

Scientific Rationality

Normative versus Descriptive Construals

1. Two Conceptions of the Methodology of Science

In the course of the past few centuries, scientific inquiry has vastly broadened man's knowledge and deepened his understanding of the world he lives in; and the striking successes of the technologies based on the insights thus gained are eloquent testimony to the basic soundness of scientific modes of research. In view of these achievements, scientific inquiry has come to be widely acknowledged as the exemplar of rationality in the pursuit of reliable knowledge. But there is no unanimity among students of the methodology and the history of science when it comes to the question whether, or to what extent, it is possible to specify precisely a set of procedural rules or standards which are characteristic of scientific inquiry, and adherence to which qualifies science as the exemplar of rationality in the pursuit and acquisition of knowledge.

This problem has in recent years been the focus of an intense and fruitful controversy between two schools of thought which I will refer to as the analytic-empiricist and the historic-sociological, or pragmatist, school. By the former, I understand here a body of ideas which, broadly speaking, developed out of logical positivism and the work of kindred thinkers; among the protagonists of the more recent historic-sociological approach, I have in mind particularly Thomas Kuhn and Paul Feyerabend. Let me briefly and roughly sketch the background of the controversy.

In the view of analytic empiricism, it is indeed possible to formulate characteristic rules and standards of scientific procedure, and it is specifically the task of the methodology or of the philosophy of science to exhibit, by means of "logical analysis" or "rational reconstruction," the logical structure and the rationale of scientific inquiry. The methodology of science, thus understood, is concerned solely with certain logical and systematic aspects of science which form the basis of its soundness and rationality—in abstraction from, and indeed to the exclusion of, the psychological and historical facets of science as a social enterprise.

This construal of methodology is clearly analogous to the conception of formal logic as a discipline concerned solely with questions pertaining to the validity

of arguments, the logical truth and the logical consistency of sentences, and the like, in deliberate abstraction from the genetic and psychological aspects of human reasoning. While formal logic thus does not afford an empirical, descriptive theory of "how we think," or "how logicians, mathematicians, and scientists think," the rules and criteria provided by logical theory can be employed prescriptively or normatively, i.e., as standards for a critical appraisal of particular inferences, claims to logical truth, and the like. Thus used, the principles of formal logic constitute, not categorical norms, but instrumental ones: conditions for the rational pursuit of certain objectives, such as guaranteeing the transfer of truth from premises to an inferred conclusion.

Analogously, one may say that, as understood by analytic empiricism, the principles established by the methodology of science could serve as conditions for the rational pursuit of empirical inquiry, as criteria of rationality for the formulation, test, and change of scientific knowledge claims. One well-known example of such normative-critical use of methodological maxims is the logical empiricists' rejection of neovitalism, not as false, but as being no empirical theory at all, on the ground that it violates the methodological requirement of testability-in-principle. Another example is Popper's refusal to grant the status of scientific theories to the doctrines of psychoanalysis and of Marxism on the ground that they violate certain requirements of his methodology, in particular those of falsifiability-in-principle and of avoidance of conventionalist stratagems.

The historic-sociological school, on the other hand, for reasons soon to be considered, rejects the idea of methodological principles arrived at by purely philosophical analysis, as it were: it insists that an adequate theory of scientific method must be based on a close study of the practice of scientific inquiry and should be able to explain at least some aspects of actual scientific theorizing, past as well as present.

The debate between the two schools of thought has been focused to a large extent on one fundamental and comprehensive issue, the problem of theory choice. This is the question of whether there are general principles governing the choice between competing theories in a field of inquiry, and if so, whether or to what extent such principles can be presented as conditions of rationality for scientific inquiry in the sense envisaged by analytic empiricism.

The issue arises in its most dramatic, and most widely discussed, form in reference to scientific revolutions in the sense of T. S. Kuhn, which eventually call for a choice between two comprehensive theoretical systems or paradigms, such as those represented by Newtonian and by relativistic mechanics.

Analytic empiricism might envision criteria of choice in the form of general rules determining which of two competing hypotheses or theories has the higher probability or "rational credibility," as judged by the results of experimental tests and by other relevant information available at the time. Carnap's theory of inductive logic, for example, is an impressive effort at formulating precise general criteria of this kind for the rational appraisal and comparison of scientific hypotheses, though not of complex theories.

