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Contents

Preface to the Third Edition

The main revision for the third edition is the addition of a section on the natural and the social sciences. This complements the first part, science vs. nonscience, and resonates with issues about explanation, confirmation, science and values, and the role of theory. Part 2 focuses on the way in which these issues generate debates about the nature of the social sciences, and comparisons and contrasts with the natural sciences.

The new readings in part 2 provide an integrated set of papers which address each other, either explicitly (Taylor vs. Kuhn) or implicitly (Rosenberg vs. Machlup). They extend the issues of the other parts into debates about the social and behavioral sciences. These readings also anticipate and expand upon the papers in part 6 (Science and Values), and also throw additional light on Kuhn's views. The readings in the newly revised section on Science and Values (part 6) now include an essay on feminism and science (Giere), which discusses feminism and Positivism, Popper, Kuhn, realism and antirealism. Even the topic of part 1, science vs. nonscience, is discussed in the context of these new essays. Finally, the Hollinger essay, "From Weber to Habermas," is included in the newly revised part 6, to fill a gap in the readings.

We believe that the new part 2 and the revised and expanded part 6 adequately cover material in the old part 6, Science and Culture. We have therefore eliminated this section, except for the essay by Hollinger, and revised the section on Science and Values accordingly. The new material is also more current, since it deals with feminism, postmodernism, and (in the expanded editorial introduction to part 6) the so-called science wars and recent versions of the sociology of science (mainly in the form known as Science and Technology Studies [STS]). These are all topics that are of great interest to the general reading public, as well as to university professors and students.

This book, in its revised and expanded third edition, can be used in standard one-semester courses in the philosophy of science, two-semester

Introduction

What Is Philosophy of Science?

Most readers of this volume probably have some familiarity with science—or with one or more of the sciences. But the following question may come to mind: Just what is philosophy of science? How does it differ from science? How is it related to other areas of philosophy? We shall here attempt to provide answers to these and related questions.[1]

I. What Philosophy of Science Is Not

Let us begin with a discussion of what philosophy of science is *not*.

(1) Philosophy of science is not the history of science. The history of science is a valuable pursuit for both scientists and nonscientists. But it must not be confused with the philosophy of science. This is not to deny that the two disciplines may often be interrelated. Indeed, some have held that certain problems within the philosophy of science cannot be adequately dealt with apart from the context of the history of science. Nevertheless, it is generally held that we must distinguish between the two.

(2) Philosophy of science is not metaphysical cosmology or "philosophy of nature." The latter attempts to provide cosmological or ethical speculations about the origin, nature, and purpose of the universe, or generalizations about the universe as a whole. As examples we may cite the views of Hegel and Marx, that the universe is dialectical in character; or the view of Whitehead, that it is organismic. Such cosmologies are often imaginative, metaphorical, and anthropomorphic constructions. They frequently involve interpreted extrapolations from science. Again, certain problems within the philosophy of science may aid the construction of or involve a consideration of such cosmological theories. But here, too, there is wide agreement that they must be distinguished.

(3) Philosophy of science is not the psychology or sociology of science.

study of science as an activity, as one social phenomenon among many. Some of the topics that fall within such an inquiry are: scientists' motives for doing what they do; the behavior and activity of scientists; how (in fact) they make discoveries; what the impact of such discoveries is on society; and the sorts of governmental structures under which science has flourished. Again, certain problems in the philosophy of science may on occasion be related to such issues. But once more, it is reasonable to hold that these inquiries must be distinguished.

For the purposes of our study, the philosophy of science will not primarily mean or apply to any of the above. We will not try to comprehend the history of science. We will not present any grand cosmological speculations. We will not try to understand the scientific enterprise in terms of human or social needs. However, with regard to the latter, it is desirable to make a distinction. It is one thing to present a psychological or sociological account of science. This we will not do. It is another thing to examine philosophically the relationship of science and culture and generally of science and values. The last part of this volume will be devoted to these issues.

II. What Philosophy of Science Is

Let us attempt now to see what the philosophy of science *is*. By one widely held conception, philosophy of science is the attempt to understand the meaning, method, and logical structure of science by means of a logical and methodological analysis of the aims, methods, criteria, concepts, laws, and theories of science. Let us accept this as a preliminary characterization.

In order to illustrate or apply this characterization, let us focus on the matter of the concepts of science.

