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The History and Philosophy of Science: A Taxonomy

The temptation to ignore the temporal dimension of science, to treat it as irrelevant to the proper understanding of what science is, has always been strong among philosophers. One can detect its influence just as surely in the accounts of the nature of science given by the logical empiricists of our own century as in those put forward by the Greek thinkers of the fourth century B.C. There was more excuse for the Greeks, of course, because the empirical science of their day was still a rudimentary affair, and its developmental aspects had not had time to manifest themselves. Besides in the metaphysical climate of their time, it seemed quite obvious that the most authoritative methodological ideal for a physical science was given by the newly discovered axiomatic science of geometry. The deductive elaboration of principles themselves "self-evident" (that is, bearing their own sufficient intrinsic warrant) gave just the sort of certain knowledge of unchanging structures that the prevailing notions of Form led philosophers to seek as the goal of *epistēmē*, of "knowing" in its fullest sense. Such *epistēmē* would obviously of itself be timeless. The concepts in which it was expressed were identical in their intelligible content with the forms of natural things (Aristotle), or were imperfect images of a realm of unchanging Form, the imperfection deriving from the instability of the sensible world itself (Plato). Science in either of these views does not have a *history*, strictly speaking; the tentative groping that precedes the formulation of concept or axiom is in no way reflected in the final product, and the only specifiable *methodology* for science is clearly that of logical demonstration (syllogistic demonstration, if one makes some simple assumptions about "scientific"—i.e., universal—propositions).

The logical empiricists who have dominated the philosophy of science throughout much of the present century do not, of course, share the Greek

ideal of *epistēmē*, or the metaphysics of Form underlying it. Yet there is one important sense in which the Carnap of *The Philosophical Foundations of Physics* is a kindred spirit with the Aristotle of the *Posterior Analytics*. Each reduces the philosophy of science to a *logic* of science, to a study of science considered as a formal system. The only fruitful methodological issues, therefore, concern the way in which the different propositions in the system are related to one another, the types of inference used to validate one proposition on the basis of others. What is sought is a logical theory of confirmation which will allow one to justify a "scientific" proposition by applying a set of logical rules (whether deductive as with Aristotle, or inductive as with Carnap) to the propositions constituting the "evidence."

It is assumed by Aristotle and Carnap alike that one can make a sharp cut between that which is to be proved or justified (what Carnap calls the "hypothesis") and the evidence for it. The latter is supposed to be somehow "given"; the concepts in which it is expressed are taken to be unproblematic. Furthermore, no question is asked about how the hypothesis itself is derived in the first place, about the modifications of concept or the postulates of structure that may have been needed in order to arrive at it.[1] In defense of so dubious a set of assumptions and so drastic a limitation of goal, it is argued that only thus can purely logical modes of analysis be used, and some over-all methodological pattern established. The danger, of course, of such a procedure is that the impressive formal structures at which one arrives may be of very little service in aiding one to understand how scientists actually operate; they may turn out to be nothing more than exercises in logic, ingenious and interesting in their own right, and occa-

[1] The notion of "retroduction" which Hanson and others have taken over from Peirce in their discussion of whether or not there is a "logic of discovery" is a disagreeably ambiguous one. If "inference" is taken to be a logical method of *derivation*, there is no such thing as retroductive (or abductive) inference. Deduction and induction can be used to derive propositions that differ from the starting premises, but retroduction cannot. Insofar as it is a *method*, it is a method of confirmation only, and is in fact equivalent to what is more often called nowadays the "H-D method." If H (hypothesis) implies P (an empirically testable proposition), and P is known to be true, under certain circumstances this can serve as a warrant for taking H to be plausible (or "confirmed"). The basic pattern of retroduction is thus $H \rightarrow P, P, \therefore$ plausibly H. But this is not a mode of inferring H, in the sense of allowing us to discover it in the first place. The real crux of retroduction is to know how one is to arrive at the hypothesis contained in the major premise $H \rightarrow P$. If 'retroduction' is taken to mean the mode by which one hits upon H, given P, it is not logically specifiable and cannot be treated as a formal rule or set of quasi-formal rules after the analogy of deduction and induction. See E. McMullin, "Is There a Scientific Method?" *Proceedings of the Minnesota Academy of Sciences*. 34 (1967), 22–27.

sioned to be sure by the formal properties of empirical science, but too remote from the thought sequences that constitute "science" as the practitioners know it to warrant their being called "philosophy of science" in anything other than an honorific sense.[2]

It would be sufficient as a check on the dangers of logical "escapism" to pay close attention to science in the concrete as it is practiced here and now, in all of its diversity and variety. But in this paper, a somewhat more specific thesis will also be argued. To understand science in its modes of concept formation, in its methods of confirmation, and above all in its ontological implications, it is essential to pay attention to what may be called its "temporal dimension." It is not enough for the philosopher to consider a "slice" of scientific work at a moment in time. Rather, he has to trace the sequences by which concepts are gradually modified in the course of time, the way in which the fertility of a hypothesis over the course of time serves to confirm its validity, and the manner in which a model can continue to guide research over a long period so that one can legitimately suppose it to provide an approximate insight into the real structure of the object studied. In order to do this, the philosopher has got to draw upon the developmental aspects of science for his *evidence*. It is not merely a matter, then, of understanding the history of science in terms of philosophic categories, of making use of the philosophy of science to illuminate the history of science. Rather, what we are saying is that the philosopher must pay attention to the actual course that science has followed if he is to do his own job of understanding what science is. The connectives that he will be studying here will be historical, not logical, ones. They cannot be discovered by following some pattern of logical inference. They can only be known by determining as best one can what has *actually* happened in the course of scientific investigation in the past. If I am right in this, the relationship between the philosophy of science and the history of science is much closer than has usually been supposed.

But before we come to this specific issue, it will be necessary to give a

[2] The saying among critics of the formalist trend in recent American philosophy that "logic is the opium of the philosopher" does not, of course, imply that the philosopher can dispense with logic in his quest. Rather it is presumably meant to suggest that logical system building can all too easily become an end in itself, the philosopher's way of escape into a pleasant world of his own construction where he can gradually forget the messy and logically irreducible realities that gave rise to his system building in the first place. It is worth adding that an equally undemanding world can be reached by taking the opposite route. An unconcern for logic and for the taxing demands of correct category use is a different sort of "opiate" but one with even more serious consequences for responsible philosophy.

fairly general taxonomy of the ways in which the two enterprises of history of science and philosophy are related. These are so various that they will have to be carefully catalogued, if our main point is to emerge with any clarity.

1. Two Senses of 'Science'

When one speaks of the philosophy or the history of "science," what is meant by the term 'science'? There are two principal senses, very different in their implications for philosopher and historian alike. Science may be regarded as a collection of propositions,[3] ranging from reports of observations to the most abstract theories accounting for these observations. Let us call this S_1. S_1 is the end product of research, the careful statement in approved technical terms of something that has been empirically determined to be so, and perhaps also of a tentative explanation of *why* it is so. S_1 ordinarily contains only those definitions, theories of the measuring instruments involved, and the like, that are needed to allow another scientist, within the bounds of a research paper or book, to grasp the "data," to test their reliability if need be, and to evaluate claims made to generalize or explain them.[4] The *Principia* of Newton would be an example of S_1, as would the average paper or letter in the *Physical Review* today. It will be noted that S_1 does not contain an account of how discoveries were made, of the various false starts, of the ways in which concepts were gradually

[3] Carnap's example of taking it to consist of *sentences* rather than propositions has been followed by many. His main reason for doing this is that sentences can be exhibited; sentence tokens are perceptible singulars, comfortably unmysterious by comparison with propositions, which to him have a dangerously platonic and otherworldly status. But this leads to an altogether counterintuitive view of what science is (or indeed what logic is); it becomes dependent on the language in which the sentences are expressed (thus 'The velocity is . . .' will differ from 'La vitesse est . . .'), and even upon the word order chosen (since any change in word order or in the choice of words will make the sentence different, even though the meaning may remain unchanged). Thus H. Kyburg in his recent *Philosophy of Science: A Formal Approach* (New York: Macmillan, 1968), p. 17, takes the elements of his "science" to be English sentences. Though this is a consistent usage, the nominalistic reasons usually given in its favor seem greatly outweighed by the inconveniences of supposing a French text of physics to give us a different "science" from that given by an accurate English translation of it. A change in sentence form is not a change in the "science" conveyed; only a change in the *meaning* of what is said affects it as science.

[4] The formalist account of science supposes that S_1 should also contain *all* the so-called "rules of interpretation" that are needed to establish a semantics for the science, considered as an empty calculus. But this is never carried through in practice, and would impose an altogether formidable and perhaps impossible requirement. It is not necessary for the scientist to provide these "rules" explicitly because he assumes that most of his terms and procedures are at least partially understood in advance.

modified to fit the new problem, of the various extrascientific factors that influenced the author to adopt the theory he is proposing.

S_2 includes all of these. It is "science" considered as the ensemble of activities of the scientist in the pursuit of his goal of scientific observation and understanding. It includes the various influences that affect him significantly, perhaps unknown to himself, in this pursuit. It contains all the propositional formulations, both provisional and "finished," with the reasonings *actually* followed (not just those ultimately reported). In short, S_2 is everything the scientist actually *does* that affects the scientific outcome in any way. S_2 contains S_1; it is, however, far broader and vaguer than S_1. It is not just propositional, for it includes the building of apparatus, the making of measurements, the half-conscious speculation, the rough sketch —all brought into some sort of unity by the aim of accurately describing or explaining some feature of our experience.

It would be impossible ever to convey S_2 fully, even in the case of a relatively simple piece of scientific research. And no one tries to do so because it is S_1 in which everyone (including the scientist himself) is interested. S_1 is the measure of his achievement; it is that part of S_2 which is intersubjective, communicable, in some hopeful sense permanent. Because of its vagueness and singularity, S_2 will be difficult to comprehend; the effort to grasp it may well seem unrewarding or even futile. In the permanent record of the textbook, it is S_1 that figures, and usually in an artificial form that gives practically no clue to the real sequence of events and considerations. S_2 is, for the most part, soon forgotten; indeed, even to begin with, much of it may never have been made explicit. The interest of S_2 is only this, that in a very definite sense it serves to explain how S_1 came to be formulated in the first place.

2. History of Science

And this, of course, is of special concern to the historian. Thus, he will have to take at least *some* account of S_2. But there are very different ways of going about writing history of science. As historiography,[5] its first responsibility is to establish what the facts were: who said what, and what he

[5] There is an unfortunate ambiguity in the English word 'history.' It signifies both the sequence of events and what is written about the events. Thus, "history" of science (in one sense of the term) is about the "history" of science (in the other). The technical term 'historiography' is sometimes used for the former, but is rather cumbrous. We shall rely on context for clarification. *HS* below will, however, always mean the written account, the product of the historian.

meant by it, and what reasons he adduced. But after that, a considerable difference of emphasis is possible. At one extreme is chronicle, an establishing of the "facts" with a minimum of interpretive addition; at the other is "overview" or "applied history" where history is used to make a philosophical, theological, or political point, and the goal is discovery of an overall pattern rather than determination of contingent singulars. These divergent aims manifest themselves among historians of science as among other historians. But because what they are giving is a record not of battles or of treaties but of *ideas*, intelligibly linked with one another, they are forced to *some* extent, at least, to be interpretive. The historian of science is by definition a historian of ideas.

This suggests yet another sort of emphasis, the "history of ideas" approach now canonized by the establishment of departments and doctorates under that title in many universities in the United States. The historian of ideas has a methodologically very complex task. He has to trace a concept like *matter* or *force* or *democracy* through the writings of one or more people, subordinating the contingent historical particularities to the main aim of grasping what the concept meant and how this meaning was progressively modified. The danger of this approach (as "professional" historians are quick to emphasize) is that it may entirely subordinate history to a quite different sort of enterprise in which the connectives between, or developments of, ideas are created by the writer himself, rather than laboriously recovered from the intractable past. Ideas have a permanence and a transparency that persons and historical events lack. Thus it is tempting when tracing, let us say, the development of Newton's concept of force to pay more attention to the logical implications or plausible modifications of the concept as *we* see them than to the actual sequence as it occurred in Newton's own thought. The history of ideas can easily become a logical and analogical development whose dynamism lies in the ideas themselves and in the creativity of the person constructing it, rather than in the partial records of the free decisions and semi-opaque mental constructions of men long dead. The connectives of history are *not* always those of logic or analogy. We may wish to use a historical instance as the starting point for the exploration on our part of an idea; this is perfectly legitimate and often very revealing. But of course it is not history. The fact that it is on the whole easier to write than is "real" history need not prejudice one against its validity as a genre, but ought to serve as a warning that the two genres ought to be carefully kept apart. Their characteristic modes of evidence differ: to support a claim made in the history of ideas proper, one will have final recourse to the documents of the past; to support an assertion in the "logic of ideas," one will call upon considerations of logical inference, philosophical principle, analogical similarity, and so forth.[6]

It has seemed worthwhile to dwell on this distinction in some detail because when philosophers of science turn their attention to the history of science, it is very frequently to construct a "logic of ideas" in this sense. What bothers historians of science about this is that it often seems to them to be *masquerading* as history; it makes use of the great scientists of the past as lay figures in what *seems* to be a historical analysis but really is not. They are manipulated to make a philosophical point which, however valid it may be in itself, was really not theirs, or at least is not really shown (using the proper methods of the historian) to have been theirs. Though names of scientists and general references to their works may dot the narrative, what is *really* going on (in the sense of where the basic evidence for the assertions ultimately lies) is not history. The historian has to guard

[6] A similar difference in emphasis may be noted in the historiography of philosophy. There is an unmistakable difference of approach between, say, the average article on Kant or Aristotle written in the United States or England and one written in Germany or Italy. The "analytic" philosopher (to use a dangerously loose label) will draw upon the writings of a philosopher of the past in order to explore various philosophic options in a systematic way. The emphasis is likely to be upon conceptual clarifications and interrelations, on the *doing* of philosophy. History thus functions here as an occasion for further philosophizing of a conceptual and analytic sort. The principal criterion for a good piece of writing in this genre is the illumination it brings to some characteristic philosophical thesis or set of theses. Whether or not the historical figure whose work is being discussed was aware of the implications attributed to his position, whether or not his views shifted in the course of his lifetime, whether he was as careful as he should have been in his choice of words to convey the points he wished to make, all of these questions, while not unimportant to the "analytic" historian of philosophy, will be incidental by comparison with the central concern, which is at bottom not so much historical as conceptual-philosophical.

At the other extreme is the historian who painstakingly tries to reconstruct the thought of some philosopher of the past in all its historical singularity; he will insist on working with the texts in their original language (this need not be of nearly so much concern to the "analytic" historian); he will attempt to reconstruct the cultural and intellectual milieu out of which the philosopher wrote, and trace the various stages of his thought. The criteria of a good piece of work here are the degree of assurance given that this *is* indeed what the philosopher said and meant, and the quality of the understanding afforded of how he came to say it. This sort of historiography focuses on the historical singular in all its contingency, whereas the other abstracts from this contingency in order to construct analyses of some permanent philosophic interest. The important thing to note here, as in the case of history of science, is that *both* genres of research are perfectly legitimate; they fulfill different functions, each of them an indispensable one. It is only when debates arise (as they so frequently do) about how "history of philosophy" *ought* to be undertaken that one comes to realize how easily the use of a single label can lead one to assume that there is (or ought to be) a single methodologically well-defined enterprise corresponding to it.

against rejecting this genre entirely, simply on the grounds of its not being history. What he ought rather demand is that it clarify its credentials, and avoid the suggestion that its warrant is a historical one, i.e., certain things that happened in the past.