Kuhn, on the other hand, considers the search for general and precise criteria of theory choice as basically misguided and doomed to failure. He acknowledges that there are certain general considerations, repeatedly noted also by earlier writers, which influence the decisions scientists make in the context of theory choice; he characterizes them as shared preferences or values of the scientific community; among them are a preference for theories of quantitative form whose predictions show a close fit with experimental findings; for theories covering a wide variety of phenomena; for theories that correctly predict novel phenomena; for fruitful theories, for simple theories rather than complex ones.¹

But he notes that, for reasons we will consider later, those desiderata do not suffice unambiguously to single out one of two competing theories as superior to the other; he further insists that there just are no generally binding principles that compel a unique choice on the basis of "logic and experiment alone,"² and he presents the adoption of a new theory by the scientific community in the field as the result of a process which involves deliberation on the part of individual scientists, together with efforts at mutual persuasion, but whose final outcome depends also on a variety of other factors and thus is not uniquely determined by rules of rational procedure of the kind analytic empiricism might envision.³

Yet despite his naturalistic, socio-psychological account of theory choice, Kuhn calls science a *rational* enterprise. Thus he declares: "scientific behavior, taken as a whole, is the best example we have of rationality," and "if history or any other empirical discipline leads us to believe that the development of science depends essentially on behavior that we have previously thought to be irrational, then we should conclude not that science is irrational, but that our notion of rationality needs adjustment here and there."⁴

Thus, Kuhn views his construal of scientific theorizing as affording a *descriptive-explanatory* account of certain important characteristics of the actual development of science and as equally affording a *normative* or *prescriptive* account by exhibiting certain characteristics in virtue of which that development is to be qualified as rational. Indeed, in response to Feyerabend's question whether Kuhn's account is to be read as descriptive or as prescriptive, Kuhn declares unequivocally that it "should be read in both ways at once."⁵

2. Explanation versus Justification: The Janus Head of Methodology

Taken literally, Kuhn's pronouncement is surely untenable. Descriptive sentences are not prescriptive: the former purport to tell us what *is* the case; the latter what *ought* to be done, or what would be a right or appropriate or rational course of procedure in a given situation. For example, as we noted, the principles of logical theory may be said to furnish prescriptions, in the sense of criteria, for deductively valid reasoning; but they certainly are not descriptions of how people do in fact reason—or there could be no talk of people making logical

mistakes. Analogously, a prescription or a criterion of rationality for theory choice cannot also be a description of how theory choices are in fact made by practicing scientists.

It is quite possible, however, to give a perfectly plausible interpretation to the idea, of which Kuhn's dictum is an example, of ascribing to methodological principles a Janus head with one descriptive and one prescriptive face. Consider, for example, the analytic-empiricist principle or condition, T, of testability-in-principle for scientific hypotheses or theories. On the interpretation I have in mind, the assertion that T is both descriptive and prescriptive would be a misleading conflation of two distinct claims, which I will call T_D and T_P , respectively.

T_D would be an empirical, descriptive claim to the effect that scientists do in fact share a commitment to the condition of testability and thus have a shared disposition to conform to the principle in their research. This empirical claim can evidently be invoked to *explain* why scientists in certain research situations proceed in such and such a manner, namely, why they bar from further consideration a proposed theory that they have come to consider as untestable-in-principle.

But—and here lies the confusion in the view of methodological principles as both prescriptive and descriptive—it is not the methodological norm requiring testability that explains the scientists' procedure, but the associated socio-psychological hypothesis T_D that the scientists are committed to that norm.

The second of the two claims conflated by the Janus head conception of methodological principles is an (instrumentally) prescriptive or normative one, T_P , to the effect that adherence to the testability condition is a condition of rationality for scientific inquiry.

Let us note that in so far as this second claim can be made good, it can serve to *justify* particular scientific research procedures or decisions by showing that they conform to the specified conditions of scientific rationality.

Methodological principles for which both the associated empirical claim and the associated normative claim are sound can therefore serve to "*account for*" particular instances of actual scientific behavior in the double sense of the ambiguous term "*account*." We can give an *explanatory account* of a particular case of scientific procedure or decision by pointing out that the scientists involved were committed to acting in accordance with the methodological norms; and those principles can also provide a *justificatory account* of the scientists' procedure by showing that it conforms to certain conditions of scientific rationality.

There are certain kinds of human decision and action which do, to some degree of approximation, admit of such a two-faced account by reference to pertinent methodological considerations.

