Chapter Three

From Theories to Research Traditions

The intellectual function of an established conceptual scheme is to determine the patterns of theory, the meaningful questions, the legitimate interpretations...S. Toulmin (1970), p. 40

Theories are inevitably involved in the solution of problems; the very aim of theorizing is to provide coherent and adequate solutions to the empirical problems which stimulate inquiry. Theories, moreover, are designed to avoid (or to resolve) the various conceptual and anomalous problems which their predecessors generate. If one looks at inquiry in this way, if one views theories from this perspective, it becomes clear that the central cognitive test of any theory involves assessing its adequacy as a solution of certain empirical and conceptual problems. Having developed in earlier chapters a taxonomy for describing the kinds of problems which confront theories, we must now lay down adequacy conditions for determining when a theory provides an acceptable solution to the problems which confront it.

But before we embark on that task, we must clarify what theories are and how they function, for a failure to make some rudimentary distinctions here has brought grief to more than one major philosophy of science. Entire books have been devoted to the structure of scientific theory; I am attempting nothing that ambitious. Rather, I shall want to insist on only two major points with respect to an analysis of theories.

In the first place, to make explicit what has been implicit all along, the evaluation of theories is a comparative matter. What is crucial in any cognitive assessment of a theory is how it fares with respect to its competitors. Absolute measures of the empirical or conceptual credentials of a theory are of no significance; decisive is the judgment as to how a theory stacks up against its known contenders. Much of the literature in the philosophy of science has been based upon the assumption that theoretical evaluation occurs in a competitive vacuum. By contrast, I shall be assuming that assessments of theories always involve comparative modalities. We ask: is this theory better than that one? Is this doctrine the best among the available options?

The second major claim of this chapter is that it is necessary to distinguish, within the class of what are usually called "scientific theories," between two different sorts of propositional networks.

In the standard literature on scientific inference, as well as in common scientific practice, the term "theory" refers to (at least) two very types of things. We often use the term "theory" to denote a very specific set of related doctrines (commonly called "hypotheses" or "axioms" or "principles") which can be utilized for making specific experimental predictions and for giving detailed explanations of natural phenomena. Examples of this type of theory would include Maxwell's theory of electromagnetism, the Bohr-Kramers-Slater theory of atomic structure, Einstein's theory of the photoelectric effect, Marx's labor theory of value, Wegener's theory of continental drift, and the Freudian theory of the Oedipal complex.

By contrast, the term "theory" is also used to refer to much more general, much less easily testable, sets of doctrines or assumptions. For instance, one speaks about: "the atomic theory," or "the theory of evolution," or "the kinetic theory of gases." In each of these cases, we are referring not to a single theory, but to a whole spectrum of individual theories. The term
“evolutionary theory” for instance, does not refer to any single theory but to an entire family of doctrines, historically and conceptually related, all of which work from the assumption that organic species have common lines of descent. Similarly, the term “atomic theory” generally refers to a large set of doctrines, all of which are predicated on the assumption that matter is discontinuous. A particularly vivid instance of one theory which includes a wide variety of specific instantiations is offered by recent “quantum theory.” Since 1930, that term has included (among other things) quark matrix theories, group theories, so-called S-matrix theories, and renormalized field theories—between any two of which there are huge conceptual divergences.

The differences between the two types of theories outlined above are vast: not only are there contrasts of generality and specificity between them, but the modes of appraisal and evaluation appropriate to each are radically different. It will be the central claim of this chapter that until we become mindful of the cognitive and evaluational differences between these two types of theories, it will be impossible to have a theory of scientific progress which is historically sound or philosophically adequate.

But it is not only fidelity to scientific practice and usage which requires us to take these larger theoretical units seriously. Much of the research done by historians and philosophers of science in the last decade suggests that these more general units of analysis exhibit many of the epistemic features which, although most characteristic of science, elude the analyst who limits his range to theories in the narrower sense. Specifically, it has been suggested by Kuhn and Lakatos that the more general theories, rather than the more specific ones, are the primary tool for understanding and appraising scientific progress.

I share this conviction in principle, but find that the accounts hitherto given of what these larger theories are, and how they evolve, are not fully satisfactory. Because the bulk of this chapter will be devoted to outlining a new account of the more global theories (which I shall be calling research traditions), it is appropriate that I should indicate what I find chiefly wanting in the best known efforts to grapple with this problem. Of the many theories of scientific evolution that have been developed, two specifically address themselves to the question of the nature of these more general theories.

Kuhn’s Theory of Scientific “Paradigms”

In his influential Structure of Scientific Revolutions, Thomas Kuhn offers a model of scientific progress whose primary element is the “paradigm.” Although Kuhn’s notion of paradigms has been shown to be systematically ambiguous (and thus difficult to characterize accurately), they do have certain identifiable characteristics. They are, to begin with, “ways of looking at the world”; broad quasi-metaphysical insights or hunches about how the phenomena in some domain should be explained. Included under the umbrella of any well-developed paradigm will be a number of specific theories, each of which presupposes one or more elements of the paradigm. Once a paradigm is accepted by scientists (and one of Kuhn’s more extreme claims is that in any “mature” science, every scientist will accept the same paradigm most of the time), they can proceed with the process of “paradigm articulation,” also known as “normal science.” In periods of normal science, the dominant paradigm will itself be regarded as unalterable and immune from criticism. Individual, specific theories (which represent efforts “to articulate the paradigm,” i.e., to apply it to an ever wider range of cases) may well be criticized, falsified and abandoned; but the paradigm itself is unchallenged. It remains so until enough “anomalies” accumulate (Kuhn never indicates how this point is determined) that scientists begin to ask whether the dominant paradigm is really appropriate. Kuhn calls this time a period of “crisis.” During a crisis, scientists begin for the first time to consider seriously alternative paradigms. If one of those alternatives proves to be more empirically successful than the former paradigm, a scientific revolution occurs, a new paradigm is enthroned, and another period of normal science ensues.

There is much that is valuable in Kuhn’s approach. He recognizes clearly that maxi-theories have different cognitive
and heuristic functions than mini-theories. He has probably been the first thinker to stress the tenacity and persevering qualities of global theories—even when confronted with serious anomalies. He has correctly rejected the (widely assumed) cumulative character of science. But for all its many strengths, Kuhn’s model of scientific progress suffers from some acute conceptual and empirical difficulties. For instance, Kuhn’s account of paradigms and their careers has been extensively criticized by Shapere, who has highlighted the obscure and opaque character of the paradigm itself by pointing out many inconsistencies in Kuhn’s use of the notion. Feyerabend and others have stressed the historical incorrectness of Kuhn’s stipulation that “normal science” is in any way typical or normal. Virtually every major period in the history of science is characterized both by the co-existence of numerous competing paradigms, with none exerting hegemony over the field, and by the persistent and continuous manner in which the foundational assumptions of every paradigm are debated within the scientific community. Numerous critics have noted the arbitrariness of Kuhn’s theory of crisis: if (as Kuhn says) a few anomalies do not produce a crisis, but “many” do, how does the scientist determine the “crisis point”? There are other serious flaws as well. In my view, the most significant of these are:

1. Kuhn’s failure to see the role of conceptual problems in scientific debate and in paradigm evaluation. Insofar as Kuhn grants that there are any rational criteria for paradigm choice, or for assessing the “progressiveness” of a paradigm, those criteria are the traditional positivist ones such as: Does the theory explain more facts than its predecessor? Can it solve some empirical anomalies exhibited by its predecessor? The whole notion of conceptual problems and their connection with progress finds no serious explanation in Kuhn’s analysis.

2. Kuhn never really resolves the crucial question of the relationship between a paradigm and its constituent theories. Does the paradigm entail or merely inspire its constituent theories? Do these theories, once developed, justify the paradigm, or does the paradigm justify them? It is not even clear,

in Kuhn’s case, whether a paradigm precedes its theories or arises nolens volens after their formulation. Although this issue is extremely complex, any adequate theory of science is going to have to come to grips with it more directly than Kuhn has.

3. Kuhn’s paradigms have a rigidity of structure which precludes them from evolving through the course of time in response to the weaknesses and anomalies which they generate. Moreover, because he makes the core assumptions of the paradigm immune from criticism, there can be no corrective relationship between the paradigm and the data. Accordingly, it is very difficult to square the inflexibility of Kuhnian paradigms with the historical fact that many maxi-theories have evolved through time.

4. Kuhn’s paradigms, or “disciplinary matrices,” are always implicit, never fully articulated. As a result, it is difficult to understand how he can account for the many theoretical controversies which have occurred in the development of science, since scientists can presumably only debate about assumptions which have been made reasonably explicit. When, for instance, a Kuhnian maintains that the ontological and methodological frameworks for Cartesian or Newtonian physics, for Darwinian biology, or for behavioral psychology were only implicit and never received overt formulation, he is running squarely in the face of the historical fact that the core assumptions of all these paradigms were explicit even from their inception.

5. Because paradigms are so implicit and can only be identified by pointing to their “exemplars” (basically an archetypal application of a mathematical formulation to an experimental problem), it follows that whenever two scientists utilize the same exemplars, they are, for Kuhn, ipso facto committed to the same paradigm. Such an approach ignores the persistent fact that different scientists often utilize the same laws or exemplars, yet subscribe to radically divergent views about the most basic questions of scientific ontology and methodology. (For instance, both mechanists and energetics accepted identical conservation laws.) To this extent, analysing science in terms of paradigms is unlikely to reveal that “strong
network of commitments—conceptual, theoretical, instrumental, and metaphysical" which Kuhn hopes to localize with his theory of paradigms.

Lakatos' Theory of "Research Programmes"

Largely in response to Kuhn's assault on some of the cherished assumptions of traditional philosophy of science, Imre Lakatos has developed an alternative theory about the role of these "super-theories" in the evolution of science. Calling such general theories "research programmes," Lakatos argues that research programmes have three elements: (1) a "hard-core" (or "negative heuristic") of fundamental assumptions which cannot be abandoned or modified without repudiation of the research programme; (2) the "positive heuristic," which contains "a partially articulated set of suggestions or hints of how to change, . . . modify, sophisticate [sic]" our specific theories whenever we wish to improve them, and (3) "a series of theories, T₁, T₂, T₃, . . . " where each subsequent theory "results from adding auxiliary clauses to . . . the previous theory." Such theories are the specific instantiations of the general research programme. Research programmes can be progressive or regressive in a variety of ways: but progress, for Lakatos even more than for Kuhn, is a function exclusively of the empirical growth of a tradition. It is the possession of greater "empirical content," or of a higher "degree of empirical corroboration" which makes one theory superior to, and more progressive than, another.