Newton scholars, for instance, sometimes get annoyed when they read a piece by a philosopher who singles out a passage in the General Scholium or in the Queries, say, and analyzes its implications in detail, without adverting to the fact that the passage went through many drafts before attaining final form, or perhaps was later modified or even repudiated by Newton. The crucial question here is whether the philosopher is using Newton as *evidence* or as *illustration*. Is he claiming the fact that Newton argued in a particular way as a support for a more general claim about the nature of science, thus explicitly relying on the authority of what Newton said or did? Or is he merely taking a passage from Newton's work to illustrate a philosophic claim which stands in its own right (so that if a historian can show that Newton did not really write the passage or later repudiated it, the philosopher's point is left basically unaffected)?

Another sort of problem arises when a historian of ideas uses a methodological notion (the hypothetico-deductive method, for instance) or first-order concept (like *mass*) to discuss the work of some scientist of the past, even though the concept or distinction was only elaborated in explicit form in later times. The critic may urge the danger of anachronism here. Thus, even the title of Clagett's classic work, *The Science of Mechanics in the Middle Ages*, has been criticized on this score. Likewise, one will find reviewers objecting on principle to discussions of Plato's concept of matter, of Galileo's notions of virtual work or momentum, of Descartes's use of hypothetico-deductive modes of confirmation. They argue that these writers were not explicitly aware of such concepts, in fact had no terms corresponding to them; thus, it must inevitably mislead readers to find scientific work described and analyzed in terms that were only developed later on. Even the translation into a modern language of earlier texts (a translation into English, say, of a medieval Latin commentary on Aristotle's *Physics*) would seem to face the same objection; terms like 'force' and 'velocity' have been sharpened so much even in ordinary usage that there are simply no one-word equivalents in English today for many of the basic terms of medieval natural philosophy.

The solution, of course, is that the reader ought to be warned, indeed ought to realize without need for warning in the case of a translation, that the precisions and implications of the modern terminology cannot be carried backwards in time without a specific warrant for doing so in each case. The use of contemporary *methodological* distinctions is clearly valid when analyzing the actual methods followed by early scientists; to say that Galileo made use of a hypothetico-deductive method of confirming that the lunar surface is like that of the earth is an illuminating and accurate way of rendering in modern terms what Galileo *did*. It does *not* necessarily imply that he had a hypothetico-deductive theory of confirmation explicitly worked out.[7] The tracing of a concept is a more difficult and riskier affair. One cannot tie oneself to the use of a particular term as the necessary and sufficient condition for the effective presence in a writer's thought of a particular concept. Thus, it would be overly narrow to hold that there could be no question of a concept of matter in the pre-Socratics just because they had no specific agreed-upon term for matter. The criteria here are too complex to enter into in detail,[8] but in general one has to see a scientific or philosophical concept as a response to a certain problematic or set of problematics, and it is often possible to trace the prehistory of such a problematic and analyze responses to it analogous with the later more specific ones associated with a particular term like 'space' or 'mass' or 'inertia.'

[7] Take a further illustration. In my essay "Empiricism and the Scientific Revolution" (in *Art, Science and History in the Renaissance*, ed. C. Singleton, Baltimore: Johns Hopkins Press, 1968, pp. 331-369), I attempted to contrast two modes of evidence, one "intrinsic" (where a scientific statement is supposed to carry conviction in its own right, by virtue of its conceptual interconnections, once it is fully understood) and the other "extrinsic" (where one calls on something extrinsic to the statement itself—an experiment or series of observations, for example—in support of the statement). These are sometimes labeled the "rationalist" and "empiricist" notions of evidence, but this is somewhat misleading since the so-called "rationalist" theory (as one finds it in Aristotle, for example) is by no means independent of specific experience by means of which the concepts originally came to be understood. One can trace the tension between these two ideals of evidence through Greek and early medieval times (when the "intrinsic" mode dominated) and the late medieval period (when the nominalists argued for an "extrinsic" mode) to the seventeenth century (when there was a gradual shift to the "extrinsic" mode, although the major scientific figures of the century still on the whole took the "intrinsic" mode to be the ideal, even though rarely attained in practice). This is a valid framework for an elucidation of the science of that period; one simply scrutinizes the actual methods used, and the occasional remarks on method made (though these latter were often belied by the former). It would be invalid only if one were to imply that the writers themselves were fully aware of what the distinction connoted, of what, for example, the empiricist theory of proof later came to be.

[8] I have discussed them in the Introduction to *The Concept of Matter in Greek and Medieval Philosophy* (Notre Dame, Ind.: University of Notre Dame Press, 1965), and again in more detail in the Introduction to *The Concept of Matter in Modern Thought* (Notre Dame, Ind.: University of Notre Dame Press, in press).

3. Two Approaches to the History of Science

Perhaps the most important difference of emphasis among contemporary historians of science corresponds roughly to the distinction drawn in the previous section between two senses of the term 'science.'[9] HS_1 is the history of science considered as a set of general propositions, together with the properly "scientific" evidence thought to count in their favor. It focuses on published work, on final versions, on pieces of research that ultimately proved successful. And it considers them not in the order in which they were carried out, but in the order in which they were ultimately presented to the world, as specific already-formulated hypotheses with their supporting evidence clearly delineated. HS_1 will clearly appeal to the scientist, because it tells how the body of propositions today called "science" gradually came to be, at what point particular concepts or theories originated, who was responsible for them, how one work influenced the ideas of another (as shown, for instance, by references to the later work). If a historian has a considerable expertise in science (so that he can venture confidently from the safe shallows of Newtonian mechanics into the deeps of electromagnetic theory or general relativity), he is quite likely to prefer HS_1, because he has the ability to elucidate the conceptual implications and connections in the original work, evaluate the sort of evidence that was used, and discuss the gradual modification of the theories and concepts involved. Such a historian may even be a trifle disdainful of the "biographical" approach of HS_2, considering it a resort for people who really don't understand the taxing mathematical, logical, and experimental complexities of the work under study.

HS_2 takes a broader aim. It attempts to understand how the specific pieces of S_1 came to be; it makes much of the unpublished draft, the penciled correction; it seeks out the social, psychological, religious influences affecting the author, as well as implicit affiliations of a scientific or philosophical kind. The documents it uses will not be just the published S_1, but also letters, unpublished manuscripts, and surveys of the philosophy, the theology, even the politics of the day. The practitioner of HS_2 will be conversant with caches of correspondence in various corners of the world; he

[9] The distinction drawn here between two approaches to the history of science is similar in many ways to that between "external" and "internal" history drawn elsewhere in this book, in Mary Hesse's paper, for example. These terms are so vague in this context, however, that a variety of opinions tend to develop about what is properly "external" to science. And of course what is adjudged to be "external" in one epoch (as Dr. Hesse notes) may very well not be in another.

will be well read in the general historical background of the period he studies. He will be a man of travel grants, with his own laboriously assembled microfilm archive. As the National Science Foundation has long since discovered, HS_2 is one of the most expensive types of scholarship! The man with the history degree, with general training in historical research, is likely to lean toward HS_2, just as the man with a primary degree in science is somewhat more likely to opt for HS_1.[10] The man of HS_2 may be tempted to consider HS_1 extremely narrow and not very helpful. He will incorporate in his own work all the HS_1 he considers relevant, but the emphasis given it and the interpretations given to its methodological and logical structures are likely to be very different from those given by a "straight" HS_1 man.

Between HS_1 *and* HS_2, there is a continuum of possible intermediates. Making the distinction as sharp as I have done above may well seem rather artificial. Yet it does serve to suggest how far the "extremes" in historiography of science may be from one another. One has only to think of standard works of history of science as remote from one another in genre as are E. T. Whittaker's *History of Theories of Aether and Electricity* and Pearce Williams's *Michael Faraday*. Even the most cursory glance at these two books shows that their aims are markedly different. The first is concerned with science as a body of theories supported by experiment. To understand how it has been gradually built up over the years is to understand better the vast, and of itself nontemporal, conceptual network, "science," in terms of which the universe is partially categorized and explained. The second book, on the other hand, is concerned with science as a human activity; it attempts to understand Faraday's science as something to which all aspects of the great scientist's background and personality contributed. Its aim is thus to explain how this *particular* piece of science occurred, what the relevant causal lines leading to it were. Whether these lines are capable of generalization, whether they occur in all or most cases of scientific research, is of lesser concern to the author, though he does have some suggestions to make on that score. And where Whittaker describes Faraday's scientific results and analyzes their significance, Williams asks how he arrived at them, what the false leads were, and how his religious beliefs affected his estimate of the relative plausibilities of the various explanatory hypotheses that occurred to him.

[10] These are, of course, outrageous sociological generalizations which would need much more evidence in their support than the random sample of historians known to one person. But I am fairly sure that they are not *too* wide of the mark.

It is obvious that the latter genre is much closer to that of the "ordinary" historian who reconstructs the contingent singular past event and tries as best he can to say why it happened the way it did. HS_2 is, in fact, like "ordinary" historiography in its general method of approach; it differs mainly in the internal conceptual complexity of part of its material. HS_1 is very different from "ordinary" history writing because it leaves aside most of the clues the historian would think relevant, and concentrates only on those features which can be logically or methodologically reduced to some pattern. It is still history, because it attempts to isolate a fragment of S_1 as it was first stated, precisely as it was first stated. But it leaves aside as irrelevant all the contingent historical circumstances surrounding this statement; only those circumstances are chronicled which are *logically* related to it, necessary therefore if one is to understand what S_1 *meant*, or what its precise warrant was. Thus the aim of HS_1 is not exclusively historical; in part at least, it is to understand S_1 as a piece of science, whereas HS_2 seeks to understand why the historical event, which is the formulating of S_1, happened as it did. In a certain sense, then, HS_1 terminates in a universal, where HS_2 terminates in the historical singular. The criteria for a good piece of HS_1 involve our understanding of the piece of science as science, whereas for HS_2 it would be our understanding of how that "piece of science" happened when and how it did.

4. Three Approaches to Philosophy of Science

In attempting to define what is meant by a "philosophy" of science, the first problem one encounters is the notorious vagueness of the term 'philosophy.'[11] Unlike historiography, which is relatively well defined in its method and in the types of evidence on which it draws, "philosophy" can in practice be anything from a cloudy speculative fancy to a piece of formal logic. The term has become almost hopelessly equivocal in modern usage; even in academic contexts, despite the unity implied by a label like 'Department of Philosophy,' there can be the widest divergence concerning what the aims and methods of the "philosopher" should be. Five strands might be roughly separated. Something may be called "philosophy" because of (1) its concern with the "ultimate causes" of things; or (2) the immediate availability of the prescientific or "ordinary-language"

[11] I have argued elsewhere that a failure on the part of those writing what they call "philosophy of science" to say what the term 'philosophy' means for them leads to this label (at present an honorific one) being used to cover ever broader areas of thought. See "Philosophies of Nature," *New Scholasticism*, 43 (1968), 29–74.

or "core-of-experience" evidence on which it rests; or (3) the generality of the claims it makes; or (4) its speculative character, allied with difficulties in confirmation, particularly empirical confirmation of any kind; or (5) its "second-level" character, the fact that it is concerned with other first-level disciplines rather than with the world directly. In practice, some ill-defined combination of these criteria will usually be operative. It is the last (and most recent) of these senses that seems most relevant to the notion of a "philosophy" of science. It is "philosophy" just because it *is* a second-order critical and reflective enterprise. The label 'philosophy of science' is of course of very recent origin, even more recent than the separation of the domains of "science" and "philosophy" from which it takes its origin.

There are, it would seem, two quite different ways in which one could set about constructing a reflective philosophy of science (*PS*). One could look outside science itself to some broader context, and in this way derive a theory of what scientific inquiry should look like and how it should proceed. We shall call this an "external" philosophy of science (*PSE*), because its warrant is not drawn from an inspection of the procedures actually followed by scientists. *PSE* will often appear as normative, because it can serve to pass judgment on the adequacy of the methods followed in a particular piece of scientific work, or even in scientific work generally. Since it does not rest upon any analysis of the strategies actually followed by those who would regard themselves as "scientists," it need not be governed by current orthodoxies in this regard. Thus, *PSE* need not take account of the history of science, except as it furnishes illustrations. *PSE* in no way rests upon *HS*, though it must obviously give *some* sort of plausible reconstruction of *HS* if it is to be taken seriously. If a *PSE* diverges radically in its implied norms from what scientists actually appear to be doing, it is likely to be challenged, and its starting point may be called into question. Yet a surprisingly large divergence can be tolerated; it will be said simply that the "science" under discussion falls short of what "science" ought to be. One thinks, for example, of the account of the nature of science given by Aristotle in his *Posterior Analytics*, so obviously and widely at variance with what might have been inferred from his own extensive contributions to the science of biology.

There are two main types of "external" warrant for an account of the nature of science:

1. *PSM*: If one views science as the ideal of human knowing, or as one specific type of human knowing, it is plausible to suppose that its nature

can best be understood by beginning from a general theory of knowing and being. This was essentially the starting point from which both Plato and Aristotle commenced in their discussion of the nature of science; to a large extent it was still the framework within which Descartes constructed his *Discourse on Method*. Such a *PS* can begin from an epistemological or from a phenomenological starting point; it will derive from a more general "metaphysical" theory, therefore; hence the label '*PSM*.' Since it is a *PSE*, the "metaphysics" here should not be a science-based one (otherwise the warrant would not be extrinsic). When we speak of a *PSM*, therefore, it will be assumed not only that its warrant is basically a "metaphysical" one (another admittedly vague label), but also that it is prior to any analysis of the actual procedures followed in science.

2. *PSL*: To the extent that science is thought of as a logical structure of demonstration or of validation, *PS* becomes akin to a formal logic, whether a deductive logic of demonstration (like the Aristotelian theory of syllogism) or an inductive logic of confirmation (such as that constructed by Carnap). Such a *PS* can be judged as one would any other purely formal system, in terms of consistency, simplicity, and so forth. Only the most general specifications of what would constitute "demonstration" or "inductive evidence" may be needed to get the system construction under way. There may be very little reference to present or past scientific practice; it is not suggested that this logic is the one actually followed by scientists in their work of discovery or of validation. Rather, it is a reconstruction, an idealized formal version of what, for example, proof *really* amounts to in science, whether the scientist knows it or not. It may be interpreted normatively as suggesting how, for instance, scientists *should* proceed when faced with two competing theories. Or it may be intended *only* as rational reconstruction of a general logic that is intrinsic to scientific inquiry, though not capable of being made operationally specific enough to serve as a methodological manual for the scientist wondering how best to do his work.