(1) There are numerous concepts that are used in many sciences but not investigated by any particular science. For example, scientists often use such concepts as: causality, law, theory, and explanation. Several questions arise: What is meant by saying that one event is the cause of another? That is, what is the correct analysis of the concept of cause? What is a law of nature? How is it related to other laws? What is the nature of a scientific theory? How are laws related to theories? What are description and explanation in science? How is explanation related to prediction? To answer such questions is to engage in logical and methodological analysis. Such an analysis is what philosophy of science, in part, is (according to this conception).

(2) There are many concepts used in the sciences that differ from the ones mentioned above. Scientists often speak of ordinary things—such as beakers, scales, pointers, tables. Let us call these observables. But they also often speak of unobservables: electrons, ions, genes, psi-functions, and so on. Several questions then arise: How are these entities (if they are entities) related to things in the everyday world? What does a word such as "positron" mean in terms of things we can see, hear, and touch? What is the logical justification for introducing these words which (purport to) refer to unobservable entities? To answer such questions by means of logical and methodological analysis constitutes another part or aspect of what philosophy of science is (according to the conception we are considering).

Now, with regard to the kinds of concepts mentioned in (2), one might ask: Why analyze these concepts? Don't scientists know how to use them? Yes, they certainly know how to use terms such as "electron," "friction coefficient," and so on. And often they pretty much agree about whether statements employing such expressions are true or false. But a philosopher, on the other hand, might be puzzled by such terms. Why? Well no one has ever directly seen a certain subatomic particle, or a frictionless body, or an ideal gas. Now we generally agree that we see physical objects and some of their properties—spatial relations, and so on. The philosopher of science asks (among other things) whether it is possible that a term such as "positron" can be "defined" so that all the terms occurring in the definition (except logical terms, such as "not," "and," "all") refer to physical objects and their properties. He attempts to reduce or trace such "theoretical constructs" to a lower level in the realm of the observable. Why? Because unless this is done, the doors all open to arbitrarily postulating entities such as gremlins, vital forces, and whatnot.

As we can see, throughout such conceptual investigations as those mentioned above, the standpoint adopted by the philosopher of science is often a commonsense standpoint. Thus certain questions which may be asked by other divisions of philosophy (such as epistemology) are not asked here, for example, whether a table really exists. If one wants to say that this means that philosophy of science has certain limitations, then we must agree. But not much follows from admitting this, for those other questions can always be raised later when we turn to other kinds of philosophical problems. Hence for the philosophy of science, we do not need to raise them. We may use the standpoint of common sense.

III. Some Main Topics in Philosophy of Science

The characterization of philosophy of science we have given in the preceding section does not adequately cover all of the kinds of issues and problems generally recognized as falling within the scope of philosophy of science. Hence it is perhaps best to resist trying to find a single formula or "definition" of philosophy of science and to turn to a different task.

Let us now briefly consider some of the main specific topics and questions with which philosophy of science is concerned. (In this volume, we will be able to focus on only some of these issues.)

(1) The formal sciences: logic and mathematics. Logic and math are often referred to as sciences. In what sense, if any, are they sciences? How do we know logical and mathematical truths? What, if anything, are they true of? What is the relation of mathematics to empirical science?

(2) Scientific description. What constitutes an adequate scientific description? What is the "logic" of concept formation which enters into such description?

(3) Scientific explanation. What is meant by saying that science explains? What is a scientific explanation? Are there other kinds of explanations? If so, how are they related to those of science?

(4) Prediction. We say that science predicts. What makes this possible? What is the relation of prediction to explanation? What is the relation of testing to both?

(5) Causality and law. We sometimes hear it said that science explains by means of laws. What are scientific laws? How do they serve to explain? Further, we sometimes speak of explaining laws. How can that be? Many laws are known as causal laws. What does that mean? Are there noncausal laws? If so, what are they?

(6) Theories, models, and scientific systems. We also hear it said that science explains by means of theories. What are theories? How are they related to laws? How do they function in explanation? What is meant by a "model" in science? What role do models play in science?

(7) Determinism. Discussions of lawfulness lead to the question of determinism. What is meant by determinism in science? Is the deterministic thesis (if it is a thesis) true? Or what reason, if any, do we have for thinking it to be true?

(8) Philosophical problems of physical science. The physical sciences have, in recent years, provided a number of philosophical problems. For example, some have held that relativity theory introduces a subjective component into science. Is this true? Others have said that quantum physics denies or refutes determinism. Is this true or false?

