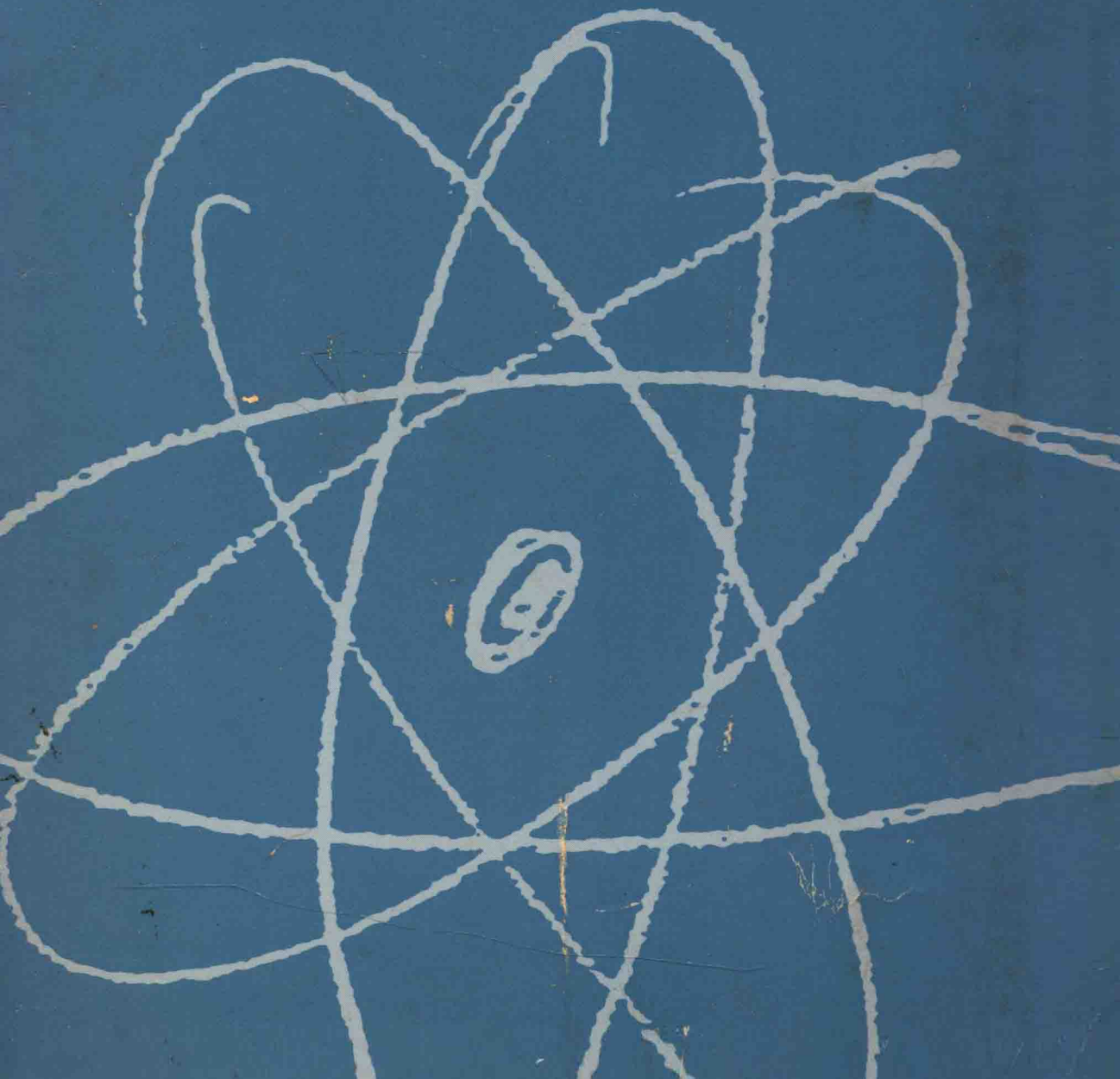
JANET A. KOURANY

Scientific Knowledge

Basic Issues in the Philosophy of Science



SCIENTIFIC KNOWLEDGE

BASIC ISSUES IN THE PHILOSOPHY OF SCIENCE

Janet A. Kourany

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PREFACE

Logical Positivism was a philosophical movement that began in Vienna in the 1920s and guided philosophy of science for at least thirty years. Since its demise in the 1960s, philosophers of science have not succeeded in developing a unified program of research to take its place. Indeed, philosophers of science continue to disagree not only about the nature of science but also about the aims and methods of philosophy of science itself.

The result of this unsettled state of the field is that very few textbooks in the philosophy of science have been available in recent years. The present anthology is an attempt to remedy this situation. Its aim is to introduce readers to the most basic issues in the philosophy of science without trying to minimize the prevailing controversy. Thus, Part 1 of the book is devoted to an overview of issues and alternative approaches to them that are current in philosophy of science today, whereas Parts 2 through 5 explore the most basic of these issues through essays representing a variety of positions and approaches. All the essays in this book, products of some of the most distinguished scholars in the field, have been chosen for their clarity and philosophical significance: They presuppose no special scientific or philosophical (for example, formal logic) background on the part of the reader. Introductions to each section provide a framework and the necessary background information for the essays.