The best known recent instance of a *PSL* of this latter type is Carnap's inductive logic. This is a formidably complex and logically fascinating formal system relating various types of confirmation in a mathematically expressible way. But no one has been able to suggest how the basic "measure" utilized by Carnap (that of degree of support of a hypothesis, *H*, on the basis of evidence, e) could be related to any actual hypothesis/ evidence situation in empirical science. Thus, though Carnap's logic has been (and continues to be) of great interest to logicians, it is not clear that it has led to an understanding of what goes on in scientific inquiry. Yet it qualifies as a *PSL* in intention, at least; the reason for undertaking it, and the general conceptual framework of *hypothesis, evidence, plausibility*, in terms of which it was developed, derived from empirical science. But the justification for it as an intellectual construction lies in its logical interest, rather than in any insight it provides into the actual procedures of the scientist.

Discussions of the nature of science up to the seventeenth century were nearly always "external" in character, though one occasionally finds in the later medieval and Renaissance periods some analysis, for instance, of the actual methods of "composition" and "resolution" followed by scientists. The theory of science was based on a prior metaphysics or on an autonomous logic.[12] And even though the pioneers of the scientific "revolution" purported to be drawing upon new sources for their methodology, they were still much closer to the *PSM* and *PSL* of the Greek tradition than they were willing to admit. Though Bacon, Boyle, Huygens, and many others depended on their knowledge of the practice of science in their analyses of methodological and epistemological issues, it was only in the nineteenth century that writers like Whewell and Mill took this new source of *PS* with complete seriousness.[13] It is easy to see why the astounding successes of the new mechanics, and the beginnings of a new era in biology, geology, and chemistry, should make it for the first time plausible that if one wished to understand the nature of science, one should look at what scientists actually *do*. No longer did "science," a stable knowledge of the world, seem a remote ideal; in terms of practical success, it had clearly been achieved already.

3. *PSI*: In contrast, therefore, with *PSE* is a philosophy of science which relies for its warrant upon a careful "internal" description of how scientists actually proceed, or have in the past proceeded. The function of different methodological elements (law, hypothesis, predictive validation, etc.) is studied not in the abstract, but in the practice of the scientists themselves.[14] This approach presupposes that one can already identify competent scientists and successful pieces of research. *PSI* is based on what scientists *do* rather than upon what they say they are doing; when contemporary scientists set out to give an account of the nature of scientific method, they can sometimes be as remote from scientific practice as were Aristotle or Descartes. They may have some sort of idealized *PS* in mind, an oversimplified isolation of one procedure, perhaps, or even a *PSM* in disguise.[15] A *PS* constructed by a scientist is not necessarily *PSI*, and if it is *PSI*, it is not necessarily accurate *PSI*. The evidence on which *PSI* is based is a descriptive account of the procedures by which empirical science is built; though the testimony of scientists is of primary importance in achieving such a description, such testimony cannot be taken without question, especially if there is reason to suppose that the scientist allows a *PSE* or an overly simplified *PSI* to color what he has to say of his own procedures.

By comparison with *PSE*, *PSI* is a relatively empirical undertaking, not very different in this respect from an empirical science itself. If one wishes to give a *PSI* analysis of the role of models in science, one begins from a carefully documented review of how scientists have made use of models. *PSI* thus differs in several important respects from *PSE* (whether of the traditional *PSM* or *PSL* varieties). It is expressly second-level, in that it takes another intellectual discipline as its object of study. It presupposes an already-functioning methodology, whose pragmatic success is a sufficient warrant of its adequacy as a heuristic. There is no need to ask what science *ought* to look like, in some abstract sense. The very success of modern natural science in prediction and control gives a sufficient reason for taking it as an object of analytic epistemological study in its own right. Furthermore, the claims made in *PSI* are relatively easily confirmed, as a rule; they can usually be settled by an analysis of the interrelations of some elements of descriptive methodology. There is not much affinity, in conse-

[12] These were combined in the dominant Aristotelian account of science of this period. See, for example, A. C. Crombie, *Robert Grosseteste and the Origins of Experimental Science, 1100–1700* (Oxford: Clarendon Press, 1953), chapters 4, 11; and E. McMullin, "The Nature of Scientific Knowledge: What Makes It Science?" *Philosophy in a Technological Culture*, ed. G. McLean (Washington, D. C.: Catholic University of America Press, 1964), pp. 28–54. See also the first half of my "Philosophies of Nature."

[13] See my "Empiricism and the Scientific Revolution."

[14] A good example would be Leonard Nash's recent work, *The Nature of the Natural Sciences* (Boston: Little, Brown, 1963).

[15] Examples are not hard to find. One recalls the "pointer-reading" account of scientific method on which Eddington built his elaborate "Fundamental Theory"; Bridgman's operationalism also comes readily to mind as an illustration. A recent delightful example is an article by the biochemist J. R. Platt: "The New Baconians," *Science*, 146 (1964), 347–353. He reduces scientific inquiry to what he calls a "Baconian" method of "strong inference" which he compares to climbing a tree, each fork corresponding to a choice between alternative hypotheses; the decision on which way to go at each fork is made on the basis of crucial experiments ("clean results"). He attributes the recent rapid advance of biochemistry to its fidelity to this simple method, and suggests that other parts of science could enjoy equal success if only they could see the methodological light.

quence, between the practitioner of *PSI* and the metaphysician or moralist. (There is just as little affinity, but for different reasons, between the exponents of *PSM* and *PSL*.) This may help to explain the not infrequent tensions between philosophers of science and other philosophers; the closer to *PSI* the former are, the more likely they are, for example, to plan their conventions in conjunction with those of scientists or historians of science rather than those of philosophers.

Why are *PSI* and *PSL* with their heavily empirical or formal emphases called "philosophy" at all, then? It might seem that *PS* in either of these two genres could just as readily be called "science of science" or "logic of science," or be given an entirely new label. The main reason for retaining the name of "philosophy" is that the logical analysis of method and the drawing out of conceptual implications characteristic of both *PSI* and *PSL* present obvious analogies with the techniques traditional to the philosopher. Granted that the type of evidence called on and the mode of confirmation employed are rather different, there is still a sufficient family resemblance based on the procedures followed. And there is also a sufficient cross-relevance between *PS* of the *PSI* or *PSL* variety and other parts of "philosophy" to make it desirable that they should be studied in conjunction with one another. Besides which, we have already noted the modern tendency to describe all second-order critical discussions, whether they are of art, of history, of literature, of law, as "philosophy of . . ."

In any discussion of the relevance of history of science to philosophy of science, it makes a very great difference which type of *PS* one has in mind. Clearly, history of science may be of little concern to a practitioner of *PSE* (whether *PSM* or *PSL*), though he cannot be wholly unconcerned about serious divergences between his own account of the nature of science and the course science has actually followed. And he may want to draw upon *HS* for illustrations and indirect support. But the philosopher whose interest is *PSI* has to take history of science very seriously. It furnishes not merely examples but the basic evidence from which his inquiry has to begin. More exactly, *PSI* can begin either from a historical review or from an account of contemporary practice (or both). But even if a *PSI* practitioner prefers to focus on the details of contemporary practice, leaving the historical dimension of this practice out of account, he cannot draw any sharp distinction between past and present, and thus will have to admit the potential relevance of *HS* to what he is doing, whether he chooses to make use of it or not.

It might be argued that all there is of methodological import in the history of scientific development is likely to find a place somewhere in contemporary scientific practice, so that explicit recourse to the past history of science is unnecessary to the philosopher of science. If he bases his analysis on what scientists are currently doing, he is taking advantage of the learning process that has gone on in science itself over the centuries, as scientists have gradually become more expert in how to go about their experimental and theoretical researches. A pragmatic type of validation procedure has, after all, been at work in science itself; the methodology of today's physicist is by no means the same as that of Galileo.

While this is true up to a point, it will be argued below that *PSI* has to take into account the *developmental* aspect of science, the characteristic ways in which a theory, for instance, is modified in the face of successive anomalies. To do this properly, it will not be enough to examine the science of a particular moment; one will have to follow it over a period, even a considerable period. Besides, it may be important to note the ways in which the procedures of the scientist have changed since Galileo's time and to ask why these changes have occurred. Furthermore, historical distance allows one to isolate and understand much better the influences at work in a piece of scientific research (as in any other human activity). The philosopher may learn more about the nature of explanation in dynamics from a careful analysis of, say, the writings of Newton and his contemporaries than from a review of contemporary relativistic dynamics, not only because the simpler seventeenth-century context may reveal features of method that are more difficult to uncover today, but also because the variety of influences at work on Newton, as well as the different nuances his thought took on in successive drafts of his work, permit a more detailed analysis than would ordinarily be possible in the case of some contemporary piece of research. In summary, then, the history of science is relevant to *PSI* for two different sorts of reason: (1) because it provides complete case studies, of a kind one could not recover from contemporary science; (2) more fundamentally, because it allows one to study science in its all-important temporal dimension.

5. *HS* and Some Philosophers

The distinction drawn above between *PSM*, *PSL*, and *PSI* ought not be taken to imply that any given piece of *PS* conforms to one and only one of these patterns. In practice, one finds philosophers of science calling upon all three sorts of criteria, sometimes even in the same piece of writing, and intermingling them in very complex (and not necessarily consistent) ways. Nevertheless, it is often possible (and when possible, helpful) to characterize a piece of writing in *PS* under one or other of the categories above, depending on which of the three types of warrant seems to dominate in it. There is no reason why an author could not combine logical, metaphysical, and descriptive-empirical elements in constructing a philosophy of science. But it is of paramount importance that he not be misled (or that he not mislead the reader) about what the balance between them in his argument really is.

In particular, it is easy for an author to suppose that what he is presenting is *PSI* when it is in fact *PSE*. This is all the more likely to happen today; because of the sheer weight of evidence available on what the procedures of the scientist are, it is hazardous to put forward any philosophy of science nowadays without some attempt, at least, to make it look like *PSI*, that is, to make it appear to derive from a familiarity with current scientific practice or from an intimate knowledge of the history of science. Yet if, in fact, the genre of writing is really that of *PSM* or *PSL*, there is an obvious danger that the wrong criteria of evaluation will be applied.

Philosophers of science of even the most "external" sort have always made *some* reference, at least, to what they believe the scientific practice of their day to be. But they have not usually turned their attention to *HS*; in the logical-empiricist tradition which has dominated much of the work in *PS* of our century, virtually no attention has been paid to *HS* until recently, on the grounds presumably that the logical structures which were the philosopher's concern exhibited themselves readily in any random slice of contemporary scientific inquiry. It did not seem necessary or even desirable, therefore, to undertake first the difficult work of the historian of science as a means of carrying out the task of the philosopher of science.

This has changed in the last decades, and now one is beginning to find case histories dotted here and there throughout the journals of *PS*. But the change has brought with it some methodological headaches. How exactly *should HS* be incorporated in the philosopher's work? What weight should be given it? To what extent ought it be regarded as normative? In order to illustrate some of the difficulties that can arise, it may be useful to glance at two lengthy recent monographs. Both were written by pupils of Karl Popper, and this prompts one to ask an initial question: how should

Popper's own influential work be characterized, as *PSE* or as *PSI*?[16] No one has concerned himself more with the demarcation between "science" and "metaphysics" than has Popper, so that the distinction between *PSM* and *PSI* is a valid and important one to make in situating his own work. Does his theory of falsification start from an analysis of the ways in which scientists actually evaluate alternative theories? When he says that of two competing theories the one to be preferred is the one with more "empirical content," does this reflect a discovery he made by observing what sort of consideration influences scientists who have to choose between theories? The answer would on the whole seem to be that it does not. Popper's *PS* derives mainly from a general theory of rationality, with a considerable amount of logical analysis thrown in. It is, in fact, a good instance of a *PSE*, more specifically a *PSM*. The frequent references to scientific practice and even to instances from the history of science serve to define the conceptual-logical problem: By what sort of criteria must one suppose scientific growth to be guided if it is to be regarded as a "rational" process? The answer to this problem cannot, in his view, be found in an inspection of *HS*, both because it is very difficult to reconstruct what criteria actually *did* weigh in any historical instance of the replacement of one theory by another, and also because scientists are fallible in their methods. They can, for instance, make mistakes in holding on to established theories longer than they should, thus evincing the very reverse of the "critical attitude" that should characterize the good scientist. This "critical attitude," the attitude of someone who has comprehended the subtle methodology Popper proposes for the theoretical scientist, is an ideal to be sought after, therefore, not one which can necessarily be discovered by watching scientists go about their work. It is ultimately grounded upon considerations of the phenomenological "don't you see that it must . . ." variety, rather than upon a chronicle of the strategies that "successful" scientists have followed.

Some recent essays by Imre Lakatos, developing a variant of the Popper *PSM*, illustrate in a quite explicit way one possible (but ultimately rather equivocal) role of *HS* in a philosophy of science. He discusses two well-

[16] It may be worth reminding the reader once again that this distinction is not a sharp one, both because considerations of "external" and "internal" sorts are likely to be interwoven and also because a "metaphysics" today (in this context, an account of rationality) cannot help but be influenced, no matter how a priori or external its methodology may be, by some implicit notions on how scientists go about their work. The point of the question above is, then: what type of warrant appears to be the primary one in Popper's own practice as a philosopher of science?

known historical instances of "research programs" (i.e., general hypotheses which oriented research over a considerable period): Prout's hypothesis that the atomic weight of every pure element is a whole number and Bohr's light-emission hypothesis. Lest the reader be misled by the pages of detail in which he appears to be following the historical course of these hypotheses, he prefaces his remarks with an interesting methodological note: "A historical case-study to my mind must follow the following procedure: (1) one gives a rational reconstruction; (2) one compares this rational reconstruction with actual history, and tries to appraise actual history critically, and rational reconstruction self-critically. Thus any historical study must be preceded by a heuristic study: history of science without philosophy of science is blind. In this paper it is not my purpose to go on seriously to the second stage."[17] What Lakatos says is that in these papers it is not his purpose to write "actual history," but rather to give a "rational reconstruction" of history in the light of his general theory of science. His purpose is to illustrate this theory: "The dialectic of positive and negative heuristic in a research programme can best be illuminated by examples. Therefore I am now going to sketch a few aspects of two spectacularly successful research programmes."[18]

His aim in these examples is clearly not to provide *evidence* for his theory. Nor (despite appearances) is it to "illustrate" the theory in the ordinary sense, i.e., by pointing to actual sequences in the history of science which can be illuminated by applying the theory to them. The notion of a "rational reconstruction" (which is to precede the attempt to find out what exactly *did* happen) precludes the idea that these examples are to serve as historical illustration in the ordinary sense. Rather, they are imaginary or quasi-imaginary examples, recounting what *ought* to have happened in the course of development of physical hypotheses such as Prout's or Bohr's, if the author's theory of heuristic had been followed in all cases by the protagonists. Lakatos is not claiming that this is what *did* happen, only that ideally it is what *ought* to have happened. What is being "illuminated" here, therefore, is not the "historical example" but the theory itself. The "illustration" is a constructed one, after the fashion of a

[17] "Falsification and the Methodology of Scientific Research Programmes," in *Criticism and the Growth of Knowledge*, ed. I. Lakatos and A. Musgrave (Cambridge: Cambridge University Press, 1970), section 3c. This is a longer version of "Criticism and the Methodology of Scientific Research Programmes," *Proceedings of the Aristotelian Society*, 69 (1968), 149–186.

[18] This and the next two quotations are from the beginning of section 3c of the paper.

textbook problem meant to illustrate some point made in the text. To the extent that the anecdote conforms to "actual history" (and this for Lakatos is a separate problem), the discussion could, of course, serve as an "illumination" of history. But he is not concerned with this issue.