(9) Philosophical problems of biology and psychology. First, are these sciences genuinely distinct? If so, why? If not, why not? Further, are these sciences ultimately reducible to physics, or perhaps to physics and chemistry? This gets us into the old "vitalism/mechanism" controversy.

(10) The social sciences. There are some who deny that the social sciences are genuine sciences. Why? Are they right or wrong? Is there any fundamental difference between the natural sciences and the social sciences?

(11) History. Is history a science? We often speak of historical laws. Are there really any such laws? Or are there only general trends? Or neither?

(12) Reduction and the unity of science. We have already briefly referred to this issue. The question here is whether it is possible to reduce one science to another and whether all of the sciences are ultimately reducible to a single science or a combination of fundamental sciences (such as physics and chemistry).

(13) Extensions of science. Sometimes scientists turn into metaphysicians. They make "radical" statements about the universe—e.g., about the ultimate heat-death, or that it is imbued with moral progress. Is there any validity in these claims?

(14) Science and values. Does science have anything to say with regard to values? Or is it value-neutral?

(15) Science and religion. Do the findings and conclusions of science have any implications for traditional religious or theological commitments? If so, what are they?

(16) Science and culture. Both religion and the domain of values may be considered to be parts or aspects of culture. But surely the term culture also refers to other activities and practices. What is the relationship of science to these?

(17) The limits of science. Are there limits of science? If so, what are they? By what criteria, if any, can we establish that such limits are genuine?

IV. Philosophy of Science and Science

We hope that by now the reader has a fair grasp of what philosophy of science is. In order to provide further understanding, let us examine one way by which one might contrast science with the philosophy of science. We may best do this by focusing on the activities and concerns of scientists and of philosophers of science. There are many ways in which these differ. Let us look at just a couple of them. According to one widely held view:

(1) Scientists (among other things, and not necessarily in this order): (a) observe what happens in the world and note regularities; (b) experiment—i.e., manipulate (some) things so that they can be observed under special circumstances; (c) discover (or postulate) laws of nature which are intended to explain regularities; (d) combine laws of nature into theories or subsume those laws under theories. Philosophers of science do none of the above things. Rather, they ask questions such as: What is a law of nature? What is a scientific (vs. a nonscientific or unscientific) theory? What are the criteria (if any) by which to distinguish or demarcate those theories which are genuinely scientific from those which are not? Furthermore, according to this view:

(2) Scientists, like almost everyone else, make deductions. For example, they often construct a certain theory from various laws and observations and then from it deduce other theories or laws, or even certain specific occurrences which serve to test a theory. Philosophers of science do not do that. Rather they clarify the nature of deduction (and how it differs from other inferences or reasoning), and they describe the role deduction plays in science. For example, they ask how deduction is involved in the testing of theories.

...amples of (some of) the activities of scientists and philosophers of science, we may see that (according to the view we are considering): Whereas science is largely empirical, synthetic, and experimental, philosophy of science is largely verbal, analytic, and reflective. To be sure, in the works of some scientists—especially those who are in the more "theoretical" sciences—verbal, analytic, and reflective features may be found. But the converse is not generally true. The activities of philosophers of science are, for the most part, not empirical or experimental, and they do not add to our store of factual knowledge of the actual world. And even in those cases where the more "philosophical" activities are found in science, they are usually not pursued with the same rigor or toward the same ends as they are by philosophers of science.

We may roughly see the difference by examining the following table:

Philosophy of science	——	is comprised of	——	a metalanguage
				↓ which refers to ↓
Science	——	is comprised of	——	an object-language
				↓ which refers to ↓
Reality (or the world)	——	is comprised of	——	objects, processes, etc.

Thus we may see that, whereas science uses (an object-) language to talk about the objects of the world, philosophy of science (or at least a large "part" of it) uses (a meta-) language to talk about the language of science. In short, as a slight oversimplification, we may say: Science is talk about the world (a certain *kind* of talk, of course). Philosophy is talk about language (again, a certain kind of talk, of course).

To summarize the view we have considered: (1) The sciences consist of such things as listings of data, generalizations from them, the formulation of laws or trends, theoretical interpretations of data or laws, and arguments and evidence in favor of them. (2) Philosophy of science, to a large extent, consists of remarks about the language of science: the analysis of concepts, methods, and arguments of the various sciences; and also the analysis of the principles underlying science.

It is hoped now that our earlier characterization of the philosophy of science may be more readily understood and appreciated. Once again, according to that characterization, philosophy of science is the attempt to understand the meaning, method, and logical structure of science by means of a logical and methodological analysis of the aims, methods, criteria, concepts, laws, and theories of science.