Lakatos' model is, in many respects, a decided improvement on Kuhn's. Unlike Kuhn, Lakatos allows for, and stresses, the historical importance of the co-existence of several alternative research programmes at the same time, within the same domain. Unlike Kuhn, who often takes the view that paradigms are incommensurable and thus not open to rational comparison, Lakatos insists that we can objectively compare the relative progress of competing research traditions. More than Kuhn, Lakatos tries to grapple with the thorny question of the relation of the super-theory to its constituent mini-theories.

But against that, Lakatos' model of research programmes shares many of the flaws of Kuhn's paradigms, and introduces some new ones as well:

1. As with Kuhn, Lakatos' conception of progress is exclusively empirical: the only progressive modifications in a theory are those which increase the scope of its empirical claims.
2. The sorts of changes which Lakatos allows within the mini-theories which constitute his research programme are extremely restricted. In essence, Lakatos only permits, as the relation between any theory and its successor within a research programme, the addition of a new assumption or a semantic re-interpretation of terms in the predecessor theory. On this remarkable view of things, two theories can only be in the same research programme if one of the two entails the other. As we shall see shortly, in the vast majority of cases, the succession of specific theories within a maxi-theory involves the elimination as well as the addition of assumptions, and there are rarely successor theories which entail their predecessors.
3. A fatal flaw in the Lakatosian notion of research programmes is its dependence upon the Tarski-Popper notions of "empirical and logical content." All Lakatos' measures of progress require a comparison of the empirical content: of every member of the series of theories which constitutes any research programme. As Grünbaum and others have shown convincingly, the attempt to specify content measures for scientific theories is extremely problematic if not literally impossible.

Because comparisons of content are generally impossible, neither Lakatos nor his followers have been able to identify any historical case to which the Lakatosian definition of progress can be shown strictly to apply.

4. Because of Lakatos' idiosyncratic view that the acceptance of theories can scarcely if ever be rational, he cannot translate his assessments of progress (assuming he could make them) into recommendations about cognitive action. Although one research programme may be more progressive than another, we can, on Lakatos' account, deduce nothing from that about which research programme should be preferred or accepted. As a result, there can never be a connection between a theory
of progress and a theory of rational acceptability (or, to use Lakatos' language, between methodological “appraisal” and “advice”)

5. Lakatos’ claim that the accumulation of anomalies has no bearing on the appraisal of a research programme is massively refuted by the history of science.

6. Lakatos’ research programmes, like Kuhn’s paradigms, are rigid in their hard-core structure and admit of no fundamental changes. What should be clear, even from this very brief survey of two of the major theories of scientific change, is that there are a number of analytical and historical difficulties confronting existing attempts to understand the nature and role of maxitheories. With some of those difficulties in mind, we can turn now to explore an alternative model of scientific progress, built upon elements outlined in the previous chapters. One crucial test of that model will be whether it can avoid some of the problems which handicap its predecessors. Although there are numerous common elements between my model and those of Kuhn and Lakatos (and I readily concede a great debt to their pioneering work), there are a sufficiently large number of differences that I shall try to develop the notion of a research tradition more or less from scratch.

The Nature of Research Traditions

We have already referred to a few classic research traditions: Darwinism, quantum theory, the electromagnetic theory of light. Every intellectual discipline, scientific as well as nonscientific, has a history replete with research traditions: empiricism and nominalism in philosophy, voluntarism and necessitarianism in theology, behaviorism and Freudianism in psychology, utilitarianism and intuitionism in ethics, Marxism and capitalism in economics, mechanism and vitalism in physiology, to name only a few. Such research traditions have a number of common traits:

1. Every research tradition has a number of specific theories which exemplify and partially constitute it; some of these theories will be contemporaneous, others will be temporal successors of earlier ones;

2. Every research tradition exhibits certain metaphysical and methodological commitments which, as an ensemble, individuate the research tradition and distinguish it from others;

3. Each research tradition (unlike a specific theory) goes through a number of different, detailed (and often mutually contradictory) formulations and generally has a long history extending through a significant period of time. (By contrast, theories are frequently short-lived.) These are by no means the only important characteristics of research traditions, but they should serve, for the time being, to identify the kinds of objects whose properties I would like to explore.

In brief, a research tradition provides a set of guidelines for the development of specific theories. Part of those guidelines constitute an ontology which specifies, in a general way, the types of fundamental entities which exist in the domain or domains within which the research tradition is embedded. The function of specific theories within the research tradition is to explain all the empirical problems in the domain by “reducing” them to the ontology of the research tradition. If the research tradition is behaviorism, for instance, it tells us that the only legitimate entities which behavioristic theories can postulate are directly and publicly observable physical and physiological signs. If the research tradition is that of Cartesian physics, it specifies that only matter and minds exist, and that theories which talk of other types of substances (or of “mixed” mind and matter) are unacceptable. Moreover, the research tradition outlines the different modes by which these entities can interact. Thus, Cartesian particles can only interact by contact, not by action-at-a-distance. Entities, within a Marxist research tradition, can only interact by virtue of the economic forces influencing them.

Very often, the research tradition will also specify certain modes of procedure which constitute the legitimate methods of inquiry open to a researcher within that tradition. These methodological principles will be wide-ranging in scope, addressing themselves to experimental techniques, modes of theoretical testing and evaluation, and the like. For instance, the methodological posture of the scientist in a strict Newtonian
research tradition is inevitably inductivist, allowing for the espousal of only those theories which have been “inductively inferred” from the data. The methods of procedure outlined for a behavioristic psychologist are what is usually called “operationalist.” Put simplistically, a research tradition is thus a set of ontological and methodological “do’s” and “don’ts.” To attempt what is forbidden by the metaphysics and methodology of a research tradition is to put oneself outside that tradition and to repudiate it. If, for instance, a Cartesian physicist starts talking about forces acting-at-a-distance, if a behaviorist starts talking about subconscious drives, if a Marxist begins speculating about ideas which do not arise in response to the economic substructure; in each of these cases, the activity indicated puts the scientist in question beyond the pale. By breaking with the ontology or the methodology of the research tradition within which he has worked, he has violated the strictures of that research tradition and divorced himself from it. Needless to say, that is not necessarily a bad thing. Some of the most important revolutions in scientific thought have come from thinkers who had the ingenuity to break with the research traditions of their day and to inaugurate new ones. But what we must preserve, if we are to understand either the logic or the history of the natural sciences, is the notion of the integrity of a research tradition, for it is precisely that integrity which stimulates, defines and delimits what can count as a solution to many of the most important scientific problems.19

Although it is vital to distinguish between the ontological and the methodological components of a research tradition, the two are often intimately related, and for a very natural reason: namely, that one’s views about the appropriate methods of inquiry are generally compatible with one’s views about the objects of inquiry. When, for instance, Charles Lyell defined the “uniformitarian” research tradition in geology, his ontology was restricted to presently acting causes and his methodology insisted that we should “explain past effects in terms of presently acting causes.” Without a “presentist” ontology, his uniformitarian methodology would have been inappropriate; and without the latter, the presentist ontology would not have allowed Lyell to explain the geological past. Similarly, the mathematical ontology of the Cartesian research tradition (an ontology which argued that all physical changes were entirely changes of quantity) was very closely connected with the (mathematically inspired) deductivist and axiomatic methodology of Cartesianism. As we shall see later, it does not always happen that the ontology and methodology of a research tradition are so closely intertwined (for instance, the inductivist methodology of the Newtonian research tradition had only the weakest of connections with that tradition’s ontology), but such cases are the exception rather than the rule.

So a preliminary, working definition of a research tradition could be put as follows: a research tradition is a set of general assumptions about the entities and processes in a domain of study, and about the appropriate methods to be used for investigating the problems and constructing the theories in that domain.

Theories and Research Traditions

Every research tradition will be associated with a series of specific theories, each of which is designed to particularize the ontology of the research tradition and to illustrate, or satisfy, its methodology. The mechanistic research tradition in seventeenth century optics, for example, includes several of Descartes’ theories as well as the optical theories of Hooke, Robault, Hobbes, Regis, and Huygens.20 The phlogistic tradition in eighteenth century chemistry received more than a dozen specific theoretical formulations.21 Many of the theories within any evolving research tradition will be mutually inconsistent rivals, precisely because some theories represent attempts, within the framework of the tradition, to improve and correct their predecessors.

The individual theories constituting the tradition will generally be empirically testable for they will entail (in conjunction with other specific theories) some precise predictions about how objects in the domain will behave. By contrast, research traditions are neither explanatory, nor predictive, nor directly
testable. Their very generality, as well as their normative elements, precludes them from leading to detailed accounts of specific natural processes.

Except at the abstract level of specifying what the world is made of, and how it should be studied, research traditions do not provide detailed answers to specific questions. A research tradition will not tell us what happens to light when it is refracted at an interface between water and air; it will not tell us what happens if we put an eight-month-old female mouse into a maze; it will not tell us why lead melts at a lower temperature than copper. But it would be a mistake to conclude from the fact that research traditions do not offer solutions to specific problems that they are outside of the problem-solving process. To the contrary, the whole function of a research tradition is to provide us with the crucial tools we need for solving problems, both empirical and conceptual. (As we shall see later, the research tradition even goes so far as to define partially what the problems are, and what importance should be attached to them.) It is for just this reason that the objective evaluation of any research tradition is crucially linked with the problem-solving process. The very idea that an entity like a research tradition—which makes no predictions, which solves no specific problems, which is fundamentally normative and metaphysical—could be objectively evaluated may seem paradoxical. But nothing could be further from the case, for we can say quite simply that a successful research tradition is one which leads, via its component theories, to the adequate solution of an increasing range of empirical and conceptual problems. Determining whether a tradition is successful in this sense does not mean, of course, that the tradition has been "confirmed" or "refuted." Nor can such an appraisal tell us anything about the truth or falsity of the tradition.  