That this is really what Lakatos means becomes even clearer in the course of his discussions of the Prout and Bohr hypotheses. He begins his discussion of the former by saying that Prout "knew very well that anomalies abounded but said that these arose because chemical substances as they ordinarily occurred were impure, that is, the relevant experimental techniques of the time were unreliable." This sounds definite enough, but then one's attention is caught by a dismaying footnote: "Alas, all this is rational reconstruction rather than actual history. Prout denied the existence of any anomalies." The same happens with the Bohr story; a footnote tells us that the account of the discovery of electron spin given in the text is a "rational reconstruction," which does not correspond to the actual sequence of events.[19] Or again, to illustrate a "progressive theoretical problem-shift," he gives a detailed "imaginary case" involving a deviation in a calculated planetary path and the series of attempts made to explain it.[20]

From all this, one might think it could confidently be concluded that *HS*, qua history, plays no role either as warrant or as illustration in Lakatos's *PSM*. He does leave open a comparison of his "rationally reconstructed history" with "actual history" (in the first quotation above), allowing a modification in the former as one possible outcome. Yet it is assumed that this sort of comparison is a separate and optional enterprise; his *PSI* can apparently be constructed without it. This is *PS* at its most "external"; the role assigned here to "history" is likely to fill even the broadest minded historian with foreboding! But even more troublesome is the quite different role assigned to *HS* in his critique of methodological ("naive") falsificationism.[21] He notes the difficulty of rejecting any particular theory of rationality in science before a general theory of rationality is constructed.

[19] *Ibid.*, section 3c, toward the end.

[20] *Ibid.*, section 2a (3).

[21] Lakatos distinguishes between three types of "falsificationism," all of them associated with Popper's name. "Dogmatic falsificationism" is the simplified version which supposes that science grows when theories are refuted by facts. Popper never defended this in his published work; hence it is attributed to "P_0," ("pseudo-Popper"). "Methodological (naive) falsificationism" is a modified form of conventionalism, in which "refutation" still plays the central role but in a very weakened form. It is found especially in Popper's *Logik der Forschung* (Vienna: Springer, 1934); hence "P_1" ("proto-Popper"). "Sophisticated falsificationism" is the third option, the one Lakatos himself defends; it involves estimates of the "empirical content" of a theory and emphasizes

Instead of going on to specify such a theory, in some preliminary way at least, he unexpectedly makes an appeal to *HS* as the means of excluding various incorrect theories of rationality: "If we look at history of science, if we try to see how some of the most celebrated falsifications happened, we have to come to the conclusion that either some of them are plainly irrational, or that they rest on rationality principles radically different from [those of naive falsificationism] . . . Indeed, it is not difficult to see at least two crucial characteristics common to both dogmatic and methodological falsificationism which are clearly dissonant with the actual history of science . . ."[22]

Note that *HS* now appears as *warrant* for what the philosopher of science may assert. In this event, a "rational reconstruction" will not do, and may easily lead to circularity. If a *PS* were to be constructed along the lines suggested by the last quotation, it would clearly be a *PSI*, not a *PSM*, as the rest of the Lakatos monograph would lead one to expect. The uneasiness the reader feels with the over-all methodology of the monograph is due mainly to the equivocal role assigned to *HS*, at once emphasized and called upon as evidence, yet systematically "reconstructed" in the service of a prior theory of rationality.

One final illustration of the difficulties that philosophers can get into when they try to use *HS* in the service of their *PS* can be found in a recent monograph by Paul Feyerabend,[23] written partly by way of reaction against the Lakatos *PS* above.[24] Unlike Lakatos, Feyerabend claims to base his account of scientific method on the actual history of science as well as on "abstract" considerations. His monograph, he says, is "mainly historical. Abstract considerations are used only sparingly, and in the form of com-

corroboration rather than refutation (it is, indeed, not quite clear why it is called "falsificationism"). This is the view of "P_2," "proper-Popper." (These labels are mine; they are easier to remember than 'P_0,' 'P_1,' and 'P_2'!) Lakatos personifies his three Popper figures in a vivid way ("$Popper_2$ can easily get rid of $Popper_1$'s untenable falsificationist elimination rule"), and concludes that "the real Popper is a strange mixture of $Popper_1$ and $Popper_2$, and the only way to understand him is by cutting him into two" ("Falsification and the Methodology of Scientific Research Programmes," Appendix). Lakatos's method of "rational reconstruction" is obviously not confined to *HS*; it extends to the work of philosophers also. This use of lay figures with the names of real people seems a distracting and unnecessarily vulnerable way of writing philosophical analysis.

[22] "Falsification and the Methodology of Scientific Research Programmes," section 2b.

[23] "Problems of Empiricism, II," in *The Nature and Function of Scientific Theory*, ed. R. G. Colodny (Pittsburgh: University of Pittsburgh Press, 1970).

[24] Which he believes to be defective precisely because of the "tremendous abyss" (section 1) between it and "certain important episodes" in *HS*.

ments on the historical material. This material shows, I think, that there is something seriously amiss with the professional philosophy of science of today . . . [It] not only fails to adequately *describe* some of the most exciting episodes in the history of thought; it would also have given extremely bad advice to the participants."[25] This is explicit enough: he is about to elaborate a *PSI* based on the history of science, and is critical of other philosophers (notably the Popper group) for their failure to adopt the same approach.

For his historical material, he chooses the work of Galileo in cosmology. Upon this he bases a general thesis about the nature of science: "Progress [in science] is made by the gradual accumulation of views that are absurd or refuted [relative to the status quo], views which though undermined by reason and fact still support each other to such an extent that they finally supersede everything else."[26] Any attempt to see in this "progress" something "rational" (describable in some sort of methodological pattern) is thus doomed to failure: "My discussion of Galileo has, therefore, not the aim to arrive at the "correct method" [of science]. It has rather the aim to show that such a "correct method" does not *and cannot* exist. More especially, it has the limited aim to show that counterinduction [ignoring facts that do not fit] is very often a reasonable move."[27] Feyerabend does not explore the significant term 'reasonable' here; he does not ask what norms of "reasonableness" might have guided Galileo (assuming that he *did* ignore facts). Instead he is mainly concerned to reject the conventional empiricist and falsificationist accounts of science. It is simply *not* the case (he argues) that the transition from the Aristotelian-Ptolemaic cosmology to the Copernican-Galilean one "consisted in the replacement of a refuted theory by a more general conjecture which explained the refuting instances, made new predictions; and was corroborated by the observations carried out to test these new predictions."[28]

Instead of this account of Galileo's achievement, Feyerabend suggests a diametrically (and provocatively) different view: "While the pre-Copernican astronomy was in trouble (was confronted by a series of refuting instances) the Copernican theory (as found in Galileo's work) was in even greater trouble (was confronted by even more drastic refuting instances); but being in harmony with still further inadequate theories it gained

[25] Section 1.

[26] Section 1.

[27] Section 13.

[28] Section 7, end.

strength and was retained, the refutations being made ineffective by *ad hoc* hypotheses and clever techniques of persuasion."[29]

He concentrates on two features of Galileo's Copernican arguments, his use of the new telescopic data (especially those concerning relative planetary brightnesses), and his indirect method of getting the reader to admit the claim that an observer will not perceive a uniform motion which he himself shares. In the case of the first, Feyerabend argues that "the telescope produced spurious and contradictory phenomena and some of its results could be refuted by a simple look with the unaided eye. Only a new theory of telescopic vision could possibly bring order into the chaos . . . Such a theory was developed by Kepler."[30]

Feyerabend assumes that "evidence obtained in accordance with the older Aristotelian views" would have been "bound to clash with the new astronomy." Yet the natural philosopher had an obligation (why?) to "preserve the new astronomy," and this meant "that he must develop methods which permit him to retain his theories in the face of plain and unambiguous refuting facts,"[31] until the development of the lower-level "auxiliary sciences" would allow the apparent "facts" to be reinterpreted.[32] This leads him to the broad generalization that "a new period in the history of science commences with a backward movement that returns us to an earlier stage where theories have smaller empirical content."[33]

Galileo's kinematic arguments are treated even more severely by Feyerabend; in his view, they are "propagandistic machinations," concealing a basic "change in experience, an invention of a new kind of experience."[34] Like Koestler, he sees in Galileo's whole approach to the physics of motion a set of deliberate "psychological tricks" which "obscure the fact that the experience on which Galileo wants to base the Copernican view is nothing but the result of his own fertile imagination, that it has been invented."[35] The "essence of Galileo's trickery" lies in his use of something that purports to be the Platonic method of anamnesis in order to introduce a "new kind of experience manufactured almost out of thin air . . . [which] is then solidified by insinuating that the reader has been familiar with it all the time."[36] In particular, by focusing on certain special physical contexts (like the deck of a moving ship), he can argue that we have in fact already implicitly accepted the nonperceptible character of shared motion. By generalizing this to include the earth, Galileo thus "conceals" the fact that he has really redefined experience itself. The "absolutist" interpretation of our motion claims (that we can perceive what is *really* at rest and *really* in motion) nevertheless is "empirically entirely adequate." No difficulties arise with it (as long as we keep away from contexts like ships and other moving systems). Thus: " 'Experience,' that is, the totality of all facts from all domains described with the concepts which are appropriate in these domains, *this* experience cannot force us to carry out the change which Galileo wants to introduce. The motive for a change must come from a different source."[37] And the source is twofold—a "metaphysical urge for unity of understanding" and a prior bias in favor of Copernicanism, which Galileo has accepted in advance of the evidence and is not pre-

[29] Section 7, end.

[30] Section 5, end. It is not at all clear what service Kepler's theory would have rendered in support of the reliability of Galileo's data. In point of fact, it never does seem to have been called upon explicitly as an auxiliary warrant for the telescopic data the Copernican argument required.

[31] Section 8. He makes much of the famous passage where Galileo praises Copernicus for laying aside the apparently refuting "facts" concerning the variations in apparent planetary sizes, which were much smaller than they should have been according to his theory. But no instance is given of Galileo *himself* deliberately setting aside awkward astronomical "facts"; in his Copernican arguments (as he reminds the reader more than once), he seems to take inconsistency between fact and theory very seriously. To make the point that Galileo did not follow the classical inductive procedures of empiricism, it is not necessary to go to the other extreme and assert that he constantly ignored facts that counted against his views; it is quite enough to observe that the anamnesis he relies on so much was not a bringing of new experimental evidence but rather a reminder of familiar facts and a warning that it was more difficult than it seemed to describe them consistently and correctly.

[32] The claim that various "auxiliary sciences" of physiology and optics would have been needed (and were not available) in order to make Galileo's telescopic data sufficiently reliable to count as independent evidence is open to serious question. It is true that the first telescopes were quite imperfect, and that for a few years contradictory data were reported. It is also true that expectation plays a role in what is seen. But what is one to make of the fact that the astronomers of the Collegio Romano had verified all the observations Galileo reported in the *Sidereus Nuncius* (except his incorrect description of Saturn) as early as April 1611, i.e., within a year of their being made? And these men were as yet Aristotelian in their sympathies. The phases of Venus, the sunspots, the satellites of Jupiter, the variations in planetary diameter and brightness, all of these were observed by a variety of different astronomers within a few years of Galileo's announcement of them. The regular periods observed for the satellites and for the phases of Venus strongly indicated that they were no artifact of physiology or optics. There was surely no need of "auxiliary sciences," especially when all these telescopic data had been woven together by Galileo under a single consistent theory. These data were not, of course, "hard facts" independent of the support given them by incorporation in such wider theories. But to say this is not to say that there was no empirical or factual basis for their use by Galileo to decrease the plausibility of the Ptolemaic system almost to zero and to corroborate the Copernican view.

[33] Section 8.

[34] Section 14.

[35] Section 13.

[36] Section 16.

[37] Section 14.

pared, on any account, to give up.[38] Thus by letting "refuted theories support each other, he built a new world-view which was only loosely (if at all!) connected with the preceding cosmology (everyday experience included); he established fake connections with the perceptual elements of this cosmology which are only now being replaced by genuine theories (physiological optics; theory of continua), and whenever possible, he replaced old facts by a new type of experience which he simply *invented* for the purpose of supporting Copernicus."[39]

To evaluate this reconstruction of Galilean mechanics with the care it deserves would take us very far afield indeed.[40] And the intention of introducing it here was not to provide a critical analysis. Rather, it was to illustrate one quite characteristic manner of approach to the history of science on the part of philosophers. Still, something has to be said about Feyerabend's crucial "change-of-experience" mode of characterizing the Galilean move, in order to appreciate the relation between *HS* and *PS* in his world view. First, he has chosen his example well; it is quite difficult to find any other in the history of modern science where the transition from one theory to another could plausibly be described as a change in experience or as a shift in the meaning of the observation statements on which the two theories are based. It is worth noting this, because it suggests that one cannot validly derive a *general* view of the nature of scientific advance from this instance only or from it principally. The Ptolemaic-to-Copernican move was of a very special methodological kind (as the term 'revolution' is often loosely used to underline); before one could call upon it in support of a general theory of science, one would have to dissect with great care the epistemological and ontological tangles latent in it. The moral of this is, of course, that the philosopher who turns to *HS* for the main warrant for his views has to beware (as has anyone else who "uses" history) lest he be influenced in his choice of historical evidence by the very theory he is going to substantiate and which he has already implicitly adopted in advance. There is more than a danger of special pleading unless a real effort is made by the philosopher of science to review the wide diversity of types of theoretical change that the history of science has to offer.

[38] It is usually assumed that Galileo was a Copernican from his first days in Padua; in support of this his letter of 1597 to Kepler is quoted. Yet there seems to be good evidence for saying that he remained doubtful about the Copernican world view until the telescopic data of 1609–11 refuted the main alternative. Though authorities like Tannery and Hall have Galileo seeking evidence in physics and astronomy for a Copernicanism already intuitively seen to be correct, this is not convincingly borne out by the evidence one can piece together from his Paduan period (1592–1610). For a discussion of this issue, see the section "Copernicanism and the Origins of the *Discorsi*" in the Introduction to my *Galileo: Man of Science* (New York: Basic Books, 1967), pp. 20–24.

[39] Section 16.

[40] For the elements of a very different view, see my Introduction to *Galileo: Man of Science*, especially the section "Copernicanism and the Origins of the *Discorsi*"; "Empiricism and the Scientific Revolution"; and "A Turning-Point in Physics: Galileo's *Discorsi*," to appear in a forthcoming volume of the Pittsburgh series edited by R. G. Colodny.

More seriously, though, does even the Galileo case support Feyerabend's view about the nature of scientific change? It does not seem so. Apart altogether from the moral overtones of terms like 'trickery,' 'cheat,' 'conceal' (overtones for which no adequate logical and biographical justification is given), he has not made a convincing case for saying that Galileo's use of anamnesis constituted a disguised "change of experience." First, it was not the case that the Aristotelian assertion that the earth is immobile relied upon some special "experience" of the earth at rest; rather, it was a theoretical conclusion from an elaborate teleological argument involving the ideas of natural motion and natural place, and the entire Empedoclean physics of the four elements. Nor was it the case that Galileo was alleging in his support an "experience" of the earth's motion. Feyerabend equates "experience" with an "interpretation" of sense impressions, a "judgment" inferred from phenomena, and is willing to admit that there was no change in the "phenomena" when Galileo persuaded men to agree that the earth is moving. What has changed is the *interpretation* of the phenomena. But ought this be called a "revision of our experience"?

