Copernicus, Newton announced his "system of the world," it encountered almost universal applause for its capacity to solve many crucial empirical problems. What troubled many of Newton's contemporaries (including Locke, Berkeley, Huygens, and Leibniz) were several conceptual ambiguities and confusions about its foundational assumptions. What was absolute space and why was it needed to do physics? How could bodies conceivably act on one another at-a-distance? What was the source of the new energy which, on Newton's theory, had to be continuously super-added to the world order? How, Leibniz would ask, could Newton's theory be reconciled with an intelligent deity who designed the world? In none of these cases was a critic pointing to an unsolved or anomalous empirical problem. They were, rather, raising acute difficulties of a *nonempirical kind.* Nor is it merely "early" science which exhibits this phenomenon.

If we look at the reception of Darwin's evolutionary biology, Freud's psychoanalytic theories, Skinner's behaviorism, or

modern quantum mechanics, the same pattern repeats itself. Alongside of the rehearsal of empirical anomalies and solved empirical problems, both critics and proponents of a theory often invoke criteria of theoretical appraisal which have nothing whatever to do with a theory's capacity to solve the empirical problems of the relevant scientific domain.

Of course, this pattern has not gone unnoticed by historians, philosophers and sociologists of science; it is too obvious and too persistent to have been ignored altogether. But the usual response, when confronted with cases in which theories are being appraised along nonempirical vectors, has been to deplore the intrusion of these "unscientific" considerations and to attribute them largely to prejudice, superstition, or a "pre-scientific temperament." Some scholars (such as Kuhn) have gone so far as to make the absence of such nonempirical factors a token of the "maturity" of any specific science.[1] Rather than seeking to learn something about the complex nature of scientific rationality from such cases, philosophers (with regret) and sociologists (with delight) have generally taken them as tokens of the irrationality of science as actually practiced.[2] As a result few scholars who study the nature of science have found any room in their models for the role of such conceptual problems in the rational appraisal of scientific theories.[3] Empiricist philosophies of science (including those of Popper, Carnap and Reichenbach) and even less strident empiricist methodologies (including those of Lakatos, Collingwood and Feyerabend)—all of which imagine that theory choice in science should be governed exclusively by empirical considerations—simply fail to come to terms with the role of conceptual problems in science, and accordingly find themselves too impoverished to explain or reconstruct much of the actual course of science. Such empiricist theories of science exhibit particularly awkward limitations in explaining those historical situations in which the empirical problem-solving abilities of competing theories have been virtually *equivalent.* Cases of this kind are far more common in science than people generally realize. The debates between Copernican and Ptolemian astronomers (1540-1600), between Newtonians and Cartesians (1720-1750), between wave and particle optics (1810—1850), between atomists and anti-atomists (1815 to about 1880) are all examples of important scientific controversies where the empirical support for rival theories was essentially the same. Positivistically inspired accounts of these historical encounters have shed very little light on these important cases: this is scarcely surprising since the positivist holds empirical support to be the only legitimate arbiter of theoretical belief. These controversies must, by the strict empiricist, be viewed as mere *querelles de mots,* hollow and irrational debates about issues which experience cannot settle.

A broader view concerning the nature of problem solving—one which recognizes the existence of conceptual problems—puts us in a position to understand and to describe the kind of intellectual interaction that can take place between defenders of theories which are equally supported by the data. Because the assessment of theories is a multi-factorial affair, parity with respect to one factor in no way precludes a rational choice based on disparities at other levels.

The Nature of Conceptual Problems

Thus far, we have defined conceptual problems by exclusion, suggesting that they are nonempirical. Before we can understand their role in theory appraisal, we must clarify precisely what they are and how they arise. To begin with, we must stress that a conceptual problem is a problem *exhibited by some theory or other.* Conceptual problems are characteristics of theories and have no existence independent of the theories which exhibit them, not even that limited autonomy which empirical problems sometimes possess. If empirical problems are first order questions about the substantive entities in some domain, conceptual problems are higher order questions about the well-foundedness of the conceptual structures (e.g., theories) which have been devised to answer the first order questions. (In point of fact, there is a continuous shading of problems intermediate between straightforward empirical and conceptual problems; for heuristic reasons, however, I shall concentrate on the distant ends of the spectrum.)

Conceptual problems arise for a theory, *T,* in one of two ways:

1. When *T* exhibits certain internal inconsistencies, or when its basic categories of analysis are vague and unclear; these are *internal conceptual problems.*
2. When *T* is in conflict with another theory or doctrine, *T′*, which proponents of *T* believe to be rationally well founded; these are *external conceptual problems.*

Each of these forms of conceptual problems needs to be analyzed in some detail.