A research tradition may be enormously successful at generating fruitful theories and yet flawed in its ontology or methodology. Equally, one can conceive that a research tradition might be true, and yet (perhaps because of the unimaginativeness of its proponents) unsuccessful at generating theories which were effective problem solvers. Hence to abandon or reject a research tradition is not (or ought not be) to judge that tradition false. Nor, in rejecting a research tradition as momentarily unsuccessful, are we necessarily relegating it to permanent oblivion; to the contrary, we can explicitly stipulate conditions which, if satisfied, would revive and recussitate it. Thus, when we reject a research tradition, we are merely making a tentative decision not to utilize it for the moment because there is an alternative to it which has proven to be a more successful problem solver.

Just as the fortunes of a research tradition are linked closely to the problem-solving effectiveness of its constituent theories, so too is the determination of the adequacy of a specific theory inextricably bound up with an assessment of the problem-solving effectiveness of the entire set of theories spawned by the research tradition of which that theory is a part.  

If a theory is closely linked to an unsuccessful research tradition, then—whatever the problem-solving merits of that particular theory—it is likely to be regarded as highly suspect. For instance, Count Rumford's theories of heat conduction and convection were far superior to any alternative theories of thermal flow in fluids available in the period from 1830 to 1815. Nonetheless, few scientists took Rumford's theories seriously because (as they saw it) the research tradition in which Rumford worked (deriving from Boerhaave) had been discredited by the emergence of rival research traditions in chemistry (especially Joseph Black's), which suggested that heat was a substance rather than, as Rumford imagined, the random motion of particles. Rumford's specific theories only became fashionable in the 1840s and 1850s because by that time the balance between various research traditions had shifted sufficiently that many scientists were more prepared to consider specifically specific theories (like Rumford's) which grew out of a kinetic research tradition.

Contrariwise, a theory, even an inadequate one, will have some strong arguments in its favor if it is linked with a research tradition that is otherwise highly successful. Thus, theories of mechanistic physiology in the late seventeenth century (such as those of Borelli and Pitcairn) were highly regarded in many circles where the mechanistic research tradition was flourishing.
even though, judged entirely on their own merits, they were significantly inferior to certain theories in other, less successful research traditions.2

Up to this point, I have been deliberately vague about describing the kind of relation which exists between a theory and its "parent" research tradition. I have spoken of research traditions "inspiring" or "containing" or "generating" theories, and about theories "presupposing" or "constituting" or even "defining" research traditions. This is an extremely complex matter; the ambiguity of the metaphors I have invoked to characterize the theory/research tradition connection is a symptom of the difficulty of tackling this problem head-on.

But that task cannot be further postponed. I shall begin by saying what the relation between theories and research traditions is not. It is not, for instance, one of entailment. Research traditions do not entail their component theories; nor do those theories, taken either singly or jointly, entail their parent research traditions. One might wish it were otherwise, for then it would be a simple matter to determine mechanically which theories belonged to any given research tradition, or the research tradition(s) lurking behind any theory. But to see the theory/research tradition connection in such formal terms is completely to misunderstand the differences in kind between the two. A research tradition, at best, specifies a general ontology for nature, and a general method for solving natural problems within a given natural domain. A theory, on the other hand, articulates a very specific ontology and a number of specific and testable laws about nature. To be told, as the Newtonian research tradition in mechanics tells us, that we should treat all nonrectilinear motions as cases of centrally directed forces, does not entail any specific theory about how to explain, say, the motion of a compass needle in the vicinity of a current-carrying wire. To develop a "Newtonian" theory for that particular phenomenon, we must (as Ampère did) go far beyond the deductive consequences of the Newtonian research tradition. To be told, as the nineteenth-century "mechanical" research tradition tells us, that heat is simply a form of motion, does not deductively lead us to Boltzmann's version of the kinetic theory of gases or to statistical thermodynamics.

Similar considerations apply to the reverse relation between theories and research traditions. For example, given the theory of impact as developed by Huygens, we cannot deduce the basic assumptions of the research tradition within which Huygens worked. (We may, of course, be able to deduce that Huygens was working in a research tradition in which collision phenomena constituted an important unsolved problem, for if not, why should Huygens have bothered working out a theory of collision?). But it is never possible to deduce the whole of a research tradition from one, or even all, of the theories allied to it.

The reason why entailment will not help here is very simple: there are a number of mutually inconsistent theories which can claim allegiance to the same research tradition, and there are a number of different research traditions which can, in principle, provide the presuppositional base for any given theory.

Examples of both phenomena abound: many scientists in the Cartesian optical tradition argued that light traveled faster in optically denser media; other theorists, within the same tradition, asserted the converse. Staying within the history of optics, there are numerous examples of competing research traditions claiming to justify the same theory. For instance, Newton's theory that light has certain periodic properties was accepted alike by scientists in the wave and corpuscular traditions. If entailment were the relation between research traditions and theories, then it would be impossible for such situations to arise. Since the relation we are trying to explore is evidently not one of entailment, what can we say positively about it?

There are at least two specific modes by which theories and research traditions are related: one is historical, the other is conceptual. It is a matter of historical fact that most if not all of the major theories of science have emerged when the scientist invented them was working within one or another specific research tradition. Boyle's theory of gases developed within the framework of the mechanical philosophy. Buffon's embryological theories were developed as efforts to apply the Newtonian research tradition to biological phenomena. Hertz's theories of sensation were developed within the research tradition of associationist psychology. Hertz's electrical theories were linked in important ways with the Maxwellian research tradition.
A specific theory, abstracted from its historical context, may not give unambiguous clues as to the research tradition (or traditions) with which it is associated. It is just this fact which has led many scientists and philosophers to imagine that theories are usually appraised and assessed independently of the research traditions of which they are a part. But we should not be misled by the fact that a theory, taken abstractly, does not have its "parent" research tradition stamped all over it. Historical research can always (at least in principle) identify the research tradition(s) with which a particular theory has been associated. In this sense, the connection between a theory and a research tradition is as real as any fact of the past, and it is as important as the most important facts of the past. In order to see how important these connections are, we need to look at the ways in which theories and research traditions can interact.

The most important modes of interaction are generally influences of the research tradition upon its constituent theories. These influences take a variety of forms:

1. Among the other roles of a research tradition, it is designed to delimit, at least partially and in outline, the domain of application of its constituent theories. It does this by indicating that it is appropriate to discuss certain classes of empirical problems in the given domain, whilst others belong to foreign domains, or are "pseudo-problems" which can be legitimately ignored. Either the ontology or the methodology of the research tradition can influence what are to count as legitimate problems for its constituent theories. If, for instance, the methodology of a research tradition specifies—as it usually

will—certain experimental techniques which alone are the legitimate investigational modes for determining what are the data to be explained, then it is clear that only "phenomena" which can be explored by those means can, in principle, count as legitimate empirical problems for theories within that tradition. A classic example of this process is offered by nineteenth-century phenomenological chemistry. Scientists in this tradition argued that the only legitimate problems to be solved by the chemist were those which concerned the observable reactions of chemical reagents. Thus, to ask how this acid and this base react to form this salt is to pose an authentic problem. But to ask how atoms combine to form diatomic molecules cannot conceivably count as an empirical problem because the methodology of the research tradition denies the possibility of empirical knowledge of entities the size of atoms and molecules. For other research traditions in nineteenth-century chemistry, questions about the combining properties of certain entities not directly observable constituted authentic problems for empirical research.19 (Contemporary behavioristic psychology and quantum mechanics likewise have methodologies which strongly preclude from consideration as problems certain "phenomena" which other research traditions countenance.)

Similarly, the ontology of a research tradition may exclude certain situations from, or include them within, the appropriate domain. Thus, the rise of the Cartesian mechanistic research tradition in the seventeenth century radically transformed the accepted problem domain for optical theories. It did so by arguing, or rather by simply postulating, that problems of perception and vision—problems which had classically been regarded as legitimate empirical problems for any optical theory—should be relegated to psychology and to physiology, fields outside the domain of optics, so that such empirical problems could be safely ignored by the mechanistic optical theorist.

A different kind of example is provided by late nineteenth-century physics, where the subtle fluid tradition (of Faraday, Maxwell, Hertz, and others) countenanced as legitimate empirical problems queries about the properties of the electromagnetic aether. Indeed, the classic Michelson-Morley experiments
were originally conducted in order to determine the drag coefficient of bodies moving through such an aether. With the emergence of special relativity theory, however, a new research tradition and its related ontology cut out from the domain of the empirical problem of physics all questions about the elasticity, density, and velocity of the aether—questions which had been central empirical problems between 1850 and 1900. These few examples should make it clear that research traditions can play a decisive role in specifying the sorts of things that are to count as potentially solvable empirical problems for their constituent theories.

2. Equally important is the way in which a research tradition can generate conceptual problems for its constituent theories. Indeed, the bulk of the conceptual problems which any theory may face will arise because of tensions between that theory and the research tradition of which it is a part. It often happens that the detailed articulation of a theory will lead to the adoption of assumptions which run counter to those allowed by the research tradition of that theory. In such a situation, it is commonplace for critics of the theory to point to such a tension as a major conceptual problem for it. When, for instance, Huygens came to develop a general theory of motion, he found that the only empirically satisfactory theories were those which assumed vacua in nature. Unfortunately, Huygens was working squarely within the Cartesian research tradition, a tradition which identified space and matter and thus forbade empty spaces. As Leibniz and others pointed out to Huygens, his theories were running counter to the research tradition which they claimed to instantiate. This was an acute conceptual problem of the first magnitude, as Huygens himself sometimes acknowledged. Similarly, when Thomas Young—working within the Newtonian optical research tradition—found himself offering explanations for optical interference which presupposed a wave-theoretic interpretation of light, he was chastised for not recognizing the extent to which his wave theory violated certain canons of the research tradition to which he seemingly paid allegiance. Here again, we can see how the dissonance between a research tradition and its component theories can generate acute conceptual problems.