Feyerabend distinguishes between two "paradigms," involving two different "natural interpretations": the Aristotelian one, asserting that *all* motion is "operative" (produces perceptible effects), and the Galilean one, asserting that only *relative* motion is operative. And then he describes the transition to Galilean mechanics as a change in paradigm. But this will not do, for reasons that Galileo himself carefully brought out. A person in a uniformly moving system (like a boat) cannot say what the motions of the objects he perceives about him would seem like to an observer on an unseen distant shore. He can perceive only the *relative* motions of these objects; perceived motion is thus motion relative to the observer in such a case, and the further difficult question of the observer's own possible motion must then be asked. But how is it to be answered? Galileo argued that the Aristotelian had no consistent way of answering this in such a way that he could disprove the possibility that the earth itself might be a moving reference frame. If sense experience will not tell someone in the hold of a ship whether the ship is moving, how is it supposed to tell us whether the earth is moving?

The context to which attention is drawn is one already familiar to the Aristotelian. What Galileo argues is that since his opponent *already* interprets observations made in such a context in a "relativist" way, how can he consistently do otherwise in the case of observations made on the earth's surface? It is not the case that the Aristotelian maintains an "absolutist" paradigm for all contexts. If this had been the case, Galileo's anamnesis would not, in fact, have worked. The difficulty facing the Aristotelian is an inconsistency in his *own* "paradigm." He has to admit that he already makes use of different interpretations in different contexts, and he is unable to justify his taking the earth as a privileged system without introducing a speculative theory of motion, one which Galileo has little trouble in demolishing. There is nothing fictional or "manufactured" about the experience on which Galileo draws, then, in his analysis of how we already describe our experiences in moving carriages and the like.

It is true that Salviati is "leading" Simplicio, just as Socrates did the slave boy in the *Meno*, and that there is an element of rational reconstruction about the result, in the sense that Simplicio has been led to see that the assertion that the earth is really in rapid motion in an orbit around the sun, however counterintuitive it may seem at first sight, cannot be claimed to be false on the basis of immediate experience without getting into inconsistencies elsewhere in one's descriptions of perceived motion. Simplicio's ultimate agreement with this argument was not a matter of his being duped or of his unwittingly accepting an arbitrary or at best only partially justified reconstruction. Rather, it was a matter of his being brought to realize the implications and responsibilities of consistent usage inherent in even the simplest attempts to systematize everyday kinematic concepts. Though Simplicio may interpret his experience differently in consequence of the anamnesis arguments, the only alternative Salviati left open to him was to deny the possibility of any ultimate unity in our understanding of motion. But for Galileo (as for any other scientist then or now), this was not a real alternative.

Feyerabend's reconstruction of the Copernican-Galilean "revolution" does not, therefore, carry conviction. A fortiori, it cannot provide a basis for a general theory of scientific change. Though it serves a valuable purpose in directing our attention to the methodological complexity of Galileo's work and the impossibility of fitting it into conventional empiricist or falsificationist patterns, it fails for three reasons: (1) it exaggerates some details and ignores others; (2) it generalizes readily and without much analysis of possible contextual differences; (3) it is suffused with a moral passion that transforms history almost into melodrama. But this sounds very like what Feyerabend says of *Galileo's* method, and by implication of the method of science generally. This gives us the clue we need to understand what Feyerabend is doing. The method of inquiry he describes is not so much Galileo's as it is his own. It is demonstrably the method he follows in constructing the argument of the paper we have just analyzed. To accept the account of Galilean physics given there, and to see in it an acceptable paradigm of scientific inquiry, would be simultaneously to accept Feyerabend's own methods of analysis and reconstruction as "scientific" and satisfactory.

Despite appearances, therefore, Feyerabend's *PS* in this paper does not rest upon *HS*. He brings a prior notion of rational inquiry to bear upon the history of science with a view to finding there some support for his view. But the way in which he does this forces one to say that his *PS* is *not* grounded in history; whatever support it has, it must draw from elsewhere. It is, thus, a *PSE* not a *PSI*, and it is a *PSE* of a peculiarly risky sort in that by purporting to be a *PSI*, it is effectively exempted from exploring its real warrant.

A final example of a rather different genre is afforded by Bernard Lonergan's influential and difficult work *Insight*.[41] His aim is to provide a general theory of intelligence. In Part One of his book, he discusses some characteristic structures of scientific inquiry, like measurement and probability theory. It sounds as though he is building a general theory of insight on an analysis of insight in science, presumably because it is the area whose epistemology has been the most carefully explored. The author himself, indeed, often seems to assume that this is what he is doing. This would make of this part of his book a *PSI* on which a more general metaphysics could later be constructed. But on closer view, this is not in fact what is going on. The author is *not* attempting to describe actual scientific practice; he is deriving in a quite abstract way what the features of coordinate measurement or mechanical explanation *have* to be. At this point, it sounds like a *PSM* of a broadly Kantian sort. But this is not correct either. What Lonergan seems to be doing in his Part One is presenting a speculative analysis

[41] London: Longmans, Green, 1957.

of some common structures of scientific inquiry and then treating this analysis *itself* as an instance of *metaphysical* insight (into science, as it happens, but it could just as readily be art) in order to construct a general theory of insight and judgment. It would, therefore, be more correct not to use the label '*PS*' at all in this case.[42] This instance differs from the other two in that the "internal" element called upon by the philosopher as apparent warrant of his assertions is current scientific practice rather than *HS* itself. Yet it illustrates the same general point as did the others: that *PSE* is often presented as though it were a *PSI*. Both modes of doing *PS* are perfectly valid. The danger is, however, that such an approach may easily divert attention from the difficult and crucial issue of justifying what is being said.

Can the philosopher allow himself to be *entirely* governed by what happens (or has in the past happened) in scientific practice? Is there an analogy here between the philosopher formulating a theory to account for the procedures of science and a physicist formulating a theory to account for the behavior of gases? To press such an analogy, to suppose that everything a scientist does contributes positively to a theory of science is clearly wrong. Scientists (unlike gases) can make mistakes; there can be bad pieces of research. And scientists can gradually learn to do things better, so that later science could conceivably be more significant than earlier science. But is there any norm for what should count as a "good" or "bad" piece of research work? any norm, that is, prior to the construction of a *PSI*? If not, how is the practitioner of *PSI* to proceed? Can he leave aside those events in *HS* which don't fit in with his views, on the grounds that they were "bad" science, or at least untypical of the "best" science? Would there not be a danger of *petitio principii* in such a procedure? Would such a *PS* be genuinely internal?

This is a real difficulty for anyone who purports to be giving a *PSI*. Can a *PSI* be normative? Does not this implicitly convert it into a *PSE*? A *PSI* has no source of autonomous prescientific evidence which would allow it to judge the adequacy of a particular piece of scientific work. Nevertheless, a *PSI* can legitimately point out when such a piece of work departs from

[42] This reconstruction of the methodological status of part one of *Insight* is put forward tentatively; the work is a very complex one and has given rise to much controversy. For two different appraisals of its relationship to "orthodox" *PS*, see E. McKinnon, "Cognitional Analysis and the Philosophy of Science," in *Spirit as Inquiry*, ed. F. Crowe (New York: Herder and Herder, 1964), pp. 43–68, and E. McMullin, "*Insight* and the *Meno*," *ibid*., pp. 69–73.

the "normal," from the strategies that have proved in the past most "successful." Since it purports to be giving an account of what actually goes on in science, this is as far as it can go. It could not, for example, mount a critique of "normal" procedure itself without becoming a methodologically different sort of undertaking, one intended to define the ideal rather than explore the actual. One last reminder is in order, that in most cases a *PS* will not fall neatly into either category: it will draw from above as well as below. It will be governed by unstated metaphysical presuppositions, logical considerations of consistency, esthetic values, as well as by some knowledge of what has been going on in science these three centuries past. Our purpose in separating these considerations, and in classifying the types of *PS* built on only one of them to the relative exclusion of the others, was to focus attention on an important but often-overlooked ambiguity: what counts as evidence in *PS*, and in particular what role *HS* plays in it.

6. Philosophy of Science: Three Areas of Inquiry

In the preceding sections, we have been speaking of *PS* as though it were a single well-defined enterprise. This is far from being the case. *PS* comprises all those philosophic inquiries that take science as their starting point or as their object of concern. When discussing the distinction between *PSE* and *PSI* above, we assumed that the problems of *PS* are *epistemological* in nature, so that one could turn either to a more general theory of knowledge or to an inspection of the procedures actually followed in science in order to solve them. But two other sorts of problem have also got to be taken into account; they belong to the domains traditionally called ontology and philosophy of nature respectively. The abbreviations '*ES*' (epistemology of science), '*OS*' (ontology of science), and '*PN*' (philosophy of nature) will be convenient here. *ES* would at one time have been regarded as part of logic. *OS* constitutes a relatively new problematic, although there are some hints of this problematic in Plato's thought and in later medieval discussions of astronomy and optics. *PN* would originally not have been distinguished from "physics" (natural philosophy) itself. The development of Newtonian science profoundly affected all three of these. *ES* was greatly enlarged and strengthened as science itself became more and more sophisticated and self-conscious in its methods. *OS* became a crucial issue only where there was a sufficient body of scientific theory to make a question about its ontological import unavoidable. *PN* became a separate domain only when "philosophy" and "science" themselves began to separate in the post-Newtonian period. Metaphysics and physics had always been distinguished. But a distinction between the "philosophic" and "scientific" approaches to an issue is of very recent origin. *ES*, *OS*, and *PN* have come to be grouped together in recent decades under the broad title of "philosophy of science," a title which would have made no sense in Newton's day.[43]

ES is concerned with science as a way of knowing (explaining, proving, discovering, measuring, conceptualizing, etc.). It is a general methodology of empirical science; it is not concerned with particular scientific theories or even with particular domains (biology, chemistry, etc.) except insofar as the difference of domain brings with it a difference of methodology.[44] Most of the published work in what is called "philosophy of science" today would fall into this category. Topics like the nature of explanation in science, the logic of confirmation or discovery, account for more than half of all the essays in current anthologies of *PS* in the United States (in the Pittsburgh, Minnesota, Delaware, and Boston series, for example).[45] Although in principle *ES* is a general theory of scientific method, it is ordinarily elaborated in the context of the most developed sciences, notably mechanics, from which in the past the ideal of scientific method has most often been elaborated. Of late, however, philosophers have begun to realize the negative effects of this concentration of *ES* upon what is in fact a quite untypical part of science. "Explanation" in mechanics means something quite different from explanation in a structural science like biology or chemistry or geology. With the change in *PS* already noted from external (*PSE*) to internal (*PSI*) modes of warrant, *ES* has broadened very

[43] In some countries (U.S.S.R., Germany), and in some philosophic traditions (especially those of Aristotle, Aquinas, Kant, and Hegel), this grouping is less common. A strong distinction would be drawn between "theory of science" ("critique of the sciences," etc.) on the one hand, comprising *ES* and *OS*, and *Naturphilosophie* (*PN*) on the other. In the International Congresses of Philosophy, these constitute two different sections, though the assignment of papers to one or the other becomes ever more arbitrary. In the Vienna Congress of 1968, whether one submitted a paper to the "Theory of Science" division or to the "Philosophy of Nature" division seemed to depend largely on one's country of origin or on one's own philosophical standpoint. See my Introduction to the *Naturphilosophie* section of the Congress *Proceedings* (vol. 4, pp. 295–305): "Is There a Philosophy of Nature?" The main reason this distinction is not emphasized by English and American philosophers is that they are skeptical of the possibility of an autonomous philosophy of nature.

[44] Quantum theory has, for instance, suggested to some philosophers that a special multivalued logic is required where noncommuting operators stand for physical parameters.

[45] See E. McMullin, "Recent Work in the Philosophy of Science," *New Scholasticism*, 40 (1966), 478–518.

much in scope and has grown in sophistication. Because science represents in some sense an ideal of human knowing, *ES* (whether of the *PSE* or *PSI* variety) is highly relevant to the more general issues of epistemology and metaphysics. In some recent instances, indeed, the position adopted in *ES* has determined the entire shape of philosophy, as with logical positivism.

A second area of *PS*, closely related to the first, is the ontology of science (*OS*), the exploration of the ontological relevance of the claims made by empirical science. *OS* reduces, in essence, to a single question: to what extent do the postulational structures of science reveal a "real" structure, whether of the world or of the human mind? Various philosophers have argued that although science makes our experience intelligible by formulating correlations that enable predictions to be made, we cannot infer from this that scientific theories have any ontological import. They may be no more than arbitrary fictions, convenient instruments of prediction. *OS* is concerned, therefore, not with the general structures of scientific knowing, nor with the specific physical structures that occur in nature, but with the question of how these are related to one another. What, in brief, does science tell us about the world? This question has been a crucial one for philosophers ever since the time of Hume, who was the first to defend a phenomenalist ontology which would deny an intelligible structure to nature, and therefore by implication refuse any sort of realist view of science. There has been a significant difference between the *OS* of scientists and that of philosophers: the former did not have to contend with Hume. For the most part, they have maintained a realist *OS* (with the exception of some physicists working in the area of mechanics, an atypical part of science, as we have already noted). The resolution of this ontological issue is quite crucial for contemporary metaphysics, especially for a metaphysics (like that of process philosophy) which derives part of its warrant from the results of scientific theory.

Empiricist philosophers have paid relatively little attention to *OS*, as a glance at standard United States handbooks of *PS* will show.[46] For an empiricist of Humean sympathies, *OS* is not even a meaningful issue. But apart from this, *ES* and *PN* are much more congenial from the point of

[46] One notable exception is E. Nagel's *The Structure of Science* (New York: Harcourt, Brace and World, 1961). For detailed work in *OS*, see, for example, J. J. C. Smart, *Philosophy and Scientific Realism* (London: Routledge and Kegan Paul, 1963); W. Sellars, "The Language of Theory," in his *Science, Perception and Reality* (New York: Humanities, 1963); G. Maxwell, "The Ontological Status of Theoretical Entities," *Minnesota Studies in the Philosophy of Science*, vol. III (Minneapolis: University of Minnesota Press, 1962), pp. 3–27.

view of "research," since there is an abundance of material to work on; new problems arise as new scientific theories are formulated. The philosopher of science who busies himself with *PSI* (whether *ES* or *PN*) can easily leave *OS* out of account altogether; by comparison with other parts of *PS*, the problem it poses tends to seem an intractable one. Yet it is *OS* that poses the most specifically "philosophical" issue of any part of *PS*; until one has faced this issue, all other findings in *PS* are suspended in the air.

The third area of *PS* depends quite sensitively for its characterization upon the position one adopts in *OS*. Many scientific theories appear to have far-reaching implications for traditional philosophic problems concerning the nature of mind, the relations of space and time, the nature of causality, and so forth. If one defends a realistic or quasi-realistic theory of science, then the implications of relativity theory, of the theory of evolution, of cybernetics, and the like, have to be taken seriously by any philosopher who wishes to understand the most general traits of the physical world. One can describe these implications as a "philosophy of nature," meaning that the scientific theories themselves allow us to formulate a properly "philosophic" cosmology. On the other hand, if a nonrealist *OS* be defended, what passes as *PN* in the other view is likely to be regarded as no more than a speculative extension of science, a series of conceptual clarifications, "philosophical" only in a very loose sense. Since a realistic view of science will be defended later in this paper, the title '*PN*' will be used; it will be assumed that the philosopher is not debarred from making statements on his own account about the physical world or about specific structures like time or mind.

