

more emphasis than it has yet received. A scientist choosing between two theories ordinarily knows that his decision will have a bearing on his subsequent research career. Of course he is especially attracted by a theory that promises the concrete successes for which scientists are ordinarily rewarded.

7. The least equivocal example of this position is probably the one developed in Scheffler, *Science and Subjectivity*, chap. 4.

8. If the group is small, it is more likely that random fluctuations will result in its members' sharing an atypical set of values and therefore making choices different from those that would be made by a larger and more representative group. External environment—intellectual, ideological, or economic—must systematically affect

the value system of much larger groups, and the consequences can include difficulties in introducing the scientific enterprise to societies with inimical values or perhaps even the end of that enterprise within societies where it had once flourished. In this area, however, great caution is required. Changes in the environment where science is practiced can also have fruitful effects on research. Historians often resort, for example, to differences between national environments to explain why particular innovations were initiated and at first disproportionately pursued in particular countries, e.g., Darwinism in Britain, energy conservation in Germany. At present we know substantially nothing about the minimum requisites of the social milieux within which a sciencelike enterprise might flourish.

Larry Laudan

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.

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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) quantum field 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 ambiguous¹ (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,² 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"³ 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.⁴ He has correctly rejected the (widely assumed) cumulative character of science.⁵ 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.⁶ Feyerabend⁷ 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 exemplification 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.⁸ 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 behavioristic 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 energeticists 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"⁹ which Kuhn hoped 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;¹⁰ (2) the "positive heuristic," which contains "a partially articulated set of suggestions or hints on how to change, . . . modify, sophisticate [*sic*]"¹¹ our specific theories whenever we wish to improve them, and (3) "a series of theories, T_1, T_2, T_3, \dots " where each subsequent theory "results from adding auxiliary clauses to . . . the previous

theory.”¹² 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 incommensurable¹³ 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 programs 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 successive 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.¹⁴ As Grünbaum and others have shown convincingly, the attempt to specify content measures for scientific theories is extremely problematic if not literally impossible.¹⁵ 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.¹⁶

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.¹⁷ 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.¹⁸

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 maxi-theories. 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 sig-

nificant 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.*

* * *

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 alternative 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,³⁷ 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 knowledge 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.³⁸ 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.³⁹ 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 very 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 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,⁴⁰ 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 gen-

eration 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 tradi-

tion 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 inconsistency 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 micro-constituents 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.⁴¹ 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 link-

age between progress and pursuit outlined above offers us a healthy middle ground between (on the one side) the insistence of Kuhn 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*.

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PHILOSOPHY OF SCIENCE NATURALIZED

In arguing a "role for history," Kuhn was proposing a naturalized philosophy of science. That, I argue, is the only viable approach to the philosophy of science. I begin by exhibiting the main general objections to a naturalistic approach. These objections, I suggest, are equally powerful against nonnaturalistic accounts. I review the failure of two nonnaturalistic approaches, methodological foundationism (Carnap, Reichenbach, and Popper) and metamethodology (Lakatos and Laudan). The correct response, I suggest, is to adopt an "evolutionary perspective." This perspective is defended against one recent critic (Putnam). To argue the plausibility of a naturalistic approach, I next sketch a naturalistic account of theories and of theory choice. This account is then illustrated by the recent revolution in geology. In conclusion I return to Kuhn's question about the role of history in developing a naturalistic theory of science.

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1. KUHN'S NATURALISM

In the very first chapter of *The Structure of Scientific Revolutions*, Kuhn sought to establish "a role for history." Part of that role, he implied, is as data for "a theory of scientific inquiry." And by "theory" he meant something comparable to theories in the sciences themselves. Thus, referring to standard philosophical distinctions, such as that between "discovery" and "justification," he wrote:

Rather than being elementary logical or methodological distinctions, which would thus be prior to the analysis of scientific knowledge, they now seem integral parts of a traditional set of substantive answers to the very questions upon which they have been deployed. That circularity does not at all invalidate them. But it does make them parts of a theory and, by doing so, subjects them to the same scrutiny regularly applied to theories in other fields. If they are to have more than pure abstraction as their content, then that content must be discovered by observing them in application to the data they are meant to eluci-