Internal Conceptual Problems

The most vivid, though by no means the most frequent, type of internal conceptual problem arises with the discovery that a theory is logically inconsistent, and thus self-contradictory. Probably most common in the history of mathematics, inconsistent theories have often been detected in almost all the other branches of science.[4] Little need be said about the acuteness of such problems. Unless the proponents of such theories are prepared to abandon the rules of logical inference (which provided the groundwork for recognizing the inconsistency), or can somehow "localize" the inconsistency, the only conceivable response to a conceptual problem of this kind is to refuse to accept the offending theory until the inconsistency is removed.[5]

More common, as well as more difficult to handle, are a second class of internal conceptual problems; namely, *those arising from conceptual ambiguity or circularity within the theory.* Unlike inconsistency, the ambiguity of concepts is a matter of degree rather than kind. Some degree of ambiguity is probably ineliminable in any except the most vigorously axiomatized theories. It may even be true that some small measure of ambiguity is a positive bonus, since less rigorously defined theories can often be more readily applied to new domains of investigation than more rigid ones. But granting that, it is nonetheless true that systematic and chronic ambiguity or circularity within a theory often has been, and should be, viewed as highly disadvantageous.

Examples of such conceptual problems abound in the history of science. For instance, Faraday's early model of electrical interaction was designed to eliminate the concept of action-at-a-distance (itself a conceptual problem in earlier Newtonian physics). Unfortunately, as Robert Hare showed,[6] Faraday's own model required short range actions-at-a-distance. Faraday had merely replaced one otiose concept by its virtual equivalent. Even worse, Faraday's model—as Hare was quick to point out—postulated "contiguant" particles, which were not really contiguous at all. These kinds of criticisms led Faraday to re-think his views on matter and force and were eventually responsible for the emergence of Faraday's field theory, which avoided these conceptual problems. Taking another example from nineteenth-century physics, it was often alleged by the critics of the kinetic-molecular theory (e.g., Stallo and Mach) that the kinetic theory was nonexplanatory because circular. For instance, it explained the elasticity of gases by postulating elastic constituents (i.e., molecules). But, observed the critics, because we understand no more about the causes of elasticity in solids than we do in fluids, the kinetic explanation is entirely circular.[7]

The increase of the conceptual clarity of a theory through careful clarifications and specifications of meaning is, as William Whewell observed more than a century ago, one of the most important ways in which science progresses. He called this process "the explication of conceptions" and showed how a number of theories, in the course of their temporal careers, had become increasingly precise—largely as a result of the critics of such theories emphasizing their conceptual unclarities.[8] Many important scientific revolutions (e.g., the emergence of the theory of special relativity, the development of behavioristic psychology) have depended largely on the recognition, and subsequent reduction, of the terminological ambiguity of theories within a particular domain.

Although both these types of internal problems are doubtlessly important in the process of theory appraisal, neither have played as decisive a historical role as the other kinds of conceptual problems have.

External Conceptual Problems

External conceptual problems are generated by a theory, T, when T is in conflict with another theory or doctrine which the

proponents of T believe to be rationally well founded. It is the existence of this "tension" which constitutes a conceptual problem. But what precisely do the "tension" and the "conflict" amount to? The easiest form of "tension" to define, although by no means the most frequent, is that of *logical inconsistency* or *incompatibility*. When one theory is logically inconsistent with another accepted theory, then we have a vivid example of a conceptual problem.

The development of astronomy in ancient Greece, to which we have already referred, provides a useful case in point. The unsolved empirical problem here (it was actually a host of related problems) was summarized in tables of planetary motion, tables which recorded the apparent positions of the sun, moon, and planets at different times. This was the initial empirical problem which had to be resolved. The succession of planetary theories in antiquity, from the homocentric spheres of Eudoxus and Aristotle to the complex epicycles, eccentrics, and equants of Ptolemy, illustrates a series of attempts to solve the problems of early astronomy. But as soon as the early astronomical theories were developed each of them in turn generated a plethora of other problems, some of them empirical, others conceptual. Thus, the homocentric spheres of Eudoxus and Aristotle failed to explain accurately the retrogradations of the planets and the seasonal inequalities exhibited by the data. These phenomena were clearly recognized as unsolved problems. On the other hand, the later system of Ptolemy managed to avoid most of the anomalous problems which earlier Greek astronomy had encountered, but the price it paid to do so was that of *generating enormous conceptual problems.* Ever since the time of Plato, astronomers had worked on the assumption that the heavenly motions were "perfect" (i.e., that each planet moved in a perfect circle about the earth at constant speed). This assumption put enormous constraints on the kinds of hypotheses which were open to astronomers. Ptolemy's system, for all its empirical virtues, ran afoul of these prohibitions by making assumptions about the behavior of celestial bodies (e.g., the hypothesis that certain planets move around empty points in space, that planets do not always move at constant speed, and the like) which were in flagrant contradiction with the then universally accepted physical and cosmological theories about the nature and motion of the heavenly bodies. In spite of ingenious efforts to reconcile these differences by Ptolemy and others, most of the crucial conceptual problems remained, and were to plague the development of mathematical astronomy until the end of the seventeenth century (and even beyond).