The constraining role of research traditions. As we have already said, it is the primary function of a research tradition to establish a general ontology and methodology for tackling all the problems of a given domain, or set of domains. As such, it acts negatively as a constraint on the types of theories which can be developed within the domain. If the ontology of the research tradition denies the existence of forces acting at a distance, then it clearly rules out as unacceptable any specific theory which relies on noncontact action. It was precisely for this reason that "Cartesians" such as Huygens and Leibniz (committed to an ontology of pushes and pulls) found Newton's theory of celestial mechanics so obtuse. Einstein's theory of the equivalence of matter and energy excludes from consideration any specific theory which postulates the absolute conservation of mass. The mechanistic tradition in heat theory (with its corollary that heat can be turned into work) precludes the development of theories which assume the materiality of heat, or heat conservation.

There are also many occasions where the methodology of a research tradition rules out certain sorts of theories. For instance, any research tradition which has a strongly inductivist or observationalist methodology will regard as inadmissible "specific" theories postulating entities which cannot be observed. Much of the opposition to subtle fluid theories in the eighteenth century and to atomic theories in the nineteenth century was due to the fact that the dominant methodology of the period denied the epistemic and scientific well-foundedness of theories which dealt with "unobservable entities."

In all these cases, the research tradition within which a scientist works precludes him from adopting specific theories which are incompatible with the metaphysics or methodology of the tradition.

Thus far, we have focussed attention primarily on the negative manner in which research traditions exclude certain problems and theories. They also have, however, two very positive functions...

The heuristic role of research traditions. Precisely because they postulate certain types of entities and certain methods for
investigating the properties of those entities, research traditions can play a vital heuristic role in the construction of specific scientific theories. Not, of course, because theories can in any sense be deduced from research traditions; but rather because research traditions can provide vital clues for theory construction. Consider the case of Benjamin Franklin and his efforts to articulate a theory of static electricity. Franklin was familiar with certain phenomena (particularly, electrification by friction, electrosopes, and the Leyden jar). Working within a research tradition which postulated the existence of electrical matter, Franklin needed a theory which could explain how friction electrifies bodies, how electrical bodies could attract and repel, how electricity could be stored in a condensor, and why certain bodies were conductors and others were insulators. In the early stages of the development of his theory, Franklin came to the view that positive electrification consisted in the accumulation within bodies of an excess amount of this electrical fluid, while negative electrification was caused by a deficiency of this fluid. If these specific theoretical assumptions are linked together with the ontology of his research tradition, an ontology which postulated that electricity was a form of matter and therefore conserved in the same way that ordinary matter was, it became natural to assume that electrical charge must be conserved. This important theoretical insight, subsequently confirmed in Franklin's experiments, emerged as an almost inevitable result of Franklin's thinking about the relations between his emerging theory and its parent research tradition. It did not follow logically from either the early theory itself, nor from the research tradition. It was the juxtaposition of the two that made possible this vital theoretical extension.

A different sort of heuristic role is illustrated by the early history of thermodynamics. When Sadi Carnot set out to develop a theory of steam engines, he sought to do so within the research tradition of the caloric doctrine of heat. Within this tradition, heat was conceived as a material, conserved substance capable of moving between the constituent parts of macroscopic bodies. Carnot, familiar with the work that could be performed by such simple mechanical systems as a water wheel, tried to conceive of heat flow on analogy with the fall of water, with the temperature gradient between input and output corresponding to the top and bottom heights of the waterfall. It is in terms of this analogy that Carnot develops the "proof" of his theory. It is clear that, if Carnot had not conceived of heat as a conserved substance capable of flowing from one point to another without loss of its quantity, he almost certainly would not have enunciated his theory. But that way of conceiving heat was a natural result of the research tradition within which Carnot worked.

One final example may make the point still clearer. When Descartes attempted to develop a theory of light and colors, he had already defined his general research tradition. In brief, it amounted to the assertion that the only properties which bodies can have are those of size, shape, position, and motion. The research tradition did not, indeed could not, specify precisely what sizes, shapes, positions, and motions particular bodies could exhibit. But it did make it clear that any specific physical theory, in optics or elsewhere, would have to deal exclusively with these four parameters. As a result, Descartes knew—when he set out to explain optical refraction, the colors of the rainbow, and the path of light through lens and prisms—that his optical theories would have to be constructed along such lines. So, he sought to explain colors in terms of the shape and rotational velocity of certain particles; he explained refraction in terms of differential velocities of such particles in different media. Moreover, since his general research tradition made it clear that particles of light are exactly like other material bodies, he recognized that he could apply general mechanical theorems (such as the laws of impact and the principle of conservation of motion) to a theoretical analysis of light. Again, none of his theories followed logically from his research tradition; but, in the ways indicated, that research tradition directed the construction of Cartesian theories in a number of subtle and important ways.

In all the cases mentioned thus far, the research tradition functions heuristically to suggest an initial theory for some domain. A second important heuristic role for the research tradition, as Lakatos has pointed out, arises when one of its constituent theories requires modification (because of its lack of
problem-solving ability). Any sound research tradition will contain significant guidelines about how its theories can be modified and transformed, so as to improve its problem-solving capacity.

For instance, when early versions of the kinetic theory of gases were confronted by serious predictive failures, there was enormous “flexibility” within the research tradition which pointed the way towards natural modifications that might be made. If more degrees of freedom were needed to accommodate seeming energy losses, kineticists could introduce molecular spin or alter their assumptions about molecular elasticities. If gases did not condense in accordance with theoretical predictions, the addition of weak intermolecular attractions could do the job. All these, and many similar “gambits” emerge quite plausibly from regarding matter as possessing a molecular and mechanical composition.24

The justificatory role of research traditions. It is one of the important functions of research traditions to rationalize or to justify theories. Specific theories make many assumptions about nature, assumptions which are generally not justified either within the theory itself or by the data which confirm the theory. These are usually assumptions about basic causal processes and entities, whose existence and operation the specific theories take “as given.” When, for instance, Sadi Carnot developed his theory of the steam engine, his working out of that theory presupposed that no heat was lost in performing the work of driving a piston. (That assumption later turned out to be unacceptable, of course; but it is an assumption which is absolutely crucial to Carnot’s “proof” of his theory.) Carnot offered no rationale for that assumption, and, quite rightly, felt no need to; the caloricist research tradition, within which he was working, laid it down as a primary postulate that heat was always conserved. Carnot was thus able to presuppose, in developing his theory, certain things about nature which his theory could not itself establish, not even in principle.

A century earlier, when Stephen Hales had developed his theory about the nature of “air” (i.e., gases), he was able to take it almost for granted that gases were composed of mutually repellent particles, and was able to use repulsion to explain such phenomena as elasticity and gaseous mixing. Had Hales been working in research traditions other than the Newtonian one, such an assumption would have been unthinkable, or at least in need of elaborate justification. (At a minimum, his theory would have had to address itself to justifying that assumption.) But, as a Newtonian, Hales could assume, almost without argument, that it was appropriate and legitimate to conceive of gases as swarms of mutually repellent particles. By sanctioning certain assumptions in advance, the research tradition thus frees the scientist working within it from having to justify all of his assumptions, and gives him the time to pursue specific problems of interest. Although critics outside the research tradition may fault a scientist for constructing theories based on such assumptions, the scientist knows that his primary audience—fellow researchers within the same tradition—will not find his working assumptions problematic.

Research traditions thus identify for the scientist working within them three classes of assumptions: those which are unproblematic, because justified by the research tradition; those which are forbidden by the research tradition; and, of course, those which, while not forbidden by the research tradition, definitely require a rationale within the theory (for the research tradition itself provides no rationale for them). Among scientists working within any one research tradition, there will be a broad consensus about where any given statement falls as between those three pigeonholes.

Summing up the discussion thus far, we have seen that such research traditions can justify many of the assertions which their theories make; they can serve to stamp certain theories as inadmissible because they are incompatible with the research tradition; they can influence the recognition and weighting of empirical and conceptual problems for their component theories, and they can provide heuristic guidelines for the generation or modification of specific theories.

The Separability of Theories from Research Traditions

Up to now, I have stressed that virtually all theoretical activity takes place within the context of a research tradition, that such traditions constrain, inspire, and serve to justify the
theories which are subsumed under them. Without wishing to
negate any of that, it is equally important to recognize that
there are circumstances in which theories can break away from
the research traditions which initially inspired or justified them.
Galileo's theory of fall, for instance, has (since the 1650s) been
treated separately from the Galilean research tradition; similar
things could be said about Pasteur's theory of disease,
Maxwell's theory of electromagnetism, Lavoisier's theory of
oxidation, and Planck's theory of black-body radiation, to name
only a few cases. Indeed, it is just the eventual possibility of
separating a theory from a given research tradition which gives
the misleading impression that theories exist independently of,
and owe nothing to, research traditions.

This process of theory separation is a fascinating one and
deserves to be studied in some detail. I shall limit my remarks
here to pointing out that the separation of a theory from its
parent research tradition generally takes place only when that
type theory can be taken over, either intact or by small-scale modi-
fications in it, by an alternative research tradition. Theories
rarely can exist on their own, and even when they do it is only
for short periods of time. The reasons for this are clear.

Theories are never self-authenticating; they invariably make
assumptions about the world for which they provide no
rational. Since it is one of the functions of a research tradition
to provide just such a rationale for a theory, it is normally the
case that a theory is separated from one research tradition only if
it can be absorbed (i.e., justified) within another and more
successful research tradition.

The doctrines of early thermodynamics, to which we have
referred earlier, are a case in point. Originally developed within
a caloricist research tradition (based on substantial, nonkinetic
theories of heat) by Carnot and Clapeyron, the theory of
thermodynamics proved an embarrassment during the late
1840s and 1850s, by which time the research tradition that
inspired it had been largely discredited. There was wide
agreement that the theory of thermodynamics was worth
preserving, but not (many felt) at the price of subscribing to the
research tradition which had generated it. At the same time, the
kinetic, anti-caloric research tradition was making great strides
forward in other domains, but was thought to be weak insofar
as it had been unable to equal, within the area of thermo-
dynamics, the successes which its competitor, the caloric
tradition, had achieved. It was Rudolf Clausius, writing in the
1850s, who was able to show that the theory of thermodynamics
could be developed and rationalized within the kinetic tradition,
indisputably of the caloricist assumption of the conservation
of heat. Clausius thereby showed that the theory of thermo-
dynamics was not inexorably wedded to the caloricist research
tradition and could be absorbed by the kineticist tradition. In
one fell swoop, Clausius thus managed to strengthen the case
for both thermodynamics and for kineticism, by removing what
had been a serious conceptual problem for them both. In like
manner, Newton (as a vehement opponent of the Cartesian
research tradition) was able to show that his own research
tradition could absorb the Huygensian theory of impact—a
theory which had originally been developed squarely within the
Cartesian tradition.