PN is obviously very close in methodology to science itself. They seem, in fact, to form a continuum. The conventional modern distinction between philosophy and science, which has come to seem so basic, is not readily applicable here. How is one to specify a demarcation criterion that would mark "philosophy" off from "science" in such works of *PN* as Adolf Grünbaum's *Philosophic Problems of Space and Time*? Much will depend on whether one believes an autonomous *PN* to be possible. Is it possible to construct a *PN* prior to the deliverances of science, based on the "common core of experience" or on the structures of ordinary language, or on an analysis of the general structures of possibility of any knowledge of a physical world? A wide variety of philosophers (neo-Aristotelians, Marxist

Leninists, phenomenologists, Kantians, Hegelians, etc.) maintain that such a *PN* can be developed.

They disagree, however, on how to interpret its relationship with science. Is it altogether autonomous, and thus unaffected by the growth of scientific knowledge? If so, can it perhaps even serve as a norm to judge the adequacy of the categories and methods of the scientist? This strong claim for *PN* can be found in some writers of the Hegelian, Marxist, and Husserlian traditions. A weaker claim would give the prescientific *PN* a limited autonomy only, allowing for the possibility that it might have to be modified in the light of advances in science. In other words, part of the warrant for an adequate *PN* would be the sciences of nature. A philosophy of nature would thus have to balance evidence of two rather different sorts, evidence from some prescientific source (e.g., common experience or the categorial "cuts" of ordinary language) as well as from science. Each of these would in effect be taken seriously as a partial warrant for philosophic assertions about nature; the testimony of one could, however, modify that of the other. The alternative to these two views of *PN* is one which would make it wholly derivative from science, i.e., would deny any source of evidence for a *PN* other than contemporary scientific theory and practice.

The status given a *PN* thus serves as an indicator of the distinction between "philosophy" and "science" a particular philosopher maintains, i.e., of the ways in which he chooses to define these two very vague and dangerously honorific terms. (1) He may deny the existence of a *PN* entirely, in which case all knowledge of nature, however speculative, is by definition "scientific," and "philosophy" is entirely confined to second-order questions about language or method. (2) He may allow a *PN*, but insist that it be entirely derivative from science. In this case, the distinction between "philosophy" and "science" is in terms of speculative character or generality or the like. (3) He may allow a partial warrant for *PN* prior to the constructive activity of science. This gives a very complex notion of "philosophy," since quite different types of evidence can be relevant to it, and it can make first-order assertions about the world, of higher generality than those of science but presumably in a continuum with these. (4) Finally, he may hold out for a completely autonomous *PN* prior to science, in which case he can draw a very sharp distinction between "philosophy" and "science" on the basis of the type of evidence on which each rests. Since he does not in this case admit science as a source of properly "philosophical" knowledge of nature, he will not have a *PS* concerned with nature; *PS* for him will cover at most only *ES* and *OS*. This rather summary and abstract taxonomy may suffice to bring out the wide variety of approaches that may be covered by our label "philosophy of nature."[47] It would take us much too far afield to evaluate these approaches, and in particular to investigate whether or not there *is* a genuinely autonomous "philosophical" mode of approach to nature different from that followed by the scientist. But perhaps enough has been said to suggest that this part of *PS* is more complex and controversial in character, methodologically speaking, than are *ES* and *OS*. The existence of a *PN* (if it be admitted) suggests that philosophy and science have somehow got to complement one another—or else compete with one another—in the construction of a total world picture.

The distinction just drawn between three different approaches to *PN* can also be expressed in terms of the "external-internal" division above, even though it was originally elaborated in the context of *ES* rather than *PN*. If the warrant for *PN* is independent of science, we have a *PNE*; if it rests upon the theories of science, it is a *PNI*. If there is some external source of evidence for a *PN* but the procedures and theories of science are also taken into account, we have a *PN* of mixed warrant (*PNM*).[48] *OS* can likewise be governed either by external or by internal considerations (or by a combination of the two). One could, for instance, develop a positivist *OSE* on the basis of a Humean phenomenalism. A "pure" *OSI* is less likely; one would not normally wish to base an ontology on an analysis of science exclusively. Metaphysical and epistemological considerations of a more general sort would presumably have to be taken into account in deciding what the ontological implications of scientific theory are. In the final sections of this essay, I will argue that an adequate *OS* has to take the developmental aspects of science very seriously. Some features of the history of theoretical models will be brought forward to support a realistic *OS*. Broader philosophical considerations for or against realism are not

[47] I have developed this schema in more concrete detail against the background of the major exponents of *PN*, past as well as present, in "Philosophies of Nature." I have argued there that this question of the type of warrant on which a *PN* is supposed to rest makes an illuminating basis of distinction between contemporary approaches to nature. The tension between the different possible approaches to knowledge of nature a philosopher may take up has been of very great importance in the Kantian and more recently the Marxist-Leninist schools. For the latter, see my review-article on David Joravsky's book *Soviet Marxism and Natural Science, 1917–1932* (London: Routledge and Kegan Paul, 1961), in *Natural Law Forum*, 8 (1963), 149–159.

[48] These are the *PN1*, *PN2*, and *PNM*, of my article "Philosophies of Nature." *PNM* seems to me the most defensible sort of *PN*; I have argued elsewhere that it plays an implicit but important role in the heuristics of science (section 9 of the Epilogue to *The Concept of Matter in Modern Thought*).

raised. It could be argued that the only way of accounting for the history of the Bohr model is by assigning a realistic import to the physical structures it postulates. One might in this way construct an independent *OSI* based on an analysis of *HS*, though in the long run one would probably want to broaden it to an *OSM* by introducing arguments of a more general sort in favor of realism in epistemology and ontology.

What is the relevance of history of science to the three domains of *PS* described above? We have just implied that an adequate *OS* has to be based at least partly on information drawn from *HS*: that the universe is such that the sort of hypothetical structural models used by scientists function in inquiry in the way they do is perhaps the most significant clue to ontology that we possess. On the other hand, *HS* has very little direct relevance to *PN*. One will not be concerned in the fashioning of a *PN* with the details of how particular theories come to be formulated and progressively modified; rather what counts is the best theory available now. *HS* is, as we have seen, highly relevant to the epistemology of science, if taken as an "internal" study (*ESI*).

Another way of conveying the difference between the ways in which the three main divisions of *PS* approach the study of science is to say, using the distinction between S_1 and S_2 elaborated in section 1 of this paper, that *ES* must be concerned with S_2 since the epistemologist has to take into account the widest possible range of evidence on how scientists proceed. *PN* on the other hand need take only S_1 into consideration; only the confirmed results of science have a bearing on the philosophy of nature. *OS* is concerned much more with S_2 than with S_1. The finished propositional product of the scientist is enigmatic in its ontological implications, as we shall see. Realist and positivist alike can interpret it to their taste. It is only when the temporal dimension of science, the developmental aspect of S_2, is taken into account that a decision can be reached on the central issue of *OS*.

7. Philosophy and Psychology

What is the relationship between the "philosophic" mode of investigating science and other systematic modes of understanding human activity such as psychology (including variants like psychoanalysis) and sociology?[49] The distinctions we have already drawn may help us to bring some

[49] Other social sciences, like economics and politics, are much less relevant because their "laws" and explanatory hypotheses concern facets of human behavior or of social structure that are remote from the doing of science. Though economic or political situa-

clarity to this question, one which is quite crucial to the currently disputed question of a "logic of discovery" in science.[50] The science we have in mind here is, of course, S_2. Psychology is not relevant to the understanding of S_1, but it may well tell us something of the conditions under which S_2 is furthered. Only *ES* is involved in this correlation with the work of the social sciences; psychology is clearly irrelevant to *OS* and *PN*. ES_2 is, it would appear, the possible point of contact between *PS* and other modes of "understanding science," understanding it, that is, specifically as a human activity. Understanding is usually thought to involve two "moments": the discovery of regular patterns and the explanation of why these regularities recur in the way they do. Our question, then, reduces to this: what are the principal ways of understanding the patterns that occur in the complex of activities we call scientific research?

The answer we shall hazard is that only two need be considered: the philosophical and the psychological. We can trace regular conceptual or propositional connections, whether these be strictly *logical* (governed by a specifiable formal rule) or analogical. We can examine the scientist's activity with a view to describing and interrelating propositions implicit in it, the beliefs that guide him, the data he has obtained, the hypotheses he advances, and so forth. We can trace the gradual modification of a concept (like the concept of ether in Newton's thought), where it is possible to give plausible conceptual grounds ("reasons") for the modification's having occurred the way it did. The "pattern" here is a relation between ideas or can somehow be associated with such a relation. The techniques are those of conceptual analysis. The *ideal* here would be that of a complete logical reduction, the discovery of a fully formal system which would simulate the theory or procedure under investigation. But this is rarely possible,

tions and motives can obviously influence scientific research, this influence would be best understood as a rule in psychological or sociological terms. If, for example, one were to ask why certain lines of research moved more rapidly than others in the United States over the last two decades, one would have to take into account the availability of federal grants for some types of research and not for others. Note that this is a *psychological* explanation (the efficacy of the economic motive) rather than an economic one. Scientific activity does not lend itself to what we would ordinarily regard as economic or political patterning. To understand the economic situation that made one type of research more desirable to the federal government than another may require quite a bit of economic or political analysis. But this analysis is likely to be of only marginal interest to someone who is trying to discover invariants in scientific activity *as such*.

[50] R. Blackwell in chapter 3 of his recent *Discovery in the Physical Sciences* (Notre Dame, Ind.: University of Notre Dame Press, 1969) distinguishes between four possible ways of patterning scientific discovery: logical, psychological, historical, and epistemological. In general, though, distinctions of this sort have not been analyzed by philosophers of science in any detail.

since scientists do not follow a strict (i.e., fully specifiable formal-deductive) logic of this sort in the more significant parts of their work. Indeed, to the extent that they do follow fully formal rules, their reasoning is unlikely to be significant, since it is only unfolding something already conceptually and propositionally given.[51]

A *psychological* pattern is, broadly speaking, some regularity in human behavior, thought to be attributable to the specific personality structure (intellectual abilities, emotional makeup, character, etc.) of the individuals exhibiting it.[52] Since this includes propositional "behavior" (thinking, writing, proving), logical patterns are not going to be easily distinguished from psychological ones.[53] Since people generally obey the *modus ponens* rule in their reasoning, is it not a psychological law as well as a logical one? This is a matter of definition, of course, but logical rules of inference are usually *not* regarded as properly "psychological" laws, even though they

[51] This important point I have tried to make in some detail in "Freedom and Creativity in Science," in *Freedom and Man*, ed. J. C. Murray (New York: Kenedy, 1965), pp. 105–130.

[52] It is not necessary to distinguish explicitly between sociology and psychology in this context. Sociology seeks information about the behavior of persons as members of specific groups, and about the interactions of groups considered as units. But insofar as one wishes to *explain* this behavior or these interactions, one must ultimately return to psychology. As a type of *information*, sociology is distinct from psychology. But as modes of *explanation*, they tend to become one. E.g., much has been written about the influence of religion on the growth of science in the seventeenth century. Correlations have been sought between creativity in science and Unitarianism (or Anglicanism or free thought or Christianity). But if such are found, one will still have to ask why a Unitarian *should* have been a better scientist, and the answer will have to be either *conceptual* (lying in Unitarian belief) or *psychological* (the type of personality structure commonly found among Unitarians). The former approach is the commoner among those philosophers and historians who have discussed this problem (see, for example, R. S. Westfall, *Science and Religion in Seventeenth Century England*, New Haven, Conn.: Yale University Press, 1958). In a rather disreputable recent book, *The Scientific Intellectual* (New York: Basic Books, 1963), Lewis Feuer argued that science is favored by a hedonistic and antiauthoritarian personality; he links this sort of personality with certain religious groups and with various social movements. Members of religious groups that are nonhedonistic and authoritarian are, he claims, unlikely to contribute to science. The difficulty about his argument is that when in his impressionistic tour of the history of science he meets with counterexamples (Newton being a rather obvious one!), he goes on to claim that a *second* source of scientific creativity lies in neurotic conflict of a Freudian sort. This renders his original thesis impregnable, since notable scientists who do not qualify as hedonists or libertarians are automatically labeled by him as neurotics, even in the absence of independent evidence to this effect! This is the sort of writing that brings anguish to philosophers and historians of science alike. But a start has been made with sociology-psychology of science of a more responsible sort, such as one finds in the work of Derek Price or Bernard Barber.

[53] The whole question of how logic and psychology are to be situated relative to one another is so dismayingly vast, so amorphous in its ramifications, that these remarks have to be considered as no more than loose generalities.

are clearly patterns of a quite basic sort in the operation of the human psyche. The reason for not including them in the scope of psychology is that the "dynamism" of these regularities, their ultimate ground, is thought to lie in the propositions rather than in the psychological structures of the thinker.[54] Even if these latter structures were quite different, the assumption is that logical laws would still govern the thinking of the individual, as an ideal to be striven after, even if perhaps not always followed.

To the extent that the scientist's procedures are *not* completely bound by logical rule, however, it would seem that psychological considerations may have to be taken into account. The formulation of a hypothesis, for instance, is not a deductive process. It may be guided by analogies but by definition it goes "beyond the evidence," i.e., beyond what could be arrived at on the basis of formal logic alone. It is relevant, therefore, to inquire whether the pattern of *discovery*, say in science, can be at least partially accounted for in terms of psychological laws and theories.[55] To "account for" it here means to situate it as one human ability among others, to show if possible "how it works," an ambitious mechanistic metaphor but implying nothing much more than that the characteristic stages in discovery should be categorized in some general way. A good example of this sort of effort is Koestler's massive work, *The Act of Creation*,[56] which explores creativity in art, humor, and science, and suggests as an "explanation" of what occurs an ability on man's part to juxtapose hitherto unrelated matrices of thought. Whether this is an "explanation" or only an insightful *description* of what happens can be debated, but it is enough for us to note that insofar as it would "explain," it would do so by pointing to

[54] But many would disagree. Kant reduced logical (and many other sorts of) law to "psychological" structures, in a sense of 'psychological' admittedly very different from that of contemporary psychology. In an informal paper at the International Congress of Philosophy in Mexico City in 1963, Carnap argued that the grounds for accepting logical laws as "laws" (i.e., for making use of them in our thinking) are purely pragmatic and experimental—we find that they work. They are no more "psychological," then, than are physical laws. They do not express how our minds operate, but rather how the world is. In the lively debate which followed, Max Black urged that they are analytic features of the language we use, requiring no pragmatic or other sort of specific justification (Carnap's own earlier position). This sort of debate has been going on since Greek times, when Aristotle argued that logic and metaphysics presented different facets of the same basic structure, that of Being. It is not necessary for our discussion of the nature of *PS* that we should enter into this question in detail.

[55] One of the difficulties about answering this question is that there are so many different sorts of "psychology," ranging from depth psychology to behaviorist studies of animal behavior. Obviously, the implications of each of these for *ES* are likely to differ.