One might reasonably object: But this view of philosophy of science does not do justice or apply to the list of topics in the philosophy of science provided in the preceding section. We are sympathetic to such an objection. Whereas our initial characterization does apply to many of the problems and concerns found in that list, it does not apply to others—for example, the topics of science and religion, or science and culture. Hence we propose that our initial characterization be modified in order to take such matters into account. We propose the following as an amended characterization of the philosophy of science. Philosophy of science is the attempt (a) to understand the method, foundations, and logical structure of science and (b) to examine the relations and interfaces of science and other human concerns, institutions, and quests, by means of (c) a logical and methodological analysis both of the aims, methods, and criteria of science and of the aims, methods, and concerns of various cultural phenomena in their relations to science.

V. The Scope of This Book

As we have mentioned, we cannot within a single volume do justice to all of the topics which fall within the domain of philosophy of science. We have therefore chosen six topics which (a) are crucial ones in philosophy of science, (b) are intrinsically interesting to the layperson as well as to the scientist or philosopher, and (c) are accessible to the beginning student. Similarly, the readings we have selected reflect those features. The topics are:

1. Science and Pseudoscience
2. The Natural and Social Sciences
3. Explanation and Law
4. Theory and Observation
5. Confirmation and Acceptance
6. Science and Values

Since we have provided discussions of these topics in the introductions to the parts of the book, we shall not make further comments about them at this point.

We truly hope that the readers of this volume will derive as much enjoyment from the book as we have had in our production of it. We urge that the Study Questions at the end of each part be utilized. For further reading we have provided selected bibliographies.

E.D.K.

1. Many of the views regarding science and the philosophy of science presented in this introduction and in the introduction to Part 1 stem from the lectures and writings of Herbert Feigl, May Brodbeck, John Hospers, and Sir Karl Popper.

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[13] Toulmin, S. *The Philosophy of Science.* New York: Harper & Row, 1960. [Still worth reading; author takes a novel approach to science.]

*[1], [3], [8], [9], [10], and [12] are anthologies that deal with various issues in the philosophy of science, including many discussed in this volume.

Part 1

Science and Pseudoscience

Introduction

The major topics we shall discuss in this essay are: the aims of science; the criteria of science, or the criteria for distinguishing that which is scientific from that which is nonscientific; the question "What is science?"; and the central issues of the readings which follow. But, first, let us begin by making some distinctions.

I. Some Distinctions

Before turning to the topics above it will be helpful to consider some ways of classifying the various sciences. Among these, the following should be noted.

(1) Pure sciences versus applied sciences. It is widely held that we must distinguish: (A) science as a field of knowledge (or set of cognitive disciplines) from (B) the applications of science. It is common to refer to these as the pure and applied sciences. (A) Among the pure sciences we may distinguish: (a) the formal sciences, logic, and mathematics; and (b) the factual or empirical sciences. Among the latter we may also distinguish: (b1) the natural sciences, which include the physical sciences, physics, chemistry, and so on, and the life and behavioral sciences, such as biology and psychology; and (b2) the social sciences, such as sociology and economics. (B) The applied sciences include the technological sciences—such as engineering and aeronautics, medicine, agriculture, and so on.

It should be noted that there are at least two levels of application among the various sciences. There is, first, the application of the formal sciences to the pure, factual sciences. Since the factual sciences must have logical form and usually utilize some mathematics, such application is often held to be essential for the development of the pure factual sciences. Different from this

Here the findings of the pure, empirical sciences are applied (in a different sense of "applied") to disciplines which fulfill various social, human purposes, such as building houses or roads and health care.

(2) Law-finding sciences versus fact-finding sciences. We recognize that such sciences as chemistry and physics attempt to discover universal laws which are applicable everywhere at all times, whereas such sciences as geography, history (if it is a science), and perhaps economics are concerned with local events. It is often said that the subject matter of the latter consists of particular facts, not general laws. As a result, there are some who wish to limit the term "science" to the law-finding sciences. Upon the basis of the criteria of science (such as those which will be presented later, or others), we believe that we may say that both the law-finding disciplines and the fact-finding disciplines are capable of being sciences if those (or other) criteria are met. Furthermore, one might argue that there are no purely fact-finding sciences. If so, to speak of law-finding versus fact-finding may, in many cases, indicate an artificial disjunction.