Take, for example, the case of an engineer in charge of quality control who has to decide whether a given large quantity of hormone tablets or of ball bearings manufactured by his firm is to be released for sale or is rather to be reprocessed or discarded because of excessive deviations from specified requirements. His decision is based on the results obtained by performing quantitative tests on a random sample drawn from the whole batch. Given the test results, the

decision made by the engineer may well be *explainable*, and indeed *predictable*, by the empirical assumption that the engineer generally employs such and such specific decision-theoretical criteria in situations of this kind. On the other hand, the criteria here invoked—or some more general principles of mathematical decision theory, from which the criteria can be derived—can provide a *justificatory* account of his decisions by exhibiting them as rational.

Similarly, the use of the double-blind method in testing a new drug for safety and effectiveness might be *justified* by arguing that the method is *rational* in the sense of offering better chances of avoiding certain kinds of error than do simpler tests; and its application by medical investigators in a particular study might be *explained* by pointing out that, in the course of their professional training, the investigators have acquired a disposition, a habit, to use the procedure in tackling research problems of the given kind.

The interpretation I have suggested for a “Janus-headed” conception of methodological principles seems to me the only plausible one. Yet, it does not seem to me to be in full accord with Kuhn’s general characterization of scientific theory choice. Before turning to this issue, however, I will have to consider more closely the conception of methodological principles as prescriptive, i.e., as expressing conditions of rationality for scientific inquiry.

3. On the Notion of Scientific Rationality

It is interesting, but also somewhat perplexing that Feyerabend, Kuhn, Popper, Lakatos, and other protagonists in the recent methodological controversy have offered quite diverse pronouncements on the rationality or irrationality of various modes of inquiry without always giving a clear indication of the intended sense of “rationality.” For example, we find Lakatos charging Kuhn’s account of scientific theorizing with irrationalism and with appeal to mob psychology,⁶ whereas Kuhn, as noted earlier, holds that his account presents scientific research behavior as a whole as the best example we have of rationality.

What is to be understood here by “rationality,” and what kinds of consideration could be properly adduced in support of, or in opposition to attributions of rationality to science as a whole or to certain methodological rules and the corresponding modes of inquiry? I have no satisfactory general answers to these questions, but I would like to offer some tentative reflections on the subject.

To begin with, a given action or a mode of procedure cannot be qualified as rational or as irrational just by itself, but only in consideration of the goal that it is aimed at. For a man to jump fully clothed from a bridge into the river below may be rational if he intends to save a drowning swimmer and believes himself capable of doing so; it is irrational if he intends to get to the other side as fast as possible. In addition to the goal, an appraisal of the rationality of an action will have to take into account also the information available to the agent—or, more specifically, the beliefs entertained by the agent—concerning different courses of action available to him for the pursuit of his goal, and concerning the likeli-

hood of their leading to the desired result. For example, a would-be rescuer is not acting rationally in jumping into the river if he believes that he cannot swim, so that his effort would be virtually certain to fail.

To put the point somewhat loosely: a mode of procedure is rational, relative to a certain goal and a given body of means-ends information, if, judged by that information, the procedure offers an optimal chance of attaining the goal.

In so far, then, as methodological principles express rules for scientific procedure, they do not constitute absolute or categorical norms, but relative or instrumental ones: they do not categorically tell us what to do but rather what way of proceeding is rational in the sense of offering the best chance of attaining a certain scientific objective.

In regard to the procedural rules laid down in laboratory manuals for the pursuit of certain limited and highly specific objectives, such as the measurement of particular quantities or the experimental testing of particular kinds of hypotheses, it can quite plausibly be argued that—given the current scientific knowledge in the field—they qualify as optimal, and thus as rational in the sense indicated.

4. Rationality in the Scientific Pursuit of Knowledge

It is a much more elusive task to formulate instrumental criteria of rationality for scientific inquiry in general. The first problem to consider here is that of specifying the goals by reference to which such rationality is to be characterized.

Scientific inquiry is often said to be the search for truth. We might imagine that all that is known to be true, or rather, believed to be true in science at a given time is expressed by means of a large class of statements. This class will continually change, sometimes quite radically, as a result of ongoing research. One might be tempted, accordingly, to see the ultimate goal of scientific research as knowledge of the truth, the whole truth, and nothing but the truth about the world, this complete knowledge being represented by a set of sentences which would describe “everything that is the case”—including particular occurrences in past, present, and future, as well as the ultimate network of the laws of nature that bind the particular facts together.