But there are other relations besides that of inconsistency which also constitute conceptual problems for those theories which exhibit them. One common situation arises when two theories, although logically compatible, are jointly *implausible*, i.e., when the acceptance of either one makes it less plausible that the other is acceptable. For example, many late seventeenth-century theories of physiology were based on the (Cartesian) assumption that the various bodily processes were essentially caused by the mechanical processes of collision, filtration, and fluid flow. Once Newtonian physics was accepted, many critics of mechanistic physiology pointed out that such mechanistic doctrines, although logically compatible with the physics of Newton, were nonetheless rendered rather implausible by Newtonian physics. The argument went something like this: Newtonian physics, while certainly allowing for the existence of collision phenomena, nonetheless shows that *most* physical processes depend upon more that the impacts between, and the motions of, particles. To the extent that "mechanistic" (Cartesian inspired) theories of physiology postulate such processes as the *exclusive* determinant of organic change, they rest on a huge improbability. They are consistent with Newtonian physics (for that physics does not deny that there can be some material systems which are entirely mechanical); but it did seem highly implausible, given Newtonian physics, that a system as complex as a living organism could function with only a limited range of the processes exhibited in the inorganic realm.

A second example may clarify the notion of conceptual problem-generation by joint implausibility between theories. Throughout the seventeenth and early eighteenth century, the dominant theory of heat was a *kinetic* one; heat was conceived

as the rapid agitation of the constituent parts of a body. Throughout the eighteenth century, however, a number of theories in a variety of fields began to suggest that many natural processes depended upon the presence of one or more highly elastic, highly rarefied fluids which could be absorbed by, or released from, material bodies. Although electricity was the best known example, such subtle fluids were postulated to explain magnetism, neurological functioning, perception, embryology, and even gravity. As these theories became more widely accepted, and as certain observable analogies between heat, light and electricity began to be explored, kinetic theories of heat came under sustained attack. While the acceptance of, for example, a fluid theory of electricity did not entail the denial of the kinetic theory of heat, it was thought that kinetic theories of heat became increasingly implausible as one domain after another came to be dominated by highly successful ideas about the substantial, as opposed to the kinetic, nature of physical processes.

A third manner in which conceptual problems can be generated occurs when a theory emerges which ought to reinforce another theory, but fails to do so and is *merely compatible with it.* To understand what is involved in such cases, we must talk briefly about the interdisciplinary structure of science, for compatibility between two systems or theories is not, in common parlance, regarded as a sign of cognitive weakness. The various scientific disciplines and domains are never completely independent of one another. At any given epoch, there are hierarchical systems of interconnection between the various sciences which condition the rational expectations which scientists have when they appraise theories. In our own time, for instance, it is presumed that the chemist will look to the physicist for ideas about atomic structure; that the biologist should utilize chemical concepts when talking about organic microstructures. The enunciation of a chemical theory which was merely compatible with quantum mechanics, but which utilized none of the concepts of quantum theory, would be viewed askance by most modern scientists. Similarly, a theory of heredity which was compatible with chemistry but failed to exploit any of its analytic machinery, would likewise be suspect. Different epochs, of course, will have different expectations about which disciplines should borrow from, and reinforce, others. (In the seventeenth century, for instance, it was expected that any physical theory should be positively relevant to, and not merely compatible with, Christian theology.)

As should be clear, *mere* compatibility between two theories is not always a conceptual problem. No one thinks, for instance, that a theory in micro-economics is flawed if it is merely compatible with thermodynamics. But in many cases, compatibility, as opposed to positive relevance, between two theories is quite rightly viewed as a major drawback to the acceptance of the theories in question.

Our discussion thus far puts us in a position to outline a taxonomy of the various cognitive relationships which can exist between two (or more) theories:

1. *Entailment*—one theory, T, entails another theory, T_1.
2. *Reinforcement*—T provides a "rationale" for (a part of) T_1.[9]
3. *Compatibility*—T entails nothing about T_1.
4. *Implausibility*—T entails that (a part of) T_1 is unlikely.
5. *Inconsistency*—T entails the negation of (a part of) T_1.

In principle, any relation short of full entailment (1) could be regarded as posing a conceptual problem for the theories exhibiting it. It should be stressed, however, that although situations (2) to (5) can generate conceptual problems, they *pose very different degress of cognitive threat;* those degrees are represented, in increasing order, by the sequence (2) through (5).

The Sources of Conceptual Problems

In discussing external conceptual problems, I was deliberately vague about what sorts of theories or beliefs can generate conceptual problems for a scientific theory. I have avoided this issue thus far because I wanted to focus first on the kinds of connections between theories which could generate conceptual problems. The time has come, however, to spell out the other side of the issue by asking what sorts of theories can qualify to

be paired with a scientific theory in order to generate a conceptual problem; for unless we can answer that question coherently, one could trivially and mechanically generate conceptual problems for any theory simply by conjoining it arbitrarily with any "wild" belief we liked. For instance, we could create a problem for modern quantum theory by pointing out its lack of relevancy for Zen Buddhism! So far as I can tell, there are at least three distinct classes of difficulties which can generate external conceptual problems: (1) cases where two *scientific* theories from different domains are in tension; (2) cases where a scientific theory is in conflict with the *methodological* theories of the relevant scientific community; and (3) cases where a scientific theory is in conflict with any component of the prevalent *world view.* Each merits serious discussion.