(3) Natural sciences versus social sciences. Related to (2), we find that some would limit the giving of scientific status to the natural sciences alone. Sometimes the reason given is the distinction referred to above—that the natural sciences are primarily law-finding, whereas the social sciences are predominantly fact-finding. But sometimes the distinction is based on subject matter. Hence it is held by some that natural phenomena constitute the field of science but cultural phenomena constitute the field of scholarship and require understanding, *verstehen,* and empathy. But there are points at which the classification does not hold up. First, there are some predominantly fact-finding natural sciences, such as geography, geology, and paleontology. And there are some law-finding social sciences, such as sociology and linguistics. Second, the distinction according to subject matter is not a clear-cut one. Hence we shall take a "liberal" view of science and allow the use of the term "science" to apply to both the natural and the social sciences—with the recognition that there are some differences.

It is widely held that distinctions (2) and (3) do not hold up but that (1) is an acceptable distinction. However, as we shall see in the readings which follow, some have even raised doubts about the significance of (1). Here, as always, we urge the reader to reflect upon these matters.

II. The Aims of Science

Let us now turn to the question "What are the aims of science?" Using the above distinction between pure (empirical) and applied science, we may then cite the following as some of the aims of science.

(1) The aims of applied science include: control, planning, technological progress; the utilization of the forces of nature for practical purposes. Obvious examples are: flood control, the construction of sturdier bridges, and the improvement of agriculture. Since this is all fairly obvious, no further elaboration is needed.

(2) The aims of the pure, factual sciences may be considered from two standpoints. (a) Psychologically considered, the aims of the pure, empirical sciences are: the pursuit of knowledge; the attainment of truth (or the closest possible realization of truth); the satisfaction of using our intellectual powers to explain and predict accurately. Scientists, of course, derive enjoyment from rewards, prestige, and competing with others. But they often achieve a genuine inner gratification which goes with the search for truth. In some ways this is similar in quality to artistic satisfaction. It is seen, for example, in the enjoyment one derives from the solution of a difficult problem.

(b) Logically considered, the aims of the pure, factual sciences are often held to be: description, explanation, and prediction. (b1) Description includes giving an account of what we observe in certain contexts, the formulation of propositions which apply to (or correspond to) facts in the world. (b2) Explanation consists of accounting for the facts and regularities we observe. It involves asking and answering "Why?" or "How come?" This may be done by subsuming facts under laws and theories. (b3) Prediction is closely related to explanation. It consists in deriving propositions which refer to events which have not yet happened, the deducing of propositions from laws and theories and then seeing if they are true, and hence provide a testing of those laws and theories. (b4) We might also mention post- or retrodiction, the reconstruction of past events. This process is also inferential in character. Since these issues will be discussed in subsequent parts of this volume, we shall not elaborate upon them at this time. (See some of the readings in part 1 and those in parts 2, 3, and 4.) However, we might mention that, here again, there is not unanimity with regard to the aims characterized above. Once more we urge the reader to think about these (and other) issues.

III. The Criteria of Science

In this section we shall state and discuss one view with regard to what are the essential criteria of science, that is, those criteria which may be used for at least two purposes: first, to distinguish science from commonsense knowledge (without claiming that the two are radically disjunctive—in some cases they may differ only in degree, not in kind); second, to distinguish that which is scientific, on the one hand, from that which is either nonscientific or unscientific, on the other—for example, to distinguish between theories which are genuinely scientific and those which are not. It has been maintained that any those criteria.

Before turning to the view which we have selected for consideration, let us consider an example. It is quite likely that most scientists and others who reflect upon science would hold that (say) Newton's theory of gravitation is scientific (even if it had to be modified), whereas (say) astrology is not scientific. Perhaps the reader would agree. But just what is it that allows us to rule in Newton's theory and to rule out astrology? In order to stimulate the reader's reflection, we shall consider one view of what the criteria for making such distinctions are. These criteria have been stated by Professor Herbert Feigl in various lectures and in writing. Our discussion of them corresponds fairly closely to the discussion given by Professor Feigl.

The five criteria are:

(1) Intersubjective testability. This refers to the possibility of being, in principle, capable of corroboration or "check-up" by anyone. Hence: *intersubjective*. (Hence, private intuitions and so forth must be excluded.)

(2) Reliability. This refers to that which, when put to a test, turns out to be true, or at least to be that which we can most reasonably believe to be true. Testing is not enough. We want theories which, when tested, are found to be true.