There is no need to belabor the point that this goal represents at best an idealization, that it is unattainable to frail, finite, fallible man. But it may be of interest to note that the ideal of total knowledge as just characterized is unattainable for purely logical reasons, and no being can achieve omniscience in this sense. For the sentences expressing such total knowledge would have to be formulated in some suitable language: but no matter how rich a language may be, there are always facts that cannot be expressed in it.⁷ The contemplated conception of the goal of science is therefore untenable.

But this does not preclude the possibility of conceiving successive stages in the evolution of scientific knowledge as characterized by sets K_t of statements which are accepted at different times t , and which science has the goal of choos-

ing in such a way that they represent a sequence of systems of empirical beliefs which increasingly satisfy such desiderata as accuracy, comprehensiveness, simplicity, and the like.

Let us note now that independently of how those desiderata may be construed in detail, this conception of the goals of science does impose certain necessary conditions of rationality on any set of sentences that can qualify as "acceptable."

First, in view of the goals of science as just adumbrated, a set of sentences would be rationally acceptable only if it is capable of test and has in fact been tested with success.⁸

Next, an acceptable set must not be known to be logically inconsistent since otherwise its sentences could not possibly all be true.

Also, every acceptable set must be deductively closed; i.e., if K' is a subset of an acceptable set K and S' is logically deducible from K' , then S' must be included in K . The reason is that the deductive consequences of sentences that have been accepted as presumably true must be presumed true as well, and thus included in K .

These, then, are some modest necessary conditions of scientific rationality. They do not pertain to the issue of a rational choice between competing theories, but to the more basic question of what sets of sentences could possibly qualify as representing scientific knowledge at some time.

Note that these conditions of rationality are predicated upon the objectives of science as vaguely characterized by the desiderata mentioned above. If, instead of aiming at those objectives, we were rather seeking to formulate sets of sentences about the world that would afford us ever greater emotional security or esthetic satisfaction, then quite different standards of rationality would apply. For example, we might then do well not to accept all the deductive consequences of sentences we are accepting: for some might be disturbing or distasteful to us; similarly, rationality with respect to the alternative goal would not require us to judge the acceptability of sentences by the outcome of empirical tests: the question of factual accuracy is irrelevant to the objectives under consideration.

This point has a bearing on an idea put forward by Feyerabend. In his plea for methodological anarchy, Feyerabend maintains that "science as we know it today" may "create a monster," that "a reform of the sciences that makes it (*sic*) more anarchistic and more subjective . . . is therefore urgently needed,"⁹ and that "we can change science and make it agree with our wishes. We can turn science from a stern and demanding mistress into an attractive and yielding courtesan who tries to anticipate every wish of her lover."¹⁰

Feyerabend here urges the replacement of the goals of science by another set of goals. But however one might feel about the latter, the modes of procedure appropriate to the pursuit of these alternative objectives are not appropriate, or rational, means of pursuing the goals of "science as we know it."

If it is our goal to obtain reliable knowledge about the world, knowledge that, among other things, enables us correctly to predict future occurrences; knowledge that may enable us to escape harm or to prevent it; knowledge that indicates means for achieving desired ends, then we will have to check our hypotheses and

theories against carefully established data concerning the relevant features of the world—rather than follow, as Feyerabend puts it, “esthetic judgments, judgments of taste, and our own subjective wishes.”¹¹

Feyerabend suggests that a world in which “science as we know it . . . plays no role whatever . . . would be more pleasant to behold than the world we live in today, both materially and intellectually.”¹² But surely, one who is seriously concerned to enhance the welfare and the happiness of mankind would still have to proceed by the standards of *scientific* rationality in the search for knowledge about suitable means to achieve those ends.

5. Rationality in Theory Choice

Let us now turn to the question of criteria for the rational comparison of competing theories. Such criteria would have to determine which of two competing theories—such as the caloric and the kinetic theories of heat, or Newtonian and relativistic mechanics—is rationally to be preferred to the other in consideration of the objectives of scientific theorizing.

I have repeatedly referred to certain familiar characteristics, noted by Kuhn and others, which scientists widely regard as desirable features of scientific theories: precise, preferably quantitative, formulation; accuracy, i.e., close agreement between theoretical predictions and empirical data; wide scope; simplicity; prediction of novel phenomena, and the like.

Given that these desiderata serve to characterize the goals of scientific theorizing, it is clear that they provide us with conditions of rationality for the comparison, adoption, and rejection of theories.