The multitude of cases one could cite of this process ought
not lead us to underestimate its difficulty. Precisely because a
research tradition plays an important justificatory role for its
constituent theories, any alternative research tradition which is
to play the same role must be sufficiently rich conceptually, and
its partisans sufficiently imaginative, to allow it to justify and
rationalize theories which prima facie are more naturally related
to very different metaphysical and methodological traditions. (I
shall have more to say later about this process of "theory
appropriation," for it is one of the most important ways in
which new research traditions establish their scientific cre-
dentials.)

The Evolution of Research Traditions

Research traditions, as we have seen, are historical creatures.
They are created and articulated within a particular intellectual
milieu, they aid in the generation of specific theories—and like
all other historical institutions—they wax and wane. Just as
surely as research traditions are born and thrive, so they die,
and cease to be seriously regarded as instruments for furthering
the progress of science. I shall consider below how research traditions are displaced by other ones, for the aetiology of research tradition "decay" and "putrefaction" is crucial to the processes which must be understood. For now, however, I want to talk about the ways in which important and substantive changes can occur within an on-going research tradition. These changes take two distinct forms.

The most obvious way in which a research tradition changes is by a modification of some of its subordinate, specific theories. Research traditions are continuously undergoing changes of this type. Researchers in the tradition often discover that there is, within the framework of the tradition, a more effective theory for dealing with some of the phenomena in the domain than they had realized previously. Slight alterations in previous theories, modifications of boundary conditions, revisions of constants of proportionality, minor refinements of terminology, expansion of the classificatory network of a theory to encompass newly discovered processes or entities; these are but a few of the many ways in which the scientist may seek to improve on the problem-solving success of any of the theories within the research tradition. Whenever he discovers a theory which is a significant improvement on its predecessor he drops the latter immediately. Precisely because a scientist's cognitive loyalties are based primarily in the research tradition rather than in any of its specific theories, he generally has no rational vested interest in hanging onto those individual theories. (It is for just this reason that most individual theories have very short life-spans—in many cases amounting to no more than a few months or even weeks.) Because theories change so rapidly, the history of any flourishing research tradition will exhibit a long succession of specific theories.

But there is another important way in which research traditions evolve; this second class of changes involves, not the specific theories within the research tradition, but a change of some of its most basic core elements. I must discuss this type of transformation in some detail, since there are many philosophers who have denied that research traditions are capable of any significant internal modification. Both Kuhn and Lakatos, for instance, usually suggest that entities such as research traditions have a rigid and unchanging set of doctrines which identify and define them. Any change in those doctrines, it is suggested, produces a different research tradition. Since, Lakatos argues, we define a research tradition or research programme in terms of its central doctrines (doctrines which Lakatos argues we make true by fiat or by convention), any change in those central tenets is de facto the abandonment of the research tradition which was defined as the set of those tenets. As tempting as this approach is (for, if true, it would make the process of identifying research traditions relatively straightforward), I shall be arguing that we must reject it, for it can only obfuscate our effort to get some understanding of the historical processes of science.

If one looks at the great research traditions in the history of scientific thought—Aristotelianism, Cartesianism, Darwinism, Newtonianism, Stahlian chemistry, mechanistic biology, or Freudian psychology, to name only a few—one can see immediately that there is scarcely any interesting set of doctrines which characterizes any one of these research traditions throughout the whole of its history. Certain Aristotelians, at times, abandoned the Aristotelian doctrine that motion in a void is impossible. Certain Cartesianists, at times, repudiated the Cartesian identification of matter and extension. Certain Newtonians, at times, abandoned the Newtonian demand that all matter has inertial mass. But need it follow that these seeming "renegades" were no longer working within the research tradition to which they earnestly claimed to subscribe? Does Thomas Aquinas cease to be an Aristotelian because he rejects portions of Aristotle's analysis of motion? Does Huygens become a non-Cartesian because he admits the possibility of void spaces? Certain advantages will accrue if we can plausibly answer these questions negatively. To show how that is possible is the task before us.

A research tradition, we have said, is a set of assumptions: assumptions about the basic kinds of entities in the world, assumptions about how those entities interact, assumptions about the proper methods to use for constructing and testing theories about those entities. In the course of their development, research traditions and the theories they sponsor
encounter a number of problems; anomalies are discovered; basic conceptual problems arise. In some cases, proponents of a research tradition will find themselves unable, by modifying specific theories within the tradition, to eliminate these anomalous and conceptual problems. In such circumstances, it is common for partisans of a research tradition to explore what sorts of (minimal) changes can be made in the deep-level methodology or ontology of that research tradition to eliminate the anomalies and conceptual problems confronting its constituent theories. Sometimes, scientists will find that there is no amount of tinkering with one or another assumption of the research tradition which will eliminate its anomalies and conceptual problems. This becomes strong grounds for abandoning the research tradition (provided there is some alternative in sight). But, perhaps more often, scientists find that by introducing one or two modifications in the core assumptions of the research tradition, they can both solve the outstanding anomalies and conceptual problems and preserve the bulk of the assumptions of the research tradition in tact.

In the latter case, it is positively misleading to speak of the creation of a "new" research tradition, for such language conceals from us the crucial conceptual ancestry and similarity which such cases exhibit. We should speak, rather, of a natural evolution in the research tradition: an evolution which represents a change, to be sure, but a change that is far from repudiation of a former research tradition and the creation of a new one.

There is much continuity in an evolving research tradition. From one stage to the next, there is a preservation of most of the crucial assumptions of the research traditions. Most of the problem-solving techniques and archetypes will be preserved through the evolution. The relative importance of the empirical problems which the research tradition addresses will remain approximately the same. But the emphasis here must be on relative continuity between successive stages in the evolutionary process. If a research tradition has undergone numerous evolutions in the course of time, there will probably be many discrepancies between the methodology and ontology of its earliest and its latest formulation. Thus, the Cartesianism of a Bernoulli, writing a century after Descartes' death, is very different from the Cartesianism of the master. The Newtonian research tradition in Michael Faraday's hands is a far cry from that of Newton's first followers. But a finer-grained analysis of the historical evolution of these research traditions will show that there was a continuous intellectual descent from Descartes to Bernoulli, and from Newton to Faraday, and that the Cartesian and Newtonian research traditions, as different as their end-points may look from their beginnings, exhibited enormous continuity in the character of their transformations.

But such an approach leaves itself open to the obvious challenge: if a research tradition can undergo certain deep-level transformations and still remain in some sense the "same" tradition, how do we distinguish change within a research tradition from the replacement of one research tradition by another?

A partial answer to the question comes from recognizing that at any given time certain elements of a research tradition are more central to, more entrenched within, the research tradition than other elements. It is these more central elements which are taken, at that time, to be most characteristic of the research tradition. To abandon them is indeed to move outside the research tradition, whereas the less central tenets can be modified without repudiation of the research tradition. Like Lakatos, then, I want to suggest that certain elements of a research tradition are sacrosanct, and thus cannot be rejected without repudiation of the tradition itself. But unlike Lakatos, I want to insist that the set of elements falling in this (unrejectable) class changes through time. What was taken to characterize the unrejectable core of the Newtonian tradition in eighteenth-century mechanics (e.g., absolute space and time) was no longer regarded as such by mid-nineteenth-century Newtonians. What constituted the essence of the Marxist research tradition in the late nineteenth century is substantially different from the "essence" of Marxism a half century later. Lakatos and Kuhn were right in thinking that a research programme or paradigm always has certain nonrejectable...
elements associated with it; but they were mistaken in failing to see that the elements constituting this class can shift through time. By relativizing the “essence” of a research tradition with respect to time, we can, I believe, come much closer to capturing the way in which scientists and historians of science actually utilize the concept of a tradition.

Of course, this still leaves unanswered how it is that scientists decide at any given time which elements of a maxil-theory or research tradition are to be treated as “unrejectable” (a problem likewise unanswered by Kuhn and Lakatos). I cannot give a fully satisfactory answer to the question, but some hunches are probably worth exploring. Both Kuhn and Lakatos seem to believe that the decision about which elements of a maxil-theory fall into this privileged class is arbitrary and not governed by rational considerations on their account, it simply “happens.” I am unable to give a full specification of all the factors which influence the selection of the core of a research tradition, but there are clearly dimensions of the choice which are rational. For instance, one of the major factors influencing the entrenchment of any element of a research tradition is its conceptual well-foundedness. The core assumptions of any given research tradition are continuously undergoing conceptual scrutiny. Some of those assumptions will, at any given time, be found to be strong, and unproblematic. Others will be regarded as less clear, less well-founded. As new arguments emerge which buttress, or cast doubt on, different elements of the research tradition, the relative degree of entrenchment of the different components will shift. During the evolution of any active research tradition, scientists learn more about the conceptual dependence and autonomy of its various elements; when it can be shown that certain elements, previously regarded as essential to the whole enterprise, can be jettisoned without compromising the problem-solving success of the tradition itself, these elements cease to be a part of the “unrejectable core” of the research tradition. (For instance, after Mach and Frege argued that none of the other elements of the Newtonian tradition required the absoluteness of space and time, these notions moved perceptibly towards the periphery of the Newtonian research tradition.)

Research Traditions and Changes in Worldviews

We have stressed, both here and in the previous chapter, how research traditions and theories can encounter serious cognitive difficulties if they are incompatible with certain broader systems of belief within a given culture. Such incompatibilities constitute conceptual problems which may seriously challenge the acceptability of the theory. But it may equally well happen that a highly successful research tradition will lead to the abandonment of that worldview which is incompatible with it, and to the elaboration of a new worldview compatible with the research tradition. Indeed, it is in precisely this manner that many radically new scientific systems eventually come to be “canonized” as part of our collective “common sense.” In the seventeenth and eighteenth centuries, for instance, the new research traditions of Descartes and Newton were violently counter to many of the most cherished beliefs of the age on such questions as “man’s place in Nature,” the history and extent of the cosmos, and, more generally, the nature of physical processes. Everyone at the time acknowledged the existence of these conceptual problems. They were eventually resolved, not by modifying the offending research traditions to bring them in line with more traditional world views, but rather by forging a new world view which could be reconciled with the scientific research traditions. A similar process of re-adjustment occurred in response to the Darwinian and Marxist research traditions in the late nineteenth century; in each case, the core, “nonscientific” beliefs of reflective people were eventually modified to bring them in line with highly successful scientific systems.