Intra-scientific difficulties. It is very often the case that a new theory in some scientific domain will make assumptions about the world which are incompatible with the assumptions of another *scientific* theory, a theory which we have good independent grounds for accepting. Thus, the astronomical system of Copernicus—while not a theory of physics in itself—nonetheless made a number of assumptions about the motion of bodies which were inconsistent with the then accepted Aristotelian mechanics. One of the strongest sixteenth-century arguments against the Copernican system consisted in pointing out that the theory of Copernicus, although perhaps adequate so far as the astronomical evidence went, was unacceptable because it ran counter to the tenets of the best established physical theory. Even worse, Copernicus really had no well-articulated alternative system of mechanics with which to rationalize the assumptions he was making about the motion of the earth. It was Galileo's signal contribution to deal with this conceptual problem, by recognizing the incompatibility between Aristotelian physics and Copernican astronomy and by remedying the situation by designing a new physics which was independently plausible and compatible with Copernican astronomy.

The recognition and resolution of such conceptual problems has been one of the more fertile processes in the history of the natural and the social sciences.[10] If two scientific theories are inconsistent or mutually implausible, there is a strong presumption that at least one of them should be abandoned. That much is straightforward. What is more interesting is the fact that one generally cannot simply jettison one or the other of an inconsistent pair without wreaking havoc with the rest of scientific knowledge. Because theories in certain domains (say, astronomy) seem to require for their comprehension and empirical assessment the existence of theories in other domains (say, mechanics or optics),[11] *the decision to abandon one of a pair of inconsistent theories and to retain the other member of the pair usually involves a commitment to develop an adequate alternative to the rejected theory.*

As a result, such conceptual problems are generally much easier to recognize than to resolve. Rarely, if ever, can we resolve such problems by the simple device of rejecting one of the offending pair. Moreover, as we have already seen, there is nothing built into the process of scientific evaluation which can inform us in advance which member of an inconsistent pair ought to be rejected. That is a question which can be resolved only after the fact, i.e., once we have tried giving up one, then the other, and have observed with what success we can construct an adequate pair-member for the retained theory.

Two final points about intra-scientific conceptual problems should be made in passing. It should be stressed, first, that the fact that a particular theory is incompatible with another accepted theory creates a conceptual problem for *both* theories. The inconsistency relation is symmetrical, and we must not lose sight of the fact that intra-scientific conceptual problems inevitably raise presumptive doubts about both members of the incompatible pair. Second, we should observe that the noting of a logical inconsistency or a relation of non-reinforcement between two theories need *not* force scientists to abandon one, or the other, or both. Just as it can sometimes be rational to retain a theory in the face of anomalous evidence, so, too, can it be sometimes rational to retain a theory in the face of an inconsistency between it and some other accepted theory. What we must recognize is that the occurrence of such an inconsistency indicates a *weakness,* a reason for considering the abandonment of one or the other theory (or perhaps both).

Among the most vivid examples of intra-scientific difficulties were the controversies between biologists, geologists, and physicists in the late nineteenth century over the chronology of the earth. On the geological and biological side was an enormous amount of evidence to support the view that the earth was very old indeed, that it was partially fluid under the surface, and that physical conditions on its surface had remained largely unchanged for hundreds of millions of years. Both uniformitarian geology and evolutionary biology rested upon such assumptions. The physicist Lord Kelvin, however, found himself unable to reconcile these core postulates with thermodynamics. Specifically, he showed that the second law of thermodynamics (entailing an increase in entropy) was incompatible with an evolutionary account of species and that both the first and second laws were incompatible with the geologist's hypothesis that the energy reserves in the earth had remained constant through much of the geological past. General perplexity abounded. Thermodynamics had much going for it in physics, but the dominant geological and biological theories also could point to a huge reserve of solved problems. The dilemma was acute: ought one abandon thermodynamics, reject uniformitarian geology, or repudiate evolutionary theory? Or was there some other option? As it turned out, though no one could have foreseen this in advance, all three could be retained, since the discovery of radioactivity made it possible to circumvent the problems about energy conservation. What matters here, for our purposes, is that the emergence of this incompatibility created acute conceptual problems for *all* the sciences concerned. If the route to a resolution of the problems was murky, it was generally perceived that these conceptual problems, until resolved, raised strong doubts about the problem-solving efficacy of a wide range of scientific theories.