[56] New York: Macmillan, 1964.

a general human ability (possessed by some to a higher degree than others); it would then go on to break down this complex and puzzling ability into simpler and better understood sub-abilities. Contrast this with a *logical* explanation of the steps of a mental process, where rules would have to be given to justify each step of the process, or at least to estimate its inductive weight. To "explain" a process psychologically does not of itself justify the term of the process; at best, it only describes how we got there.

Michael Polanyi would broaden the scope of this sort of analysis to include not only discovery but also confirmation.[57] Or more exactly, he would assert that the conventionally sharp distinction drawn between these two rests upon some shaky empiricist assumptions. He then proceeds to give a philosophical-psychological account of scientific knowledge which sees it as an instance of the human ability to recognize pattern, to interpret tacit clues without necessarily being able to break down the process into well-defined compartments, "evidence" and "hypothesis," linked by explicit formal rules and analogies. There is (he claims) a "tacit structure" of knowing involved; to focus attention on one element in it may mean that the ability to see the whole as making sense may be lost. This brief summary does not do justice to a complex and admittedly controversial position;[58] it is introduced here only to illustrate the thesis that once one moves away from those limited parts of scientific activity which can be completely dissected in formal-logical terms, one has to take account of psychological factors side by side with the more properly logical-conceptual ones.

A complete epistemology of science (ES_2) cannot, therefore, leave psychological considerations out of account. Admittedly, such considerations are not relevant if we are merely interested in the validity of the scientific claims made, the extent to which they "explain" the data (ES_1). And this is the perspective in which the problem is most often discussed. But the wider perspective is a valid one, and the question of the methodology appropriate to it is deserving of more attention than it has so far received.

[57] *Personal Knowledge* (London: Routledge and Kegan Paul, 1958); *The Tacit Dimension* (New York: Doubleday, 1966).

[58] The main objection to the facet of it described here is that the methodological strands of scientific confirmation can, in fact, be separated off to a much greater extent than Polanyi allows. The steps by which an empirical "law" is built up, and the gradual H-D confirmation of a theory, are not just unstructurable personal acts of "seeing." The analogy with visual pattern recognition on which Polanyi frequently relies can easily be pressed too far.

Whether at this stage psychology can in *fact* offer much help on issues such as the nature of creative discovery is another matter. Such questions do not readily lend themselves to investigation in the prevailing behaviorist terms, and it is noteworthy that extensive empirical attempts in recent decades to correlate creativity with other more easily identifiable traits have been unsuccessful.[59] The "explanation" offered by psychology is, besides, of a very modest sort. It is obviously never going to reduce the creative act to specifiable rule; it can only search for some appropriate general categories in which to analyze it. Men have the ability to "see" a particular hypothesis as the best way of explaining the "facts." The distinction—one might almost say the tension—between the psychological and philosophical modes of approach to this ability soon becomes evident. Are we to rest content with describing it simply as an ability? Or ought we in each case where it operates attempt to specify the logical reasons why the hypothesis *is* the best one, or why it is confirmed by this piece of evidence?

These are the two principal ways of understanding recurrent patterns in the activities of the scientist. But how are these patterns to be *discovered*? This is where the historian comes in, because all these patterns are of themselves "historical," in the sense that they recur in time and can be documented by the ordinary methods of the historian. Does this not suggest that history ought to be added to logic and psychology as a third mode of recovering pattern in science, of "understanding science"? It is important to see why this is not the case. *HS* is *not* of itself a mode of understanding science, in the ordinary sense of discovering and explaining regularities in the practice of science. Its goal is to establish the singular, not the universal (as does epistemology or psychology). Insofar as it provides "understanding," it is an understanding of the past singular in its complexity and contingency, a different sense, therefore, of 'understanding.' To achieve it, the historian may make use of a variety of sciences: psychology, linguistics, sociology, as well as philosophy. But this does not mean that his own effort falls into the same methodological category as theirs. There is ultimately a quite fundamental division here. The historian is concerned with what happened just because it *did* happen. He may call upon universals of all sorts in his effort to establish what happened or why it happened. But his goal is not the assertion of a universal, a pattern, or the interlinking of such patterns. This is the task of the philosopher, the

[59] As a glance at such standard collections as *Scientific Creativity: Its Recognition and Development*, ed. C. W. Taylor and F. Barron (New York: Wiley, 1963), will quickly show.

sociologist, the economist, whose use of the materials of history does not commit them to the reconstruction of any *specific* set of historical events.

History is closely interwoven with these other fields. They are built up inductively from things that happen. But they are not concerned with the particularities of occurrences, only with their exemplification of a certain set of universals. The philosopher of science will discuss the nature of measurement, for instance, without adverting to any specific historic instance of it. The sociologist will assert a correlation between drug addiction and broken homes without giving historical details of the broken homes he investigated in making his generalization. Yet the philosopher and the sociologist have to begin from the activities of real people; they may not invent their material, they have to find it. This can easily be overlooked in the case of philosophy, because it is for the most part at such a high level of generality that specific reference to concrete instances, instances requiring the skill of the historian to establish or unravel them, is rarely found. When a philosopher speaks of the nature of discovery (say) in science, he will often suppose that what he is saying is so intuitively evident to anyone who has even a rudimentary acquaintanceship with science that reliance on specific instances, calling such instances in *evidence* so to speak, is simply unnecessary.

8. Logic and History

We have already seen that a philosopher who wishes to find an "external" warrant for a *PS* is likely to look either to a metaphysics (*PSM*) or to the properties of formal systems (*PSL*), whereas someone who relies on "internal" evidence (*PSI*) is likely to look for his evidence either to contemporary science or to episodes in *HS*. Of these approaches, the two that most strongly contrast with one another are the logical and the historical. It may be worth exploring these contrasts in more detail. It defines the ends of a spectrum of possible ways of relating *HS* to *PS*.

The logician and the historian approach the problem of relating two elements in a piece of scientific research in quite different ways. The logician seeks to discover a purely logical structure relating them, a structure of transparent intelligibility in its own right. This structure can be disentangled and its properties studied; it can be used to justify the move from one element to the other. What the historian seeks to establish is the *fact* that the elements occurred in a certain sequence, whether or not any formal structure can be discerned in their relationship. He may be able to "explain" the historical sequence by showing how it exemplifies some logical or psychological pattern. But his first concern is not with explanation but with a reconstruction of the past, no matter how opaque it seems.

In *PSL* in its most "external" form, science becomes the *occasion* for the logician to investigate certain formal structures that might not otherwise have come to his attention. His aim is to construct a theory of inference, a theory of confirmation, a semantics of scientific terms, or the like, in such a way that these can stand in their own right as formal systems. The important point is that *PSL*, so construed, does not rest upon an appeal to what is actually going on in science or to *HS*. When an exponent of *PSL* puts forward an inductive logic, he need not be claiming that this is what actually governs scientists in their evaluation of hypotheses. He may even be entirely indifferent to any reference from case studies in *HS*. It need not weaken his case to say that Newton did not follow the logical plan suggested. What the logician is saying is that in an abstractly described piece of scientific research, the logical relationships between the elements are of the following kind—whether or not the relationships he specifies describe any historical sequence or were grasped by the scientists involved in the research. In this external form, *PSL* is not an empirical study; it is basically of the same character as mathematics or any other formal discipline. It is only in a broad sense that it qualifies as "philosophy." It could be argued that it ought *not* be so described, any more than formal logic would nowadays be described as "philosophy." Nevertheless, this would be a mistake since *PSL* does illuminate the structures of empirical science, and does take its origin in them.[60]

[60] Two recent instances of *PSL* would be R. Carnap's *Philosophical Foundations of Physics* (New York: Basic Books, 1966), and Kyburg's *Philosophy of Science: A Formal Approach*. Carnap's book deals with probability theory, the logic of measurement, the logic of causal modalities, analyticity, correspondence rules. Kyburg organizes his book "around the concept of a formal system," and explicitly limits himself to *ES*. He leaves aside *OS* ("what does the fact that science exists at all tell us about the world?"), not because he thinks these unimportant, but because a formalist *ES* is, in his view, an indispensable starting point for any adequate discussion of them. His first chapters are characteristically headed "The Concept of a Formal System," "Quantities," "Scientific Terms," "Axioms," "Probability and Error," "Induction and Experiment." He makes extensive use of the predicate calculus and of the logic of relations, and indeed notes in his Preface that "the philosophy of science can be understood without knowing physics (though perhaps not without really understanding some science), but it cannot be understood without knowing some logic, an essential ingredient of every science." In the entire text (on the testimony of the careful index at the end), not a single scientist is mentioned, nor are there more than a few references to specific scientific works. Thus the weight of the book in no way rests on a reporting of scientific practice; an item from *HS* would not be relevant to any of the major points that Kyburg is making.

There is an obvious analogy here with the "rational mechanics" of Newton's successors. Newton developed a complex physical theory, which he himself on occasion liked to regard as a quasi-formal mathematical system, thus leaving aside all questions about the operational meaning of the concepts employed and of the empirical adequacy of the system as a means of understanding specific concrete problems, leaving aside in other words what would be regarded as the properly "physical" issues. This "rational mechanics," as it was called, was developed at the hands of Euler, Lagrange, Laplace, Hamilton, and many others. Its evolution was quite independent of any new empirical information, or any modifications in the empirical bearing of the concepts used. It was guided by purely mathematical principles; its criteria were those of pure mathematics, although its original impetus came from physics. What its proponents sought was a more elegant formal exposition, employing more economical and better defined concepts; this in turn would allow the hidden inconsistencies and vaguenesses of the earlier system to be eliminated. The construction of the well-known "Lagrangian" and "Hamiltonian" functions to help in formulating the state description of a Newtonian mechanical system was an instance of such a development. It was prompted not by any empirical inadequacy of the system but by a desire for conceptual improvement. In technique, the exponents of rational mechanics were mathematicians; its history has been similar to that of a branch of mathematics, even though its starting point was different. It is not, however, applied mathematics, a point that Clifford Truesdell has emphasized.[61] It does not simply apply a given mathematics to the formulation of a physical theory or to the solution of physical problems. Rather, new mathematical concepts have to be developed or old ones modified in response to the needs of the physicist, or as a further elaboration of a formal system first created by the physicist. Thus rational mechanics is not quite reducible either to physics or to mathematics. The analogy with *PSL*, with logic substituted for mathematics, is a fairly exact one; *PSL* likewise is not quite reducible either to philosophy or to logic.

If the logician, instead of considering general epistemological issues inherent in any part of science, turns to specific scientific theories with a view to formalizing them, he will have to take *HS* somewhat more seriously. If he aims to formalize Newtonian mechanics, he can scarcely do this without some reference to the documents. Yet this reference may serve

[61] *Essays in the History of Mechanics* (Heidelberg: Springer, 1968).

only as a starting point; he may settle for some convenient textbook account of Newtonian mechanics and focus on the logical issues involved in it, without pausing to ask whether the system he is analyzing is really that of Newton. If an objection is raised on this score, the logician is likely to be unmoved; his creative energies are concentrated on formal problems of structure, not on problems of historical interpretation. Thus his analysis of "Newtonian" mechanics is likely to identify this mechanics with a broad class of systems, independent of any particular historical text. Yet he may after all rightly claim that his analysis illuminates (at least to *some* degree) Newton's own work, its conceptual implications and its weaknesses. And he may well exhibit considerable historical sophistication in deciding how to formalize messy physical concepts like *mass* or *force*.

The value of such an approach to *PS* is that it uncovers logical structures, a thorough grasp of which is indispensable to the full understanding of science or of a particular scientific theory. Its limitations as *PS* have already been alluded to more than once, chief among them a remoteness from the actual workings of science, and a danger of escapism (from the point of view of *PS* not that of logic, of course) because of the allure of the free construct. The logical-empiricist school has naturally tended to *PSL* because the characteristically Humean epistemology of the empiricist made it difficult for him to take *HS* seriously as an independent source of philosophical insight. The variety of challenges to empiricism among philosophers of science in the past decade has brought with it a corresponding skepticism about the adequacy of logical reconstructionism as a program for *PS*. It has also opened the way for a much more thorough utilization of *HS* on the part of philosophers.

At the beginning of this section, it was noted that the logician and the historian represent the opposite ends of the intellectual spectrum in their approaches to science. Between them lie sociologists, economists, philosophers, theologians, etc. The historian tries to re-create the singular in all its individuality; he emphasizes context and distrusts generalization. The stuff of history is events, not concepts or propositions. For something to be part of history it is sufficient that it should simply have occurred. The historian does not begin with facts; he tries to establish them, and having established them, to understand them. The relationships on which this historical understanding rests are causal ones, broadly speaking. Being causal means that they are generalizable; they can in principle be concep-

tualized. If they are conceptualized, do they become logical? Can history take on the transparency of logic?

The answer (pace Hegel) is no, for two reasons principally. The conceptual-causal relations of the historian are not reducible to the formal rules of inference of the logician. These latter are not empirical; they are wholly independent of context. The causal patterns cited by the historian, and the concepts in terms of which these patterns are expressed, are learned empirically and are highly contextual in their application. The logician must go beyond the specific predicates of the scientist, the historian, the philosopher, to disengage (if he can) purely formal relations of inference holding between propositions employing unspecified predicate variables. But even if one were to allow a broader sense of 'logical' in which any conceptual-causal relation would be "logical," there is a further reason why history will not reduce to logic. It is impossible to re-create conceptually even the simplest historical event. There is no way to give an exhaustive listing of all the potentially relevant causal influences. Nor is it the case that all of these will have left a recoverable record. And the concepts in terms of which the event is described are at best only approximative and provisional. The work of the historian is by its nature a tentative one, then, always open to revision. Not only is the historical singular infinite in its complexity, but the evidence which would allow this complexity to be conceptually reconstructed is itself transient and soon irrecoverable. The boundaries of the historian's task are set by *matter*, in the Aristotelian sense of that term; whereas the logician (much more than the philosopher) can abstract from matter entirely.

To the extent, then, that a structure is logical, it has ceased to be historical. A deductive inference rests in no way for its validity upon experience or history. It is valid not because it has been followed innumerable times with success, but because it has the total transparency that makes reference to history unnecessary in its support. To the extent, on the other hand, that a particular discovery is nonformalizable, dependent for example upon a radical conceptual realignment, history will have to be called upon to warrant its reliability and significance. The tasks of the historian and of the logician are almost exactly complementary, therefore, and both are important if a full understanding of the genesis of that complex phenomenon known as "science" is to be achieved.