But these desiderata do not nearly suffice to provide an unequivocal and general criterion which will determine which of two competing theories is rationally preferable to the other. There are at least two reasons for this, as has been noted by Kuhn and to some extent by earlier writers, such as Ernest Nagel.¹³

First, not one of the desiderata has been characterized with sufficient precision to permit an unequivocal decision as to which of two competing theories satisfies the desideratum more fully. For example, none of the various efforts made by logicians and philosophers of science to explicate the notion of simplicity for theories has yielded a satisfactory generally applicable criterion for the comparison of theories in point of simplicity. Similar remarks apply to the idea of the scope of a theory; and there are considerable problems also for the comparison of theories in regard to the closeness of the fit between their implications and the available experimental data.

Second, even if precise criteria for each of the individual desiderata were available, there would remain the task of combining them all into one overall criterion of rational preferability for competing theories. But it may, and does, happen, that of two rival theories, one satisfies some of the desiderata to a higher degree, but others to a lower degree, than its rival: which of the two theories is then to be given preference? To secure one general standard of comparison, the

various desiderata would have to be rank-ordered in point of relative importance: and there is no plausible way in sight to achieve such an ordering.¹⁴

Thus, the prospects seem bleak for a precise rational reconstruction or explication, in the sense intended by analytic empiricism, of a set of general principles of rational theory choice.

These considerations are certainly powerful. But they afford no proof, of course, of the impossibility of such rational reconstruction; and I think in fact that partial advances will be made, in the spirit of mathematical decision theory, in formulating precise principles of theory choice for more limited purposes and for theories of a less comprehensive kind than the paradigmatic ones Kuhn has in mind.

It should also be noted here that the analytic empiricist school was not much concerned with the analysis of theoretical *change*; Popper was a notable exception. The main concern of other members of the group was with such topics as induction, confirmation, probability, explanation, concept formation, and the structure and function of theories. There was no general doctrine as to how far the method of analytic explication might eventually reach—especially whether it would or could cover theory choice.

Let me return now to the pragmatist view of theory choice, as developed especially by Kuhn.

Since there are no precise general criteria of preference that are observed by all scientists, it is clear that actual theory choice in science cannot be *explained* by reference to a commitment of all scientists to such precise norms. Indeed, Kuhn stresses repeatedly that while scientists share a commitment to the desiderata mentioned, they will often understand them and their relative importance in somewhat different ways.

As for the adoption of one of the competing theories, which eventually resolves the conflict in the practice of science, Kuhn emphasizes that it is determined by the group of experts in the field. Indeed, he holds that “the very existence of science depends upon vesting the power to choose between paradigms in the members of a special kind of community,” namely, the group of specialists in the field.¹⁵ What is special about the members of this group is the high agreement in their shared standards and values—the values being of the kind of the desiderata we considered earlier. Those values are emphatically seen as not expressible in explicit precise rules determining unique preferabilities among paradigms; and Kuhn must be said, I think, to view theory choice, to a large extent, not as the conclusion of a reasoned application of explicit methodological principles, but as a nonreasoned effect, as it were, of shared attitudes, preferences, and values which the scientists have acquired, in a considerable measure by nonverbal clues, in the course of their specialized professional training.

It may be of interest to recall here that already in 1906, Pierre Duhem expressed a basically similar idea in connection with his famous argument that the outcome of a scientific experiment cannot refuse a theoretical assumption in isolation, but only a comprehensive set of assumptions. If the experimental findings conflict with predictions deducible from the set, then some change has

to be made in the total set of assumptions; but no objective logical criteria determine uniquely what change should be made. That decision, says Duhem, must be left to the "good sense" of the scientists; and he adds that the "reasons of good sense do not impose themselves with the same implacable rigor that the prescriptions of logic do. There is something vague and uncertain about them. . . . Hence, the possibility of lengthy quarrels between the adherents of an old system and the partisans of a new doctrine, each camp claiming to have good sense on its side. . . ."16

In view of the considerations presented so far, I think there is no justification for charging Kuhn's account of theory choice, as has been done, with irrationalism and an "appeal to mob psychology" (referring to the role of the scientific community in theory choice). The charge of irrationalism would have to be supported by showing that Kuhn's account flaunts certain well-established and recognized standards of rationality; and I am not aware of any rule or standard that could be seriously held to be a binding requirement of scientific rationality that has been neglected or rejected by Kuhn.