Normative difficulties. Science, as it is often said, is an activity, an activity conducted by seemingly rational agents. As such, it has certain aims and goals. The rational assessment of science must therefore be, in large measure, a matter of determining whether the theories of science achieve the cognitive goals of scientific activity. What are these goals and how do we achieve them? It is one of the central functions of any philosophy or methodology of science to specify those goals and to indicate the most effective *means* for achieving them. The whole point of a methodological rule (such as Newton's classic dictum, "hypotheses non fingo") is to offer a norm for scientific behavior; to tell us what we should, or should not, do in order to achieve the cognitive, epistemic, and practical goals of the scientific enterprise.

Since antiquity, philosophers and philosopher-scientists have sought to define sets of norms, or methodological rules, which are expected to govern the behavior of the scientist. From Aristotle to Ernst Mach, from Hippocrates to Claude Bernard, thinkers concerned about science have attempted to legislate concerning the acceptable modes of scientific inference. In the early seventeenth century, the dominant image was mathematical and demonstrative, an image that became canonical in Descartes' famous *Discourse on Method.* In the eighteenth and early nineteenth century, by contrast, most natural philosophers were convinced that the methods of science should be inductive and experimental. Not surprisingly, every historical epoch exhibits one or more dominant, normative images of science. It would be a serious mistake to imagine, as many historians do, that these norms are just the concern of the professional philosopher or logician. *Every* practicing scientist, past and present, adheres to certain views about how science should be performed, about what counts as an adequate explanation, about the use of experimental controls, and the like. *These norms, which a scientist brings to bear in his assessment of theories, have been perhaps the single major source for most of the controversies in the history of science, and for the generation of many of the most acute conceptual problems with which scientists have had to cope.*

It is still widely maintained that the methodology to which a scientist subscribes is really little more than perfunctory window dressing, which is honored more in the breach than in the observance. Prominent scientists and historical scholars of our own era (most notably Einstein and Koyré[12]) have scoffed at the idea that a scientist's explicit views about methodology can exert much impact on his scientific beliefs and activities. Moreover, there are significant cases (e.g., Newton and Galileo) in which a

scientist's actual research violates almost every methodological rule to which he pays lip service. How, under those circumstances, can I argue here that methodology is a potent source for the evaluation of scientific theories and for the generation of conceptual problems?

Fortunately, the work of several historians in the last twenty years has provided overwhelming evidence that the methodological beliefs of scientists often do profoundly effect their research and their appraisals of the merits of scientific theories.[13] What all these investigations make clear (contra-Einstein and Koyré) is that the fate of most of the important scientific theories in the past have been closely bound up with the *methodological* appraisals of these theories; *methodological well-foundedness* has been constitutive of, rather than tangential to, the most important appraisals of theories.

It is for precisely that reason that perceived methodological weaknesses have constituted serious, and often acute, conceptual problems for any theory exhibiting them. It is for the same reason that the elimination of incompatibilities between a theory and the relevant methodology constitutes one of the most impressive ways in which a theory can improve its cognitive standing.

The resolution of a "tension" between a methodology and a scientific theory is often achieved by modifying the scientific theory so as to reconcile it to the methodological norms. But such problems are not always resolved in this fashion. In many cases, *it is the methodology itself which is altered.* Consider, as but one example, the development of Newtonian theory in the eighteenth century. By the 1720s, the dominant methodology accepted alike by scientists and philosophers was an *inductivist* one. Following the claims of Bacon, Locke, and Newton himself, researchers were convinced that the only legitimate theories were those which could be inductively inferred by simple generalization from observable data. Unfortunately, however, the direction of physical theory by the 1740s and 1750s scarcely seemed to square with this explicit inductivist methodology. Within electricity, heat theory, pneumatics, chemistry and physiology, Newtonian theories were emerging which postulated the existence of imperceptible particles and fluids—entities which could not conceivably be "inductively inferred" from observed data. The incompatibility of these new theories with the explicit methodology of the Newtonian research tradition produced acute conceptual problems. Some Newtonians (especially those in the so-called "Scottish School") sought to resolve the conceptual problems by simply repudiating those physical theories which violated the accepted methodological norms.[14] Other Newtonians (e.g., LeSage, Hartley, and Lambert) insisted *the norms themselves should be changed* so as to bring them into line with the best available physical theories.[15] This latter group took it on themselves to hammer out a new methodology for science which would provide a license for theorizing about unseen entities. (In its essentials, the methodology they produced was the hypothetico-deductive methodology, which even now remains the dominant one.) This new methodology, by providing a rationale for "micro-theorizing," eliminated what had been a major conceptual stumbling block to the acceptance of a wide range of Newtonian theories in the mid and late eighteenth century. (Here, as above, historians with purely empiricist models of science have completely missed the occurrence, let alone the significance, of these developments in the evolution of the Newtonian research tradition.)

Other cases of methodologically induced conceptual problems abound. Much of the debate about uniformitarian geology, much of the controversy about atomism, the bulk of the opposition to psychoanalysis and behaviorism, and many of the quarrels in quantum mechanics focus upon the methodological strengths and weaknesses of the scientific theories in question. Cases of this kind make it clear that the recognition of normative conceptual problems is a much more potent force in the historical evolution of science than some historians of science have recognized.