But it would be a mistake to assume that worldviews always crumble in the face of new scientific research traditions which challenge them. To the contrary, they often exhibit a remarkable resilience which belies the (positivistic) tendency to dismiss them as mere fluff. The history of science, both recent and distant, is replete with cases where worldviews have not evaporated in the face of scientific theories which challenged them. In our own time, for instance, neither quantum mechanics nor behavioristic psychology have shifted most people’s beliefs about the world or themselves. Contrary to quantum
mechanics, most people still conceive of the world as being populated by substantial objects, with fixed and precise properties; contrary to behaviorism, most people still find it helpful to talk about the inner, mental states of themselves and others.

Confronted with such examples, one might claim that these research traditions are still new and that older world views predominate only because the newer insights have not yet penetrated the general consciousness. Such a claim may prove to be correct, but before we accept it uncritically, there are certain more striking historical cases that need to be aired. Ever since the seventeenth century, the dominant research traditions within the physical sciences have presupposed that all physical changes are subject to invariant natural laws (either statistical or nonstatistical). Given certain initial conditions, certain consequences would inevitably ensue. Strictly speaking, this claim should be as true of man and other animals as it is of stars, planets, and molecules. Yet in our own time, as much as in the seventeenth century, very few people are prepared to abandon the conviction that human beings (and some of the higher animals) have a degree of undetermination in their actions and their thoughts. Virtually all of our social institutions, most of our social and political theory, and the bulk of our moral philosophy is still based on a worldview seemingly incompatible with a law-governed universe. Despite repeated efforts in the last three centuries to explain away this conceptual problem, it is fair to say that this is one important area where the traditional worldview has made very few concessions to the “broader implications” of some highly successful scientific traditions. 14

It has long been fashionable to imagine that the worldview or “Zeitgeist” of any epoch always plays a purely conservative role, suppressing intellectual innovation and encouraging the retention of the scientific status quo. Exponents of scientific progress have frequently bemoaned the use of “worldview” considerations which invariably stifle the emergence of new scientific ideas. E. G. Boring spoke for many scientists and philosophers when he insisted that: “Inevitably by definition the Zeitgeist favors conventionality . . . [and] works against originality.” 15

This position is bad philosophy and false history. It is philosophically weak insofar as it ignores the fact that there is no reason, in principle, why an entrenched worldview could not provide a more convincing rationale for an innovative theoretical development than for a traditional theory. Boring’s claim that a Zeitgeist automatically favors traditional theories is thus without cognitive foundation. The view is equally misleading historically. It is well known, for instance, that the Zeitgeist of late seventeenth-century England did much to hasten the replacement of the older mechanical philosophy by the newer science of Newton, precisely because Newton’s research tradition could be more readily justified within that framework than the mechanistic science of Descartes could. More recently, the emergence of “new” quantum mechanics in the late 1920s found a quick and ready reception among the many intellectuals who were already convinced that the rigid causal categories of classical science were unreliable.

The Integration of Research Traditions

Up to now I have spoken as if research traditions were invariably in competition with one another, suggesting moreover that the resolution of such a conflict comes when one alone among the competing traditions dominates and when its competitors are effectively vanquished. This is often the case. But it would be a serious error to assume that a scientist cannot consistently work in more than one research tradition. If these research traditions are inconsistent in their fundamentals, then the scientist who accepts them both raises serious doubts about his capacity for clear thinking. But there are times when two or more research traditions, far from mutually undermining one another, can be amalgamated, producing a synthesis which is progressive with respect to both the former research traditions. It is the dynamics of such situations which I want to discuss briefly here.

There are basically two ways in which different research traditions can be integrated. In some cases, one research tradition can be grafted onto another, without any serious
modification in the presuppositions of either. Thus, in eighteenth-century natural philosophy, many scientists were simultaneously Newtonians and subtle fluid theorists. Their adherence to the research tradition of subtle fluids (which was as much Cartesian as it was Newtonian) led them to postulate imperceptible aethereal fluids in order to explain the phenomena of electricity, magnetism, heat, perception, and a range of other empirical problems. Their Newtonianism, on the other hand, led them to assume that the constituent particles of such fluids interacted (not by contact, as the Cartesian tried to suggest) but rather by means of strong forces of attraction and repulsion, acting-at-a-distance across empty space. The fusion of these two research traditions was to constitute itself a major research tradition, one which Schofield has labelled “materialism.”

While undermining the presuppositions of neither of its predecessors, the amalgamation suggested important new lines of research, and put scientists in a position to deal with empirical and conceptual problems which neither of the ancestor traditions alone could resolve satisfactorily.

In other cases, however, the amalgamation of two or more research traditions requires the repudiation of some of the fundamental elements of each of the traditions being combined. In these cases, the new research tradition, if successful, requires the abandonment of its predecessors. (It is, incidentally, in just this way that most so-called scientific revolutions take place; not by the articulation of a research tradition whose ingredients are revolutionary and new, but rather by the development of a research tradition whose novelty consists in the way in which old ingredients are combined.) There are many examples of this process in the history of any discipline, scientific or otherwise. To consider some scientific cases first, eighteenth- and nineteenth-century natural philosophy is replete with such integrations. Roger Boscovich, for instance, set out deliberately to develop a new “system of nature,” by picking and choosing from among the assumptions of two incompatible research traditions, Newtonianism and Leibnizianism. Maupertuis attempted something similar. The work of their contemporary, Daniel Bernoulli, illustrates an analogous attempt to forge a compromise between the research traditions of Cartesian and Newtonian physics. In the eighteenth and nineteenth centuries, geological followers of Hutton were hammering out a new tradition which drew on elements of calorific heat theories and Vulcanist geology. These research traditions could not be preserved intact and, as a result, the Huttonians had to forge what was regarded as a “revolutionary” research tradition which incorporated elements of traditions which had been previously incompatible. Within economics, Karl Marx drew on elements from the idealism of Hegel, the materialism of Feuerbach, and the “capitalism” of Adam Smith and his English followers.

“Nonstandard” Research Traditions

It would be dishonest to leave the topic of research traditions without adding a caveat, although how important it is remains to be seen. We have thus far characterized research traditions as rather ambivalent and grandiose entities, replete with ontologies and methodologies. There is no doubt in my mind that many of the best known research traditions in science possess both these characteristics. But there also seem to be traditions and schools in science which, although lacking one or the other (or in some cases both), have nonetheless had a genuine intellectual coherence about them. For instance, the tradition of psychometrics in the early twentieth century seems to have been held together by little more than the conviction that mental phenomena could be mathematically represented. Equally, the tradition of rational mechanics in the eighteenth century seems to have cut horizontally across almost every conceivable metaphysical and methodological tradition and to have drawn together a group of thinkers committed simply to the mathematical analysis of motion and rest. The important tradition of “analytic physics” in early nineteenth century France (including Biot, Fourier, Ampère, and Poisson) seems to have had no common ontology, although its partisans doubtless shared a common methodology. In our own time, cybernetics and information theory seem to be “schools” without well-defined
ontologies. Whether, on further investigation, it will turn out
that these "nonstandard" research traditions do have ontolog-
ic and methodological elements or whether, failing that, they
will behave differently from "richer" research traditions are
still unanswered questions. Much research is still needed on
these units that are too narrow to be full-blown research
traditions but too global to be mere theories.

The Evaluation of Research Traditions

Our focus thus far has been on the temporal dynamics of
research traditions. We have learned something about how such
traditions evolve, how they interact with their constituent
theories and with wider elements of the worldview and the
problem situation.

However, I have said nothing yet about how, if at all, it is
possible for scientists to make sensible choices between alterna-
tive research traditions, nor about how a single tradition can be
appraised relative to its acceptability. This is a crucial issue, for
until and unless we can articulate workable criteria for choice
between the larger units I am calling research traditions, then
we have neither a theory of scientific rationality, nor a theory of
progressive, cognitive growth.

In the next few pages, I shall be defining some criteria for the
evaluation of research traditions, and discussing some of the
different contexts in which cognitive evaluations can be made.

Adequacy and Progress

Even though research traditions in themselves entail no
observable consequences, there are several different ways in
which they can be rationally evaluated and thus compared. Two
chief modes of appraisal, however, are the most common and
the most decisive. One of these modes is synchronic, the other is
diachronic and developmental.

We may, to begin with, ask about the (momentary) adequacy
of a research tradition. We are essentially asking here how
effective the latest theories within the research tradition are at
solving problems. This, in turn, requires us to determine the
problem-solving effectiveness of those theories which presently
constitute the research tradition (ignoring their predecessors).
Since we already discussed how to evaluate the problem-solving
effectiveness of individual theories, we need only combine
those appraisals to find the adequacy of the broader research
tradition.

Alternatively, we may ask about the progressiveness of a
research tradition. Here our chief concern is to determine
whether the research tradition has, in the course of time,
increased or decreased the problem-solving effectiveness of its
components, and thus its own (momentary) adequacy. This
matter is, of course, unavoidably temporal; without a knowl-
edge of the history of the research tradition, we can say nothing
whatever about its progressiveness. Under this general rubric,
there are two subordinate measures which are particularly
important:

1. the general progress of a research tradition—this is
determined by comparing the adequacy of the sets of theories
which constitute the oldest and those which constitute the most
recent versions of the research tradition;

2. the rate of progress of a research tradition—here, the
changes in the momentary adequacy of the research tradition
during any specified time span are identified.

It is important to note that the general progress and the rate
of progress of a research tradition may be widely at odds. For
instance, a research tradition may show a high degree of
general progress, and yet show a low rate of progress, especially
in its recent past. Alternatively, a research tradition may have
a high rate of progress during its recent past while exhibiting
limited general progress.