6. Kuhn and Dewey on Scientific Rationality: Some Affinities

As noted earlier, Kuhn maintains that his descriptive account of scientific theorizing is also to be read as prescriptive, and that it exhibits science as the best example we have of rationality. Indeed, he gives a concise explicit characterization of the prescriptive import he attributes to his account: "The structure of my argument is simple and, I think, unexceptionable: scientists behave in the following ways; those modes of behavior have (here theory enters) the following essential functions; in the absence of an alternate mode *that would serve similar functions*, scientists should behave essentially as they do if their concern is to improve scientific knowledge."¹⁷

There seems to me to exist a clear basic affinity between Kuhn's view of scientific rationality as expressed in the quoted passage and the pragmatist views that John Dewey held on the subject.

Dewey characterizes knowledge as "the product of competent inquiries,"¹⁸ and he comments on the characteristics of such inquiries as follows: "it may seem as if the criteria that emerge from the processes of continuous inquiry were only descriptive, and in that sense empirical. That they are empirical in one sense of that ambiguous word is undeniable. They have grown out of the experiences of actual inquiry. But they are not empirical in the sense in which 'empirical' means devoid of rational standing. Through examination of the *relations* which exist between means (methods) employed and conclusions attained as their consequence, reasons are discovered why some methods succeed and other methods fail . . . rationality is an affair of the relation of *means and consequences*, not of fixed first principles as ultimate premises or as contents of what the Neo-scholastics call *criteriology*."¹⁹ And: "Hence, from this point of

view, the descriptive statement of methods that achieve progressively stable beliefs, or warranted assertibility, is also a *rational* statement. . . ."²⁰

In his editorial introduction to a volume of articles by and about Dewey, Sidney Morgenbesser offers the following illuminating observations on Dewey's position in this matter:

Dewey took it to be evident . . . that science is good at getting knowledge and also good at presenting us with reasons for changing our beliefs. . . . That being the case, it is reasonable for philosophers interested in knowledge to study the institution best suited, as far as we know, for getting it.

Dewey seems to be saying that, were we to be asked to study firefighting, we would consider it reasonable to begin with the study of a well-run fire department justly renowned for its efficiency; we would study its history and the ways in which it had solved specific problems in the past. . . . We would not consider it reasonable to postpone inquiry because we had no clear criteria for fire, or reasonable to begin with a theory of ideal firefighters by reference to which we would judge and assess the work of the department in question.²¹

This passage throws into clear relief the strong similarities between Kuhn's views and those of John Dewey on this issue: both conceive scientific rationality in instrumental terms, as appropriateness for the acquisition of "warranted belief" (Dewey) or "improved scientific knowledge," as Kuhn puts it. Both seek to arrive at a clearer conception of rationality of means of a close empirical study of "competent inquiry" (Dewey), or of scientific research behavior, to use Kuhn's language; both hold that such empirical-descriptive study can yield insight into the way one *ought* to proceed in the rational pursuit of knowledge; and both voice strong skepticism (to put it mildly) concerning the characterization of rationality by means of "fixed first principles" (Dewey) established by more or less *a priori* philosophical analysis.

7. An Aside on Analytic-Empiricist "Explication"

As for the last of these points, I would like to note here, at least in passing, that the efforts of analytic empiricists to "explicate" norms for scientific inquiry, conditions of empirical significance, criteria of demarcation for scientific hypotheses, rules for the introduction of theoretical terms, and the like, were never undertaken in a purely *a priori* manner. Explications were always constructed with an eye on the practices and the needs of empirical science. Thus, for example, the early insistence that "empirically meaningful" sentences must be either verifiable or falsifiable by observational findings was abandoned for reasons that reflected close attention to the nature of scientific claims and procedures. The verifiability criterion, for example, was abandoned because it would deny the status of empirical hypotheses to any law of nature and to all sentences involving mixed quantification, such as "For every substance there is a solvent." The condition of falsifiability was given up for analogous logical reasons, and

also for a methodological one that had already been noted by Duhem in his emphasis on the need for auxiliary hypotheses as additional premises in deriving testable consequences from theoretical hypotheses.²²

Thus, explication in the sense of analytic empiricism has been guided to a considerable extent by close attention to salient features of actual scientific procedures and the logical means required to do justice to them. This process of rational reconstruction, as conceived especially by Carnap and some like-minded thinkers, does, it is true, lead to idealized and schematic models; but these are formulated in consideration of the kinds of scientific systems and procedures whose rationale they are intended to exhibit. In this sense, logical reconstruction, too, has a Janus head with one prescriptive and one descriptive face.

But logical reconstruction has limited itself to aspects of science that could be reflected in the syntactic and semantic features of a formalized model; whereas the pragmatist school introduces further considerations—such as shared values, scientific group processes, and the like—which lie outside the purview of explication as envisaged by analytic empiricism.