Many people offered helpful suggestions and advice at various stages of the project. In particular, I would like to thank Robert Audi, Michael Bradie, Myles Brand, Nancy Cartwright, Maurice Finocchiaro, Ronald Giere, Mary Hesse, Noretta Koertge, Isaac Levi, Andrew Lugg, David Malament, Thomas Nickles, Michael Ruse, Husain Sarkar, Abner Shimony, Paul Teller, and Mary Williams. At least as helpful were my colleagues at Notre Dame James Cushing, Michael Detlefsen, Edward Manier, Vaughn McKim, Philip Quinn, Kenneth Sayre, Larry Simon, and especially Ernan McMullin; reviewers Michael Boylan, Georgetown University, Frank F. Fair, Sam Houston State University, Jarrett Leplin, University of North Carolina, Greensboro, Ronald Munson, University of Missouri, St. Louis, and especially Robert Causey, University of Texas; and Wadsworth special projects reviewer Jonas Weisel. Wadsworth philosophy editor Kenneth King, meanwhile, showed remarkable patience and good cheer through all the major and minor crises that arose during the project. My husband and fellow philosopher James Sterba, however, deserves the most thanks—for all the time, energy, and support he contributed in so many ways to the project. Our daughter, Sonya, also contributed to the project in her own special ways.

PHILOSOPHY OF SCIENCE: AN OVERVIEW

1

Are the sciences moving toward a unified account of the world, or are the pictures of reality they provide becoming ever more disparate? Do scientists have any reason to believe that current scientific theories are true when all the scientific theories of the past have turned out to be false? Is there anything that especially distinguishes current theories from past ones? How can scientists test a scientific theory that is about entities and processes no one can observe? These are some questions that philosophers have raised about science—questions, in fact, that we shall deal with in this book.

Why are philosophers so interested in science? On the simplest level, such interest reflects the traditional concern of philosophy with the nature of reality and the foundations and limits of human knowledge. But the answer goes deeper than this, and it affects more than just philosophy. The knowledge science provides is immensely impressive, and this knowledge has had a profound impact on our lives. Indeed, the noted historian of science Herbert Butterfield has said, of the Scientific Revolution that was instrumental in bringing this knowledge into existence:

> It outshines everything since the rise of Christianity and reduces the Renaissance and Reformation to the rank of mere episodes. . . . It changed the character of men's habitual mental operations even in the conduct of the nonmaterial sciences, while transforming the whole diagram of the physical universe and the very texture of human life itself.

Small wonder that science has inspired a deep interest among philosophers.

But science has inspired an equally deep interest among the representatives of other disciplines as well. Philosophers have wanted to explore the general characteristics of science that most directly relate to its function as a knowledge-producing activity—the nature and kinds of its explanations, the nature of its validation procedures, its patterns of development, the truth-status of its theories, and the like. Historians have wanted to know exactly how the concepts, methods, and goals of science have reached their

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present state of development, what particular factors brought about crucial changes in these at various times and places, and what particular social and economic forces promoted or inhibited these changes. Psychologists have wanted to analyze what types of individuals have engaged in this enterprise, what relationship their personality characteristics and motivation have had to the styles of their research, and, in general, what psychological processes have characterized their research. And sociologists have wanted to understand how far and in what ways such individuals have been influenced by the social and cultural contexts in which they work, to what extent a society's presuppositions have molded the findings of scientific research.

These various approaches to science—the philosophical, the historical, the psychological, and the sociological—are not independent of one another. Thus, for example, alternative philosophical views regarding the nature of theory-testing have shaped alternative sociological views regarding the influence of social factors on scientists' acceptance or rejection of theories. And, in turn, completed sociological research in this area will test and ultimately refine those philosophical views. Similarly, historical data have suggested philosophical views regarding general patterns of scientific development, which might, in turn, help in the construction of more adequate historical narratives. Historical data might also be used to test psychological or sociological hypotheses regarding science: for example, the detailed diaries of experimental investigations, hypotheses, speculations, plans, and incidental observations left by the nineteenth-century English physicist Michael Faraday have been used to test psychological hypotheses regarding scientific inference. Successful psychological or sociological hypotheses might, in turn, disclose the relevance of factors not previously noted when gathering historical data or constructing historical narratives, thereby yielding more successful historical research.

Doubtless a completely adequate picture of science will only emerge with the integration of all these different approaches to science. Unfortu-

nately, such a picture is still a long way off. Indeed, we now lack even the separate approaches themselves in an adequate state of development. What's more, deep controversies exist in the philosophy of science, the history of science, the sociology of science, and the psychology of science—and none more so than in the philosophy of science. The controversies relate not only to particular views on particular questions but also to the methods that should be used in answering those questions and even, on occasion, to the relative importance of the questions themselves. In "Alternative Approaches to the Philosophy of Science," Ernan McMullin surveys the controversy that exists in the philosophy of science, outlining the different approaches currently being pursued in several major areas. His discussion provides a helpful backdrop to the readings that follow in two ways. Read now, before the topics in the rest of the book are covered, it will form an introduction to these topics and their interrelations. Reread later, after the topics are covered and you are well on your way to formulating your own positions on these topics. McMullin's discussion will make explicit some of the subtle but important differences of approach in the different readings. It will then also help to make explicit the approach you, yourself, have adopted in the philosophy of science and the reasons you might formulate for that approach. By then you will be invincible indeed! May you have great success in your venture.

Alternative Approaches to the Philosophy of Science

Ernan McMullin

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, 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. 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 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.

HISTORY OF SCIENCE

And this, of course, is of special concern to the historian. Thus, he will have to take at least *some*

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account of S_2. But there are very different ways of going about writing history of science. As historiography,[1] its first responsibility is to establish what the facts were: who said what, and what he 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. . . .

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."[2] 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" 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

[1] 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.

[2] 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.

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.[3] 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.[4] 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.[5] 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.[6] A *PS* constructed by a scientist is

[3] 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."

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

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

[6] Examples are not hard to find. One recalls the "pointer-reading" account of scientific method on which Eddington built his

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 consequence, 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* and *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*

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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.

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.

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-empirical 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? . . .

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 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.

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.[7]

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.[8] 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).[9] 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 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 for-

[7] 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.

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

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