Likewise, and even more importantly, the appraisals of a
research tradition based upon its progressiveness (either general
or time-dependent) may be very different from those based on
its momentary adequacy. One can conceive of cases, for
example, where the adequacy of a research tradition is relatively
high and yet it shows no general progress or even a negative
rate of progress. (In fact, many actual research traditions have
this character.) Alternatively, there are cases (e.g., behavioristic
psychology and early quantum theory) where the general
progress and the rate of progress of a research tradition are
high, but where the momentary adequacy of the tradition is still quite low.

 Needless to say, the appraisals will not always point in contrary directions, but the very fact that they can (and sometimes have) emphasizes the need to attend very carefully to the various contexts in which cognitive appraisals of research traditions are made. It is that issue which must occupy us next.

 **The Modalities of Appraisal: Acceptance and Pursuit**

 Almost all the standard writings on scientific appraisal, whether we look to philosophical or historical discussions of science, have two common features: they assume that there is only one cognitively legitimate context in which theories can be appraised; and they assume that this context has to do with determinations of the empirical well-foundedness of scientific theories. Both these assumptions probably need to be abandoned: the first because it is false, the second because it is too limited.

 I shall be arguing that a careful examination of scientific practice reveals that there are generally two quite different contexts within which theories and research traditions are evaluated. I shall suggest that, within each of these contexts of inquiry, very different sorts of questions are raised about the cognitive credentials of a theory, and that much scientific activity which appears irrational—if we insist on a uni-contextual analysis—can be perceived as highly rational if we allow for the divergent goals of the following two contexts:

 **The context of acceptance.** Beginning with the more familiar of the two, it is clear that scientists often choose to accept one among a group of competing theories and research traditions, i.e., to treat it as if it were true. Particularly in cases where certain experiments or practical actions are contemplated, this is the operative modality. When, for instance, a research immunologist must prescribe medication for a volunteer in an experiment, when a physicist decides what measuring instrument to use for studying a problem, when a chemist is seeking to synthesize a compound with certain properties; in all these cases, the scientist must commit himself, however tentatively, to the acceptance of one group of theories and research traditions and to the rejection of others.

 How can he make a coherent decision? There are a wide range of possible answers here: inductivists will say "choose the theory with the highest degree of confirmation"; or "choose the theory with the highest utility"; falsificationists—if they give any advice at all—will say "choose the theory with the greatest degree of falsifiability." Still others, such as Kuhn, would insist that no rational choice can be made. I have already indicated why none of these answers are satisfactory. My own reply to the question, of course, would be, "choose the theory (or research tradition) with the highest problem-solving adequacy."

 On this view, the rationale for accepting or rejecting any theory is thus fundamentally based on the idea of problem-solving progress. If one research tradition has solved more important problems than its rivals, then accepting that tradition is rational precisely to the degree that we are aiming to "progress," i.e., to maximize the scope of solved problems. In other words, the choice of one tradition over its rivals is a progressive (and thus a rational) choice precisely to the extent that the chosen tradition is a better problem solver than its rivals.

 This way of appraising research traditions has three distinct advantages over previous models of evaluation: (1) it is workable: unlike both inductivist and falsificationist models, the basic evaluation measures seem (at least in principle) to pose fewer difficulties; (2) it simultaneously offers an account of rational acceptance and of scientific progress which shows the two to be linked together in ways not explained by previous models; and (3) it comes closer to being widely applicable to the actual history of science than alternative models have been.

 **The context of pursuit.** Even if we had an adequate account of theory choice within the context of acceptance, however, we would still be far from possessing a full account of rational appraisal. The reason for this is that there are many important situations where scientists evaluate competing theories by means of other criteria.
FROM THEORIES TO RESEARCH TRADITIONS

criteria which have nothing directly to do with the acceptability or "warranted assertibility" of the theories in question.

The actual occurrence of such situations has often been observed. Paul Feyerabend, in particular, has identified many historical cases where scientists have investigated and pursued theories or research traditions which were patently less acceptable, less worthy of belief, than their rivals. Indeed, the emergence of virtually every new research tradition occurs under just such circumstances. Whether we look to Copernicanism, the early stages of the mechanical philosophy, the atomic theory in the first half of the nineteenth century, early psychoanalytic theory, the preliminary efforts at the quantum mechanical approach to molecular structure, we see the same pattern: scientists often begin to pursue and to explore a new research tradition long before its problem-solving success (or its inductive support, or its degree of falsifiability, or its novel predictions) qualifies it to be accepted over its older, more successful rivals.

Another side to the same coin is the historical fact that a scientist can often be working alternately in two different, and even mutually inconsistent, research traditions. Particularly during periods of "scientific revolution," it is commonly the case that a scientist will spend part of his time working on the dominant research tradition and a part of his time working on one or more of its less successful, less fully developed rivals. If we take the view that it is rational to work with and explore only the theories one accepts (and its corollary that one ought not accept or believe mutually inconsistent theories) then there can be no way of making sense of this common phenomenon.

Hence neither the use of mutually inconsistent theories nor the investigation of less successful theories—both well-attested historical phenomena—can be explained if we insist that the context of acceptance exhausts scientific rationality. Confronted by such cases, we would have to conclude, with Feyerabend and Kuhn, that the history of science is largely irrational. But if, on the other hand, we realize that scientists can have good reasons for working on theories that they would not accept, then this frequent phenomenon may be more comprehensible.

To see what could count as "good reasons" here, we must return to some earlier discussions. It has often been suggested

"in this essay that the solution of a maximum number of empirical problems, and the generation of a minimum number of conceptual problems and anomalies is the central aim of science. We have seen that such a view entails that we should accept at any time those theories or research traditions which have shown themselves to be the most successful problem solvers. But need the acceptance of a given research tradition preclude us from exploring and investigating alternatives which are inconsistent with it? Under certain circumstances, the answer to this question is decidedly negative. To see why, we need only consider the following general kind of case: suppose we have two competing research traditions, RT and RT'; suppose further that the momentary adequacy of RT is much higher than that of RT', but that the rate of progress of RT' is greater than the related value for RT. So far as acceptance is concerned, RT is clearly the only acceptable one of the pair. We may nonetheless decide to work on, further articulate, and explore the problem-solving merits of RT', precisely on the grounds that it has recently shown itself to be capable of generating new solutions to problems at an impressive rate. This is particularly appropriate if RT' is a relatively new research tradition. It is common knowledge that most new research traditions bring new analytic and conceptual techniques to bear on the solution of problems. These new techniques constitute (in the cliché "fresh approaches" which, particularly over the short run, are likely to pay problem-solving dividends. To accept a budding research tradition merely because it has had a high rate of progress would, of course, be a mistake; but it would be equally mistaken to refuse to pursue it if it has exhibited a capacity to solve some problems (empirical or conceptual) which its older, and generally more acceptable, rivals have failed to solve.

Putting the point generally, we can say that it is always rational to pursue any research tradition which has a higher rate of progress than its rivals (even if the former has a lower problem-solving effectiveness). Our specific motives for pursuing such a research tradition could be one of many: we might have a hunch that, with further development, RT' could become more successful than RT; we might have grave doubts
about \( RT' \) ever becoming generally successful, but feel that some of its more progressive elements could eventually be incorporated within \( RT \). Whatever the vagaries of the individual case, if our general aim is increasing the number of problems we can solve, we cannot be accused of inconsistence or irrationality if we pursue (without accepting) some highly progressive research tradition, regardless of its momentary inadequacy (in the sense defined above).

In arguing that the rationality of pursuit is based on relative progress rather than overall success, I am making explicit what has been implicitly described in scientific usage as "promise" or "fecundity." There are numerous cases in the history of science which illustrate the role which an appraisal of promise or progressiveness can have in earning respectability for a research tradition.

The Galilean research tradition, for instance, could not in its early years begin to stack up against its primary competitor, Aristotelianism. Aristotle's research tradition could solve a great many more important empirical problems than Galileo's. Equally, for all the conceptual difficulties of Aristotelianism, it really posed fewer crucial conceptual problems than Galileo's early brand of physical Copernicanism—a fact that tends to be lost sight of in the general euphoria about the scientific revolution. But what Galilean astronomy and physics did have going for it was its impressive ability to explain successfully some well-known phenomena which constituted empirical anomalies for the cosmological tradition of Aristotle and Ptolemy. Galileo could explain, for example, why heavier bodies fell no faster than lighter ones. He could explain the irregularities on the surface of the moon, the moons of Jupiter, the phases of Venus, and the spots on the sun. Although Aristotelian scientists ultimately were able to find solutions for these phenomena (after Galileo drew their attention to them), the explanations proffered by them smacked of the artificial and the contrived. Galileo was taken so seriously by later scientists of the seventeenth century, not because his system as a whole could explain more than its medieval and renaissance predecessors (for it palpably could not), but rather because it showed promise by being able, in a short span of time, to offer solutions to problems which constituted anomalies for the other research traditions in the field.

Similarly, Daltonian atomism generated so much interest in the early years of the nineteenth century largely because of its scientific promise, rather than its concrete achievements. At Dalton's time, the dominant chemical research tradition was concerned with elective affinities. Eschewing any attempt to theorize about the microconstituents of matter, elective affinity chemists sought to explain chemical change in terms of the differential tendencies of certain chemical elements to unite with others. That chemical tradition had been enormously successful in correlating and predicting how different chemical substances combine. Dalton's early atomic doctrine could claim nothing like the overall problem-solving success of elective affinity chemistry (this is hardly surprising, for the affinity tradition was a century old by the time of Dalton's New System of Chemical Philosophy); still worse, Dalton's system was confronted by numerous serious anomalies.\(^1\) What Dalton was able to do, however, was to predict—as no other chemical system had done before—that chemical substances would combine in certain definite ratios and multiples thereof, no matter how much of the various reagents was present. This phenomenon, summarized by what we now call the laws of definite and multiple proportions, created an immediate stir throughout European science in the decade after Dalton's atomic program was promulgated. Although most scientists refused to accept the Daltonian approach, many nonetheless were prepared to take it seriously, claiming that the serendipity of the Daltonian system made it at least sufficiently promising to be worthy of further development and refinement.