But if historians have sometimes underestimated the importance of such conceptual problems, their culpability is insignificant when compared to the utter failure of philosophers to find any role for this sort of problem in their accounts of scientific change. Even those philosophers who have been liberal enough to find a role for metaphysics in scientific development have

completely ignored the fact that the methodology to which a scientist subscribes does, and should have, a major role to play in determining that scientist's assessment of the rational merits of competing scientific theories. If a scientist has good grounds for accepting some methodology and if some scientific theory violates that methodology, then it is entirely rational for him to have grave reservations about the theory. (It is one of the crueler ironies of recent epistemology that epistemologists themselves have never come to terms with, nor found a rationale for, the decisive role which epistemology and methodology have enjoyed in the rational development of the sciences.)

Worldview difficulties. The third type of external conceptual problem arises when a particular scientific theory is seen to be incompatible with, or not mutually reinforcing for, some other body of accepted, but *prima facie* nonscientific, beliefs. Within any culture, there are widely accepted beliefs which go beyond the scientific domain. Although the exact proportion of scientific and nonscientific propositions within the total population of reasonable beliefs changes with time, there has never been a period in the history of thought when the theories of science exhausted the domain of rational belief. What I am calling worldview difficulties are like intra-scientific difficulties, except that here the inconsistency, or lack of mutual reinforcement, is not *within* the framework of science itself, but rather between science and our "extra-scientific beliefs." Such beliefs fall in areas as diverse as metaphysics, logic, ethics and theology.

For example, one of the central conceptual problems confronting the Newtonians in the eighteenth century concerned the ontology of forces. How, critics such as Leibniz and Huygens had asked, can bodies exert force at points far removed from the bodies themselves? What substance carries the attractive force of the sun through 90 million miles of empty space so that the earth is pulled towards it? How, at the more prosaic level, can a magnet draw towards itself a piece of iron several inches away? Such phenomena seemed to defy the very logic of speaking about substances and properties since properties (e.g., the power of attraction) seemed to be capable of disentangling themselves from the material bodies of which they were the properties. As Buchdahl,[16] Heimann and McGuire[17] have convincingly argued, sorting out this issue became one of the central philosophical and scientific problems of the Enlightenment. Not satisfied with the Cotesian denial that this was an acute conceptual problem (Cotes was prepared to say that nature was generally unintelligible and that the unintelligibility of distance forces was no particular source of cognitive concern[18]), philosophers and scientists all over Europe began to re-evaluate such traditional issues as the nature of substance, the relations of properties to substances, and, particularly, the nature of our knowledge of substance. What resulted from this reappraisal at the hands of Kant, Priestley, Hutton, and others was a new ontology which argued for the priority of force over matter and which made the powers of activity (rather than passive powers like mass and inertia) into the basic building blocks of the physical world. The emergence of this new ontology did several things at once: it eliminated the most acute conceptual problem for Newtonian science by exhibiting the "intelligibility" of action-at-a-distance; it brought the ontology of philosophy and the ontology of physics back into harmony; and it made possible the subsequent emergence of theories of the physical field.[19]

Those "positivist" philosophers and historians of science who see the progress of science entirely in empirical terms have completely missed the huge significance of these developments for science as well as for philosophy. Convinced that metaphysics is foreign, even alien, to the development of scientific ideas, they have written about the history of Newtonianism without even perceiving the vital bearing of these metaphysical controversies on the historical career of Newtonian doctrines.

Traditionally, worldview difficulties have tended to emerge most often as a result of tensions between science, on the one hand, and either theology, philosophy or social theory, on the other hand.[20] It is well known, for instance, that one of the major difficulties for the mechanistic scientific program of the seventeenth and eighteenth centuries was the perceived discrepancy between a theory which reduced the cosmos to a self-operating machine and certain "activist" theologies which

sought to preserve an important role to God in the day-to-day maintenance of the universe. The famous *Leibniz-Clarke Correspondence,* one of the major documents of the early Enlightenment, is replete with controversies that illustrate what I call world-view difficulties. Similarly, one major stumbling block to the emergence of evolutionary theory was the conviction, based on the best available philosophical insights, that species must be separate and distinct.[21] More recently, one of the most persistent set of conceptual problems in twentieth century physics has been the dissonance between quantum mechanics and our "philosophical" beliefs about causality, change, substance and "reality."

It is not only incompatibilities between science and philosophy or between science and theology which can lead to world-view difficulties. Conflicts with a social or moral ideology can produce similar tensions. In our own time, for instance, there are several instances where seemingly serious arguments have been lodged against a scientific theory because of moral or ethical worldview difficulties. In the Soviet Union, the Lysenko affair is a case in point. Because evolutionary biology, with its denial of the transmission of acquired characteristics, ran counter to the Marxist view that man's very nature could be changed by his environment, there were strong reservations voiced against Darwinism and Mendelism and much support was given to a scientific research effort like Lysenko's which sought to find scientific evidence for the Marxist philosophy of man. In the West, similar constraints have recently confronted researchers and theorists examining the possibility of racial differences. It has been suggested that any scientific theory which would argue for differences of ability or intelligence between the various races must necessarily be unsound, because such a doctrine runs counter to our egalitarian social and political framework.