8. Rationality versus Adaptiveness in Kuhn's Account of Scientific Theorizing

Near the end of section 1, we mentioned Kuhn's insistence that his account of scientific theorizing should be read both as descriptive and as prescriptive. As I think is shown by the intervening discussion, this claim does not admit of the kind of interpretation suggested in section 2; for Kuhn denies the existence of a set of explicitly stateable general rules or norms which would fully determine rational scientific procedure, and in particular, rational theory choice.

And I have doubts about Kuhn's own construal of the claim, which was cited early in section 6. To indicate my reasons briefly: rationality seems to me intelligibly attributable only to behavior that is causally traceable to reasoning or deliberation about suitable means for attaining specified ends. But as we noted, Kuhn quite plausibly views theory choice as not fully determined by the reasoned application of instrumental methodological principles, but in part at least as a nonreasoned effect, as it were, of shared attitudes, preferences, expectations, and procedural dispositions which the specialists in a field acquire to a large extent in subtle *nonverbal* ways through their professional training and experience. That conditioning has equipped them with a shared flair for making procedural and theoretical judgments in similar but nonidentical ways without benefit of a full corresponding corpus of explicitly verbalized professional goals and methodological norms.

Thus, theory choice is clearly not presented as resulting from the reasoned application of procedural rules which, in light of the available information, are judged to specify optimal means of advancing scientific knowledge. How, then, can the process be called rational?

Kuhn does note that the peculiarities of such group choice may have certain advantages for the advancement of scientific knowledge. For example, there are then no unequivocal rules which would determine at what point in the conflict between two theories every rational scientist should shift his allegiance from one to the other. Thus, there is opportunity for different scientists to change their allegiance at different times. A few scientists will switch to the new rival of an old theory very early, and this is desirable if the potential of the new theory is to be properly explored. Yet if all scientists regularly jumped on the new bandwagon early on, the scientific enterprise would become too unstable and would eventually cease.²³

But the benefits which thus accrue to the scientific enterprise are not, of course, objectives pursued by the reasoned adoption of a group procedure that has been deliberately chosen as an optimal means to the end of achieving those beneficial effects.

It seems to me therefore that, on Kuhn's account, group processes such as theory choice would have to be viewed as akin to certain other social institutions or behavior patterns which in anthropology and sociology are said to be "latently functional" on the ground that they fulfill certain requirements for the survival or the "success" of the group concerned, without, however, having been adopted by deliberate social choice as a means to that end. Now, such modes of behavior might be called *adaptive*, but surely not *rational*: they are not adopted as a result of goal-directed *reasoning*.

Similarly, certain traits or behavior patterns acquired by a biological group in the course of its evolution may be *adaptive*; but the acquisition of such features surely cannot be qualified as a rational process; and the familiar description of such biological traits as "purposive" is not, of course, meant to imply deliberate planning.

Interestingly, Kuhn does draw an analogy between the evolution of organisms and the evolution of scientific ideas. He describes the resolution of scientific revolutions—including the adoption of a new paradigmatic theory—as "the selection by conflict within the scientific community of *the fittest way* to practice future science. . . . Successive stages in that developmental process are marked by an increase in articulation and specialization. And the entire process may have occurred, as we now suppose biological evolution did, without benefit of a set goal. . . ." ²⁴ Elsewhere, he remarks further: "For me, therefore, scientific development is, like biological evolution, unidirectional and irreversible. One scientific theory is not as good as another for doing what scientists normally do." ²⁵

These remarks are suggestive, but they need elaboration and further support. I would not know, for example, how to construe the claim that theory choice as carried out by the scientific community selects the *fittest way* to practice further science, especially in the absence of definite objectives that might yield some explicit criteria of appraisal: the claim seems to me as elusive as the assertion that a certain kind of mimicry is the fittest mode of adaption for a given species.

To repeat: an adaptive process, even if very “successful,” cannot, I think, be qualified as rational unless it is causally traceable to motivating reasoning that can be formulated discursively in the form of deliberations aimed at the attainment of specifiable ends.²⁶

Otherwise, the process may be adaptive, it may be latently functional, but it cannot be viewed as based on reasoning for which the question of rationality can be significantly raised.

To the extent, then, that scientific research behavior cannot be accounted for as prompted by goal-directed reasoning—and this may be quite a large extent—scientific inquiry would have to be viewed neither as rational nor as irrational, but as arational.