9. Can One Do History and Philosophy of Science Together?

This essay has made extensive use of dichotomies as a tool of methodological analysis. But it has also stressed that works of scholarship rarely fall into a single neat methodological category. One distinction, however, that might seem a reasonably sharp one is that between *HS* and *PS*. Ordinarily, it is easy to tell which of these genres a particular piece of research belongs to. Can they be validly blended in a single work? The answer would seem to be that they can. *PSI*, as we have seen, often involves a careful reading of the history of science as a *warrant* for the philosophical claims made. Such work accomplishes both a historical and a philosophical goal. The writer tries to illuminate the historical instance with all the relevant philosophical analysis he can produce so that, despite its singularity, he may understand it as best he can. He also uses the documented historical instance to make a further philosophical point; it serves not merely as illustration but as evidence for this point. This genre of "history and philosophy of science" (*HPS*) is a complex, even a risky, one, as we have already seen when discussing the work of Lakatos and Feyerabend above. There are obvious dangers involved in combining two methodologies so diverse (not to mention the dangers of infuriating two professional groups whose reflexes are so different!). A good piece of *HPS* will not blur the distinction between the historical and the philosophical points it is making; by making them at the same time, or at least in the same piece, there is no intention of claiming them to be ultimately identical. We have already seen that philosophy and historiography are at bottom irreducible to one another, no matter how closely they may be interlocked in practice. It is important to grasp in as precise a way as possible what the relation is between the historical and the philosophical motifs in such writing. The historical motif is prior and in a sense basic, for on the establishing of the analysis as *history* depends its *warrant* as philosophy. It is ultimately because something happened in a certain way that a point in *PS* can be made to rest partially upon it.

If someone, in order to make a general point about meaning variance, asserts for instance that Galileo's colleagues obtained telescopic results that differed from Galileo's, it is essential that he be correct in the historical claim if it is to serve as warrant and not merely as illustration. An illustration could be replaced by some other apposite instance if it proved historically inaccurate. But if the philosophical claim in any way rests upon the case histories cited, it is weakened if any of these are shown to be unreliable. The *HPS* writer may, of course, choose simply to cite his history from someone else and not attempt to bring any fresh support for the

claim that it happened the way it is supposed to have. But even so, by making use of it for philosophical purposes, he will almost certainly have illuminated it, situated it, helped the reader to understand it better. And this, as we have already seen, is one of the two main functions of the historian. By making a series of points of general philosophical relevance in the context of Newton's dynamics, Dudley Shapere (to quote one recent example) has also illuminated Newton's own historical achievement.[62]

There is one particular category of philosophical problem where the *HPS* approach is seen at its best, and where the *PSL* methods of the logician prove inadequate. This is the investigation of the *developmental* aspects of science (S_2). If discovery in science were guided by logical laws, one could write a history of science as it *had* to occur. But, of course, science is not like this; central to it is human creativity, and there are the innumerable contingencies of influence and noninfluence. One can extract the partial logical structures of validation which are implicit in scientific research. But to see how change actually occurs in science, what factors are most often responsible for it, one has to have recourse to the historical record.

This is the approach taken by Mary Hesse, for example, in her *Forces and Fields*.[63] She traces some basic conceptual structures that have recurred in the analysis of continuous and discontinuous motion in mechanics. In particular, she emphasizes the complex philosophical problems that underlay many of the modifications of concept that occurred as mechanics attained a greater and greater precision. The resultant is good history of science; it also serves as the ground for a variety of epistemological and ontological assertions, notably the assertion of a generally realistic view of scientific constructs.

Thomas Kuhn's influential work, *The Structure of Scientific Revolutions*,[64] puts *HS* to even more striking philosophical use. He distinguishes

[62] "Philosophical Significance of Newton's Science," *Texas Quarterly*, 10 (Fall 1967), 201–215. Adolf Grünbaum argues this point cogently, and illustrates it from the historiography of relativity theory, in "The Bearing of Philosophy on History of Science," *Science*, 143 (1964), 1406–12.

[63] London: Nelson, 1961. One of the earliest and still one of the finest achievements of this genre was Pierre Duhem's ten-volume work, *Le Système du Monde* (Paris: Hermann, 1913–59). This is a history of mechanics, with special reference to celestial mechanics, but it also makes use of historical analyses to argue (in contrast to Mary Hesse) a generally positivist view of the nature of scientific theory. The prototype of *HPS* was the pioneering work of William Whewell, *The Philosophy of the Inductive Sciences, Founded upon Their History* (London, 1840).

[64] Chicago: University of Chicago Press, 1962.

between fundamental changes of "paradigm" in science ("revolutions" in his terminology), and theoretical developments that leave the basic "paradigm" unchanged. His main thesis is that changes of paradigm cannot be justified on empirical or even rational grounds, though *post factum* an effort will always be made to provide such rationalization. This is a bold claim; it denies the possibility of *any* sort of *PSL* applicable to significant advances in science. Indeed, it has seemed to many to call into question the entire set of formalist assumptions on which the logical analysis of science is based. Kuhn's *HPS* is thus the antithesis of *PSL*, which may help to explain the warmth it has generated. For us, the important thing about it is that *only* the history of science can serve as evidence in its support. It is a philosophical statement about the nature of S_2 and about the transformational characteristics of S_1. It could not be derived from a general theory of knowledge, nor could it rest upon a formal logic. Only a sensitive analysis of selected periods in *HS*, an analysis which leaves aside the preconceptions of later methodology, will suffice to tell whether it is correct or not.

Many criticisms have been leveled against the meaning-variance thesis as it variously appears in the work of Kuhn, Hanson, Feyerabend, Toulmin, and others. The main point of criticism is the tendency of these writers to exaggerate the nonformal elements in scientific change; like any other crusaders against an ancient dogma, they tend to underplay the evidence that counts in their opponents' favor. But their work has shown that the modifications of concept which lie at the root of scientific change cannot be accounted for along the deductivist lines traditionally favored by philosophers of science. If theories are regarded as quasi-axiomatic schemas, one can think of confirmation as a matter of checking inferences in the systematic way a *PSL* would demand. But how is one to justify the choice of the concepts in which both the theory and the evidence alleged in its support are expressed? On what grounds and by what means are these concepts altered? Variations of meaning of the sort that constantly occur in science cannot be accounted for in terms of a purely formal logic.

Yet it is somehow within these variations that the clue to scientific advance lies. It is not enough to lump all of them together under the label 'discovery,' treat it as irrational or irrelevant, and mark it off sharply from "validation," understood in an idealized *post factum* way that leaves aside the question of what actually *did* persuade people to adopt particular theories. Rather, one must begin with a careful analysis of the crucial moments of meaning change in *HS*, and try to see if patterns of any sort can be distinguished. If not, one may have to be content to point simply to an ability on the part of the well-trained scientist to discern a "good" theory without being able to specify what precisely makes it "good."

Kuhn and his colleagues are talking about the epistemology of science in the broader sense of science defined in section 1 (ES_2). The importance of *HS* to the resolution of their problem comes from the fact that on the one hand the knowing activity of the scientist is a temporal process, and on the other it is not usually subject to complete logical reduction (unless it is the derivation of a prediction from an already-given theory). If the knowledge processes of the scientist are part of what we wish to understand, we simply have to treat *HS* as our major source of evidence. But it is when we ask *ontological* questions about the import of the postulated theoretical structures of science (whether in general or in regard to specific structures) that the temporal dimension of science (and consequently the use of *HS* as a basic research tool) has to be taken most seriously. Only the history of science, it is clear, can serve to resolve these questions. What philosophers for a long time failed to see was that ontological questions necessarily involve the developmental aspects of science.[65] They cannot be answered (or more correctly, they will be wrongly answered) if one is content with examining a temporal "slice" of scientific work. What discloses the nature of the relation between the model and the modeled is not a logical structure of here-and-now predictions and verifications, but rather a dynamic pattern visible in the way models guide inquiry.

10. History as the Clue to Ontology

The realist-instrumentalist debate about the status of theoretical entities cannot be resolved (or, more exactly, is likely to be resolved in favor of the instrumentalist, on the good Occamist grounds that he is claiming less and achieving just as much) unless one takes into account the developmental aspect of science. And not just in an abstract way, but as a specific testimony to how theoretical entities have *in fact* guided research. The debated term 'real' can best be defined in this context by referring it to the object which gradually discloses itself through the progressive theoretical refinements offered by the scientist. The claim of a realist ontology of science is that the only way of explaining why the models of science function

[65] This point I first tried to make in a brief article in the 1955 *Proceedings* of the American Catholic Philosophical Association, "Realism and Modern Cosmology," 29, 137–150.

so successfully in the overcoming of anomalies is that they approximate in some way the structure of the object. The resources that a "good" model seems to possess to meet the unexpected challenge from the data of the twilight world that lies over against the scientist can only come from its being a "fit" for that world. The long-lasting fertility of the good theory cannot be accounted for by simply alleging the endless creativity of the human mind in the face of anomaly. The model *guides*, and it guides in a way that a summary of the original "data" could never do, no matter how "creatively" made, unless there was a resonance between model and object.

But all of this needs to be documented in detail. In the limited space remaining to us it seems best to summarize a historical instance I have discussed more fully elsewhere.[66] In 1913, Bohr suggested his famous "planetary" model of the H-atom. He was guided by the results of Rutherford's scattering experiments which indicated that the atomic mass is concentrated in a very small nucleus, of positive charge proportional to the atomic number of the element. Spectroscopic data further suggested the idea of discrete energy levels within the atom; it was plausible to connect these with the negative charges required to keep the atom as a whole neutral. But a system composed of negative charges revolving around a positively charged center ought to radiate continuously, according to classical electrodynamics. The new quantum hypothesis gave the needed clue; by quantizing energy transfer, certain orbits were equivalently "canonized," thus fixing the permitted energy states of the given system.

The model was thus suggested by the Rutherford data, the Planck quantum hypothesis, and the energy-level principle already developed in spectroscopy. There was an analogy between it and the Copernican model of the solar system, though the differences between the two models (Coulomb instead of gravitational force, discrete orbits instead of continuously variable ones) are more evident than the similarities. In its original form, it immediately accounted for the Ritz "principle of combination" (1908) which summarized in one general formula the known data (Balmer, Lyman, and Paschen series) for the spectral frequencies of the radiation emitted by hydrogen:

$$\nu = R\left(\frac{1}{n^2} - \frac{1}{s^2}\right)$$

[66] "What Do Physical Models Tell Us?" *Logic, Methodology and Philosophy of Sciences*, vol. 3, ed. B. van Rootselaar and J. Staal (Amsterdam: North-Holland, 1968), pp. 385–396.

where $n = 1,2,3$ and s is an integer greater than n. Bohr was even able to derive a formula for R that made it equal to $(2\pi^2me^4)/(h^3)$, thus allowing it to be calculated in terms of known quantities and giving a result that agreed (within three significant figures) with the value of R known from spectroscopic data.

The simple idea of a very light negatively charged particle revolving around a relatively heavy positively charged one thus accounted for all the basic facts about hydrogen with quite surprising ease and accuracy. But now the model *itself* suggested three further modifications, modifications that would be required if one were to suppose this to be a real structure, obeying the laws of physics as far as we know them. These were *not* required by the original data (the Ritz series and the Rutherford results); they were not in any sense contained in them. Nor were they directly implied by the original model, taken simply as a correlation. Only if the model is taken seriously as an approximation to a consistent physical reality is there any reason to suppose that theoretical modifications of the following sort would yield verifiable results.

In the original model, three simplifying physical assumptions had been made: first, the nucleus was assumed to be infinitely heavy (i.e., to be unaffected by the mass of the electron); second, the electron was restricted to circular orbits; third, the energy of the electron was calculated nonrelativistically. For *physical* consistency, the consequences of each of these assumptions had to be explored separately. When the finite mass of the nucleus was allowed for, R had to be multiplied by a factor of $(1 + m/M)$. This immediately explained why the lines for ionized helium (which is structurally similar to hydrogen) were not identical with the Ritz series: m/M for the two atoms is not the same. Calculation of the series for ionized helium immediately gave the correct results (the Pickering series already discovered in 1897). Second, elliptical orbits give the same energy levels as circles do, except in an electrical field, when splitting ought to occur. Since there is no physical reason why the special case of circularity should be favored, one would therefore expect this splitting. And there it was: the Stark effect, known since 1913, a fine structure of each H line, produced when the emitted atom is subjected to an intense electric field. The amount of the splitting and the polarizations produced were exactly predicted. Finally, a relativistic correction of the calculation of energy levels showed that *all* the H lines, even apart from the presence of fields, ought to show a very fine splitting. This had actually been observed, as early as 1887, for the main Balmer line. It was soon verified for all the others, and in exactly the amounts predicted.

How is this striking series of successes to be accounted for? What relationship must be postulated between the model and the world the physicist is trying to understand? Since the model is the *only* possible mode of access we have to the world, there is no way of answering this question directly. But if we try to account for the career of the model (and the philosopher is forced to account for it somehow), there seems to be no satisfactory alternative to saying that the explanatory resources of the model are due to its having revealed, however imperfectly and incompletely, an "ontological" structure, i.e., a structure intrinsic to the world over against the observer, an anchor point in a network of causal relations stretching outward in the world. This is, of course, highly metaphysical and vague, as any discussion of ontology is forced to be because of the obvious limitations of language and proof in this domain. But if someone finds such a realism intolerably naive or hopelessly vague, he is still faced with the question: from where does the fertility of the model come, from the mind of the physicist, from the purely logical resources of the original construct—or from the object modeled and partially understood?

One further development of the Bohr model, rather different in logical type from the three already chronicled, is even more significant. In 1896, Zeeman had noticed a splitting of the spectral lines emitted by hydrogen in a magnetic field. The splitting was a very complex one, sometimes doublet, sometimes triplet, sometimes a baffling multiplet. It seemed that the Bohr model ought to explain it; after all, it had explained the apparently analogous effect of an electrical field. But all attempts to find some overlooked idealization or approximation, of the kind that had explained the Stark effect, failed. For fifteen years, much energy was expended on this problem. Then a number of people began to ask themselves: what if the orbital electron were to act as a tiny magnet? There was nothing in the original model to suggest this, but it was not inconsistent with the model. Since the electron is electrically charged, the easiest way to provide it with a magnetic field is to suppose it to be spinning. This is what Goudsmit and Uhlenbeck proposed (1926). Their postulate of electron spin allowed them to calculate the normal and anomalous Zeeman effects, not only for hydrogen but for other atoms as well. Here the theoretical modification was an explicit attempt to account for a set of unexplained data. This was done by adding a *new* complication to the original model, but one physically consistent with it. To give the electron a "spin" was to specify a parameter left undetermined in the first model. It was to explore one possible causal line in an only partially determined network. The frequent success of such efforts is what one would expect *if* a realist epistemology is the correct account of what it is that grounds scientific knowledge, makes it consistent and extensible outwards to an apparently unlimited degree.

In this section, we have departed from the neutrality of the earlier taxonomic enterprise to argue for a specific (and not especially popular) philosophical position. But our main point even here still remains (in the context of this paper) a taxonomic one. Whether or not one accepts a qualified realist view of physical models, one thing at least is clear. The only sort of evidence likely to be decisive—or even relevant—in this matter is that of the history of specific models. There is nothing in the logic of a model, considered as a purely formal structure, that would help one to an answer to the ontological question. The behavior of a model in time, the fact that it went this way instead of that way, is the best clue we have to its "real nature." In this respect the philosopher is not unlike the physicist himself who when investigating a fundamental particle follows its career in all sorts of different physical situations. In this way, he builds up a picture of what the capacities of his particle are. Likewise, the philosopher operating at a second level of inquiry chronicles the conceptual "behavior" of the double helix of DNA or of some other long-lived and productive model; only from its history can he learn how seriously he should take it as a clue to "real" structure. The model, when all is said and done, is *not* a physical particle or a Platonic entity; it is the creation of the physicist. Yet because its history does not seem to lie altogether within the grasp of its inventor, we are inclined to say that this history ought to be carefully looked into. The deep sources of history are what, after all, we mean by "reality."