There is a prominent group of thinkers in contemporary science and philosophy who have argued that world view difficulties are only pseudo-problems.[22] They claim that scientific theories can stand alone and that any element of our worldview which does not square with science should simply be abandoned. I shall take issue in the next chapter with this positivistic doctrine, but for now, I should make a few disclaimers, lest I be taken for asserting more than I am:

1. It is *not* my claim that a scientific theory should necessarily be abandoned when it encounters worldview problems; in asserting the existence of conceptual problems of this type, I am only asserting the *fact* that a tension often exists between our "scientific" beliefs and our "nonscientific" ones, and that such a tension does pose a problem for *both* sets of beliefs. How that tension is to be resolved depends on the particularities of the case.

2. It is *not* my claim that every worldview problem constitutes a serious ground for reservations about a scientific theory. How serious the problem is for the theory depends upon how well entrenched the nonscientific belief is and upon what problem-solving capabilities we would lose by abandoning it.

The Relative Weighting of Conceptual Problems

Having examined in a little more detail how conceptual problems are generated, we can now think about how to assess their relative importance. It is vital to stress, at the outset, that a conceptual problem will, in general, be a *more* serious one than an empirical anomaly. No one, for instance, proposed abandoning Newtonian mechanics when it could not accurately predict the motion of the moon. But many thinkers (such as Leibniz, Huygens, and Wolff) were seriously prepared to dismiss Newtonian physics because its ontology was incompatible with the accepted metaphysics of the day. This difference in weighting arises, not because science is more rationalistic than empirical; but rather because it is usually easier to explain away an anomalous experimental result than to dismiss out of hand a conceptual problem.[23] (Let me add that I am *not* suggesting that all conceptual problems are more important than all empirical problems. I am rather making the more modest claim that most conceptual problems are of greater moment than most empirical anomalies.)

Within the domain of conceptual problems, there are certain circumstances which tend to promote or demote the initial

importance of such problems. There are at least four situations which should be distinguished here:

1. As we have already seen, the nature of the logical relation between two theories exhibiting a conceptual problem can vary enormously from inconsistency (in its most acute form) to mutual support. Other things being equal, the greater the tension between two theories, the weightier the problem will be.

2. When a conceptual problem arises as a result of a conflict between two theories, T_1 and T_2, the seriousness of that problem for T_1 depends on how confident we are about the acceptability of T_2. If T_2 has proven to be extremely effective at solving empirical problems and if its abandonment would leave us with many anomalies, then matters are very difficult for the proponents of T_1. If, on the other hand, T_2's record as a problem solver is very modest, then T_2's incompatibility with T_1 will probably not count as a major conceptual problem for T_1.

3. Another case in which it becomes meaningful to speak of the grading of conceptual problems on a scale of importance occurs when—within a particular scientific domain—we have two *competing* (as opposed to complementary) theories, T_1 and T_2. If both T_1 and T_2 exhibit the *same* conceptual problem(s), then those problems count no more against one than against the other and become relatively insignificant in the context of comparative theory appraisal. However, if T_1 generates certain conceptual problems which T_2 does *not*, then those problems become highly significant in the appraisal of the relative merits of T_1 and T_2.

4. A final determinant of the importance of a conceptual problem (as with anomalies) has to do with the "age" of that problem. If it has only recently been discovered that a theory poses a certain conceptual problem (for instance, an internal inconsistency), there is usually some grounds for hope that, with very minor modifications in the theory, we can bring it into line and thus eliminate the problem. The threat which the problem poses to the theory is generally offset by an optimism that it can be readily dealt with—an optimism that is often justified. If, on the other hand, a theory has been known to have a particular conceptual problem for some length of time, if partisans of that

theory have tried, repeatedly and unsuccessfully, to make the theory consistent, or to reconcile it with our norms and our other accepted beliefs, then that problem assumes an ever greater importance with time, and assumes an ever greater significance in debates about the acceptability of the theory (or theories) which generate(s) it.

Summary and Overview

Quite simply, the claim of this chapter is that *no* major contemporary philosophy of science allows scope for the weighty role which conceptual problems have played in the history of science. Even those philosophers who claim to take the actual evolution of science seriously (e.g., Lakatos, Kuhn, Feyerabend, and Hanson) have made no serious concessions to the nonempirical dimensions of scientific debate. We now know enough about the importance of these nonempirical factors within the evolution of science to say with some confidence that *any theory about the nature of science which finds no role for conceptual problems forfeits any claim to being a theory about how science has actually evolved.*

Although the analytic machinery thus far developed is still insufficent for constructing a general model of scientific progress and growth, we now possess enough pieces of the puzzle that we can begin to talk in an approximative way about what a problem-solving model of progress might look like. The core assumptions of such a model are simple ones: (1) *the solved problem*—empirical or conceptual—*is the basic unit of scientific progress;* and (2) *the aim of science is to maximize the scope of solved empirical problems, while minimizing the scope of anomalous and conceptual problems.*