Whether the approach taken here to the problem of "rational pursuit" will eventually prevail is doubtful, for we have only begun to explore some of the complex problems in this area; what I would claim is that the linkage between progress and pursuit outlined above offers us a healthy middle ground between (on the one side) the insistence of Kahn and the inductivists that the pursuit of alternatives to the dominant paradigm is never rational (except in times of crisis) and the anarchistic claim of Feyerabend and Lakatos that the pursuit
of any research tradition—no matter how regressive it is—can always be rational.

Adhocness and the Evolution of Research Traditions

No discussion of the various appraisal vectors utilized in science would be complete without including the notion of adhocness (an issue often discussed under the rubric “independent testability”). At least since the seventeenth century, but particularly in our own era, ad hoc stratagems and hypotheses have received much attention from scientists and philosophers alike. The determination that a theory or theoretical modification is ad hoc gives us grounds, on the usual account, for dismissing it as illegitimate and unscientific. If we are to accept the claims sometimes made by such philosophers as Popper, Grünbaum, and Lakatos, it is irrational or unscientific ever to accept a theory which is ad hoc. What does such adhocness amount to, and why, if at all, is it such a liability for theories which exhibit it?

The issue of adhocness arises most often in connection with the evolution of theories and the manner in which they handle anomalies. We are usually asked to imagine a situation in which some theory, \( T \), encounters a refuting instance, \( A \). In response to \( A \), some modification is introduced into \( T \), producing \( T' \). The conventional wisdom insists that the later theory \( T' \) is ad hoc if \( T' \) can solve \( A \), and the other known problems \( T \), could solve, but \( T' \) has no non-trivial, testable implications other than those of \( T \) and \( A \). Putting it in the language of this monograph, a theory \( T \) is ad hoc if it can solve only those empirical problems solved by its predecessor \( T \), and those which constitute refuting instances for \( T \), but no further problems.

There are several difficulties with this approach to adhocness. In the first place, we generally have no way of knowing at any given time whether a new theory \( T' \) will at some later point be able to solve new problems. To make such a judgment sensibly would require a superhuman clairvoyance about what empirical problems and what auxiliary theories (which, when conjoined with the theory, might lead to the solution of new problems) are going to emerge in the future. However, taking a cue from Adolf Grünbaum, we can relativize the above definition to situations of belief and say that a theory \( T \) is ad hoc if it is believed to solve only those empirical problems which were solved by, or refuting instances for, \( T' \).

But serious difficulties still remain. As Duhem taught us, individual theories in isolation generally solve no problems. It is, rather, complexes of theories which are involved in problem solution. Hence, we must modify the traditional characterization once again, yielding a definition such as the following: a theory is ad hoc if it is believed to figure essentially in the solution of all and only those empirical problems which were solved by, or refuting instances for, an earlier theory.

Clumsy as it is, this characterization of adhocness seems to do justice to some of the most sophisticated accounts of adhocness developed in the last decade. Assuming that adhocness is understood in this way, we are entitled to ask: what is objectionable about it? If some theory \( T \) has solved more empirical problems than its predecessor—even just one more—then \( T \) is clearly preferable to \( T' \), and, ceteris paribus, represents cognitive progress with respect to \( T \). However, we can go further than this to claim that the resort to ad hoc stratagems, as defined immediately above, is perfectly consistent with the general aim of increasing our problem-solving capacities. Ad hoc modifications, by their very definition, are empirically progressive.

This result should not be surprising. Indeed, much of what we mean by such clichés as “learning from experience” and “the self-correction of science” is represented by situations in which, when a theory encounters an anomaly, we alter the theory so as to transform the anomaly into a solved problem. While it would be a nice bonus if every theory modification could immediately solve some new problems as well as some old, unsolved ones, to insist on that requirement (as, for instance, Popper, Lakatos, and Zahar have) is to repudiate the doctrine that theories which solve more problems about the world are preferable to those which solve fewer.

In urging that adhocness (so defined) is a cognitive virtue rather than a vice, I am clearly not implying that ad hoc
theories are invariably better than non-ad hoc ones. My claim, rather, is that an ad hoc theory is preferable to its non-ad hoc predecessor (which was confronted with known anomalies). To believe otherwise is to deny a vital aspect of the problem-solving character of scientific inquiry.

But it might be argued that I have missed the point of the critics of ad hocness. They might say, "Yes, of course, T₂ is better than its refuted predecessor T₁; but the relevant comparison is between the ad hoc T₁ and some other theory T₃, which is not ad hoc but still solves as many problems as T₁." Einstein's special theory of relativity might exemplify T₃ while the Lorentz-modified ether theory was T₁. The obvious reply to such criticism is to ask why the admittedly ad hoc character of the Lorentz contraction constitutes a decisive handicap against it in comparing it with special relativity. If the empirical problem-solving capacities of the two theories are, so far as we can tell, equivalent, then they are (empirically) on a par; defenders of the view that the ad hocness of T₁ makes it distinctly inferior to T₃ must spell out why, in such cases, the comparable problem-solving abilities and equivalent degrees of empirical support can be thrown to the winds simply by stipulating that ad hoc theories are intrinsically otiose.

What seems to lie behind many discussions of ad hocness is a conviction—often present but rarely defended—that there is something suspicious about any change in a theory which is motivated by the desire to remove an anomaly. The presumption is that we cannot really trust such cosmetic surgery because, once we know what the anomaly is, it is little more than child's play to produce some face-saving change in the theory which turns the anomaly into a positive instance. I doubt that where "real" science is concerned, this task is such an easy one. We must remember that, as ad hocness has been defined, any ad hoc change must increase rather than decrease the problem-solving capacity of the theory in question. Most of the obvious and trivial ways of eliminating anomalies—e.g., arbitrarily restricting the boundary conditions, eliminating those postulates of the theory which entailed the anomaly (assuming they could be localized!), redefining terms or correspondence rules—would generally result in decreasing the problem-solving effectiveness of a theory. Hence, such manoeuvres—which we
ground for rejecting theories. But it is important to stress that the concept of adhocrness itself, thus understood, adds nothing whatever to our analytic machinery for appraising theories, since it is itself just a special case of conceptual problem generation.

I am, by no means, the first to suggest a conceptual interpretation of adhocrness; Lakatos, Zahar, and Schaffner have developed similar interpretations recently. In all their discussions, however, conceptual adhocrness remains but one of many species of adhocrness, rather than the only legitimate sense. Still worse, none of these writers has indicated how conceptual adhocrness is to be assessed, nor even what it amounts to. Equally, all these writers leave us in the dark about how seriously, if at all, it should count against a theory if it is ad hoc. The seeming virtue of the approach taken here is that it separates spurious senses of the ad hoc from legitimate ones, and it gives us machinery for assessing the degrees of cognitive threat posed by adhocrness to the theories which exhibit it.

Anomalies Revisited

Chapter one contained the paradoxical claim that the refuting instances of a theory are not necessarily anomalous problems, along with a promissory note to clarify that claim once the machinery was available to do so. The evaluational methods outlined here allow us to return to this issue. I said before that a problem was only anomalous (i.e., cognitively threatening) for some theory, T, if that problem was unsolved by T but solved by one of its competitors. Clearly, some refuting instances will satisfy this definition, but many will not. It is often the case that some prediction of a theory fails to square with the data, but no other available theory can solve the data either. In the latter situation, why should the data not count as a threatening anomaly for T?

In brief, the answer is this: Whenever a theory encounters a refuting instance, it is possible to modify the interpretative rules associated with the theory so as to disarm the "refuting" data. If, for instance, we have a theory, T, that "all planets move in ellipses" and then discover a satellite of the sun, S, which moves in a circle, we can always modify the interpretative rules governing the term "planet" so as to exclude S, thus preserving our theory intact and eliminating any appearance of refutation. If there is no other theory extant which can explain the motion of S, the exclusion of S from T's domain is perfectly reasonable and progressive—for we lose none of our previously won problem-solving successes by legislating S out of the relevant domain. By contrast, if some alternative to T can solve S, then T's legislation of S outside the domain is a regressive step, open to rational criticism precisely because T's abandonment of S as a legitimate problem entails that we sacrifice some of our demonstrated problem-solving capacity.

What this amounts to is that the modification of a theory arbitrarily in order to eliminate a refuting instance is open to criticism only if such a move would lead to a diminished problem-solving efficiency. That can generally be shown to happen only if the refuting instance is solved by some theory in the domain. Hence, a refuting instance only counts as a serious anomaly when it has been solved by some theory or other.

Summary: A General Characterization of Scientific Change

Drawing together the various strands of argument developed in this chapter, we can conclude that:

1. The adequacy or effectiveness of individual theories is a function of how many significant empirical problems they solve, and how many important anomalies and conceptual problems they generate. The acceptability of such theories is related both to their effectiveness and to the acceptability of their related research traditions.

2. The acceptability of a research tradition is determined by the problem-solving effectiveness of its latest theories.

3. The promise, or rational pursuitability, of a research tradition is determined by the progress (or rate of progress) it has exhibited.

4. Acceptance, rejection, pursuit, and non-pursuit constitute the major cognitive stances which scientists can legitimately take towards research traditions (and their constituent theories).
Determinations of truth and falsity are irrelevant to the acceptability or the pursuability of theories and research traditions.

5. All evaluations of research traditions and theories must be made within a comparative context. What matters is, not, in some absolute sense, how effective or progressive a tradition or theory is, but, rather, how its effectiveness or progressiveness compares with its competitors.

We can now move on to discuss the implications of this model of scientific progress for an understanding of some of the central historical and philosophical questions about the cognitive growth of science.

Chapter Four
Progress and Revolution

The revolutionary can only regard his revolution as a progress in so far as he is also an historian. Collingwood (1956), p. 326

The analytic machinery developed in the preceding chapters raises numerous significant questions about the historical evolution and cognitive status of the sciences. The function of this chapter is to examine the ways in which a problem-solving approach to scientific inquiry can throw new light on a number of central historical and philosophical problems about science, and to show how scientific progress, scientific rationality, and the nature of scientific revolutions can all be profitably discussed in terms of the problem-oriented model outlined above.

Progress and Scientific Rationality

One of the thorniest questions of twentieth-century philosophy concerns the nature of rationality. Some philosophers suggest that rationality consists in acting to maximize one’s personal utilities; others suggest that rationality consists in believing in, and acting on, only those propositions which we have good grounds for believing to be true (or at least to be