NOTES

1. See Kuhn (1970a), pp. 155ff., 199; (1970b), pp. 261f.; (1977), pp. 321f.
2. Kuhn (1971), p. 144.
3. Kuhn (1970a), pp. 198–200; (1977), pp. 321–325.
4. Kuhn (1971), p. 144; cf. also (1970b), p. 264.
5. Kuhn (1970b), p. 237.
6. Lakatos (1970), p. 178.
7. One reason in support of this assertion is provided by the semantical paradoxes. Another is suggested by considerations of the following kind: Suppose that the center of gravity, *C*, of some physical body moves through a line segment of length 1 cm during a time interval of 1 sec. This process may then be said to comprise a superdenumerable set of facts, each consisting in the coincidence of *C*, at one of the superdenumerably many time points in the one-second interval, with a corresponding one among the superdenumerably many points of the one-centimeter line interval. If we consider a language *L* which, like all scientific languages, contains at most denumerably many primitive symbols and no sentences of infinite length, then, as follows by Cantor’s diagonal argument, the set of all sentences expressible in *L* is only denumerably infinite and thus cannot contain a description of each of the superdenumerably many facts just mentioned. The argument can be extended to other languages.
8. This sketchy formulation glosses over some complicated questions of detail; but for the purposes of the present argument, those questions need not, I think, be entered into.
9. Feyerabend (1970), p. 76.
10. Feyerabend (1970), p. 92.
11. Feyerabend (1970), p. 90.
12. Feyerabend (1970), p. 90.
13. Nagel (1939), chapter 11, sec. 8.
14. See Kuhn’s observations on these issues in (1970b), p. 262 and (1977), pp. 321–326.
15. Kuhn (1970a), p. 167; cf. also (1970b), p. 263, par. 1.
16. Duhem (1962), p. 217.
17. Kuhn (1970b), p. 237 (*Italics in original*).
18. Dewey (1938), p. 8.
19. Dewey (1938), p. 9 (*Italics in original*).

20. Dewey (1938), p. 10 (Italics in original).
21. Morgenbesser (1977), p. xxv.
22. Duhem (1962), pp. 183–190.
23. Kuhn (1970b), p. 262; (1977), p. 332.
24. Kuhn (1970a), pp. 172–173.
25. Kuhn (1970b), p. 264.
26. Such causal traceability has to be construed in a sufficiently liberal sense to accord rationality to actions that are performed in accordance with certain rules which were originally established by reasoning aimed at selecting suitable means for achieving specific ends, but which the agent has learned to conform to without being fully aware of the underlying rationale.
This formulation is still sketchy and in need of fuller elaboration; but it is sufficient, I think, to make the point here intended.

REFERENCES

- Dewey, John (1938): *Logic: The Theory of Inquiry*. New York: Holt.
- Duhem, P. (1962): *The Aim and Structure of Physical Theory*. New York: Atheneum, 1962 (French original first published in 1906).
- Feyerabend, P. (1970): "Against Method: Outline of an Anarchistic Theory of Knowledge." In Radner, M. and Winokur, S. (eds.), *Minnesota Studies in the Philosophy of Science*, vol. IV (1970), pp. 17–130.
- Kuhn, T. S. (1970a): *The Structure of Scientific Revolutions*. Second edition. Chicago: The University of Chicago Press, 1970.
- Kuhn, T. S. (1970b): "Reflections on my Critics." In Lakatos, I. and Musgrave, A. (eds.), *Criticism and the Growth of Knowledge*. Cambridge University Press, 1970, pp. 231–278.
- Kuhn, T. S. (1971): "Notes on Lakatos." In Cohen, R. S. and Buck, R. C. (eds.) *PSA 1970, Boston Studies in the Philosophy of Science*, vol. VIII (1971), pp. 137–146.
- Kuhn, T. S. (1977): "Objectivity, Value Judgment, and Theory Choice." In Kuhn, T. S., *The Essential Tension. Selected Studies in Scientific Tradition and Change*. Chicago and London: The University of Chicago Press, 1977; pp. 320–339.
- Lakatos, I. (1970), "Falsification and the Methodology of Scientific Research Programs," in Lakatos, I. and Musgrave, A. (eds.), *Criticism and the Growth of Scientific Knowledge* (Cambridge, UK: Cambridge University Press, 1970) pp. 91–195.
- Morgenbesser, S. (1977): (ed.) *Dewey and His Critics. Essays from The Journal of Philosophy*. New York: The Journal of Philosophy, Inc.
- Nagel, E. (1939): *Principles of the Theory of Probability*. The University of Chicago Press.