The more numerous and weightier the problems are which a theory can adequately solve, the better it is. If one theory can solve more significant problems than a competitor, then it is preferable to it. At one level, this is a noncontroversial claim. If we interpret problems exclusively in the sense of what we have called "solved empirical problems," many philosophers of science would accept that progress does amount to the solution of such problems. But, as we have seen, *there are problems in science other than solved empirical ones, specifically anomalous and conceptual problems.* My definition of progress chiefly becomes controversial (and potentially interesting) when we interpret it as applying to the latter as well as to the former. My reasons for wishing to broaden the base in this way should now be clear. If it counts in favor of a theory when it can accumulate solved empirical problems (as the standard view allows), then it should also count *against* a theory if it generates anomalous and conceptual problems. *Indeed, the problem-solving effectiveness of a theory depends on the balance it strikes between its solved problems and its unresolved problems.* How exactly does this work?

Let us begin with a very crude model of scientific evolution. Imagine some domain in which we notice a certain puzzling phenomenon, *p*. The phenomenon *p* constitutes an unsolved problem for the scientist who wishes to develop a theory, T_1, specifically with a view toward resolving *p*. Once T_1 is announced, several things are likely to happen simultaneously. Some fellow scientist may observe that T_1 predicts other phenomena in the domain besides *p*. These predictions will be tested, and, very often, some of them will not be borne out in our observation. Thus, the observation of these discrepant results will constitute one or more anomalies for T_1. At the same time, it may be pointed out that T_1 makes certain assumptions about natural processes which run counter to some of our most widely accepted theories, or that it is incompatible with our methodological norms. This will constitute one or more conceptual problems for theory T_1.

Thus far in this imaginary chronology, we are not clear whether any progress has been made. It is true that T_1 has solved its initial empirical problem, *p*, and to that extent, we can say that "progress" has been made. Unfortunately, however, the very theory, T_1, which cleared up that problem, has generated several others; in this case, anomalies and conceptual problems. It is entirely possible that more serious problems have been generated than resolved by the invention of T_1. But let us carry the example through in time for a while. Suppose that a second theorist comes along who is convinced that he can improve T_1. What does improving T_1 mean? Very roughly, such improvement would be exhibited by showing that a new theory, T_2, could explain the initial empirical problem of T_1 without generating the same, or as many, anomalies and conceptual problems as T_1 produced. If T_2 managed to do as much work at the empirical problem level as T_1 did, without all of T_1's attendant empirical and conceptual difficulties, we could all agree that it would be more reasonable to accept T_2 than to accept T_1; that, indeed, the acceptance of T_2 was progressive and that the continued espousal of T_1 was unprogressive or regressive.

Generalizing from this simple example, we could define an *appraisal measure* for a theory in the following way: *the overall problem-solving effectiveness of a theory is determined by assessing the number and importance of the empirical problems which the theory solves and deducting therefrom the number and importance of the anomalies and conceptual problems which the theory generates.*

The step from here to a rudimentary notion of scientific progress is straightforward. Given that the aim of science is problem solving (or, more precisely, the mini-max strategy sketched above), *progress can occur if and only if the succession of scientific theories in any domain shows an increasing degree of problem solving effectiveness.* Localizing the notion of progress to specific situations rather than to large stretches of time, we can say that *any time we modify a theory or replace it by another theory, that change is progressive if and only if the later version is a more effective problem solver (in the sense just defined) than its predecessor.*

There are many ways in which such progress can occur. It may come about simply by an expansion of the domain of solved empirical problems with all the other appraisal vectors remaining fixed. In such a case, the replacement of T_1 by T_2 (which solves more empirical problems) is clearly progressive. Progress can also result from a modification of the theory which eliminates some troublesome anomalies or which resolves some conceptual problems. Most often, of course, progress occurs as a result of all the relevant variables shifting subtly.

Given the exclusive emphasis by most philosophers on empirical problems, and their solution, it is important to stress

that, on the model outlined here, (1) progress can occur without an expansion of the domain of solved empirical problems, and is even conceivable when the domain of such problems contracts; and (2) a theory change may conceivably be non-progressive or *regressive,* even when the index of solved empirical problems *increases,* specifically, if the change leads to more acute anomalies or conceptual problems confronting the new theory than those exhibited by the predecessor theory.

Although an outline of a theory of cognitive progress is now emerging, there is still one crucial dimension missing. In all the talk about problem solving, there has been some confusion about what *kinds* of things solve problems. I have been using the term "theory" to designate those complexes whose problem-solving capacities must be appraised; in order to clarify the types of problems in science, I have had to postpone a discussion about what kinds of things can solve problems. We must examine that side of the problem-solving equation before the rough-hewn model of progress outlined here can be refined into a valuable tool of analysis.