(3) Definiteness and precision. This refers to the removal of vagueness and ambiguity. We seek, for example, concepts which are definite and delimited. We are often helped here by measurement techniques and so forth.

(4) Coherence or systematic character. This refers to the organizational aspect of a theory. A set of disconnected statements is not as fruitful as one which has systematic character. It also refers to the removal of, or being free from, contradictoriness.

(5) Comprehensiveness or scope. This refers to our effort to attain a continual increase in the completeness of our knowledge and also to our seeking theories which have maximum explanatory power—for example, to account for things which other theories do not account for.

Let us consider these criteria in greater detail.

(1) Intersubjective testability. (a) Testability. We have noted that in science we encounter various kinds of statements: descriptions, laws, theoretical explanations, and so on. These are put forth as knowledge-claims. We must (if possible) be able to tell whether evidence speaks for or against such knowledge-claims. If the propositions which express those claims are not capable of tests, we cannot call those propositions true or false or even know how to go about establishing their truth or falsity. It should be noted that the criterion is one of testability, not tested. For example, at a given point in time, "There are mountains on the far side of the moon" was testable though not tested.

(b) Intersubjective. "Intersubjectivity" is often employed as a synonym for "objectivity." And the latter term has various meanings. Some of these are: (i) A view or belief is said to be objective if it is not based on illusions, hallucinations, deceptions, and so on. (ii) Something is referred to as objective if it is not merely a state of mind but is really "out there" in the external world. (iii) We often use "objective" to indicate the absence of bias and the presence of disinterestedness and dispassionateness. (iv) "Objectivity" also refers to the possibility of verification by others, and hence excludes beliefs, which stem from private, unique, unrepeatable experiences. Science strives for objectivity in all of these senses. Hence we take "intersubjective" to include all of them.

(c) Intersubjective testability. It is often held that (according to the view we are considering) in order for a proposition or theory to be judged scientific it must meet this first requirement. Indeed, many of the other criteria presuppose intersubjective testability. We cannot even begin to talk of reliability or precision unless this first criterion has been met.

(2) Reliability. Science is not merely interested in hypotheses which are intersubjectively testable. It is also interested in those which are true or at least have the greatest verisimilitude or likelihood of being true. Hence the need arises for the criterion of reliability. Whereas the first criterion stressed the possibility of finding assertions which are true *or* false, the second stresses the end result of that process. We judge a claim or body of knowledge to be reliable if it contains not merely propositions which are capable of being true *or* false but rather those which *are true* or which have the greatest verisimilitude. We find such propositions to be true (or false) by means of confirmation. Complete verification, and hence complete certainty, cannot be achieved in the factual sciences.

It should be noted that, first, the reliability of scientific assertions make them useful for prediction; second, although the assertions of many enterprises are testable (for example, those of astrology as much as those of astronomy), only some of them are reliable. And we reject some of them precisely because they are unreliable. The evidence is against them; we do not attain truth by means of them.

(3) Definiteness and precision. The terms "definiteness" and "precision" may be used in at least two related senses. First, they refer to the delimitation of our concepts and to the removal of ambiguity or vagueness. Second, they refer to a more rigid or exact formulation of *laws*. For example, "It is more probable than not that *X* causes disease *Y*" is less desirable than "The probability that *X* causes *Y* is 98%."

(4) Coherence or systematic character. In the sciences, we seek not merely disorganized or loosely related facts but a well-connected account of the facts. It has been held by many that we achieve this via what has been called the hypothetico-deductive procedure of science. This procedure includes: (a) our beginning with a problem (which pertains to some realm of phenomena); (b) the formulation of hypotheses, laws, and theories by which

to account for those phenomena or by which to resolve the problem; (c) the deriving (from (b)) of statements which refer to observable facts; (d) the testing of those deduced assertions to see if they hold up. Thus we seek an integrated, unified network, not merely a congeries of true statements.

But, of course, we also seek theories which are consistent, which are free from self-contradictions. The reason for insisting upon such coherence is obvious; hence there is no need for elaboration.

(5) Comprehensiveness or scope. The terms "comprehensiveness," and "scope" are also used in two senses, both of which are essential in science. First, a theory is said to be comprehensive if it possesses maximum explanatory power. Thus Newton's theory of gravitation was ranked high partly because it accounted not only for the laws of falling bodies but also for the revolution of the heavenly bodies and for the laws of tides. Second, by "comprehensive" we often refer to the completeness of our knowledge. This of course does not mean finality. We do not think of the hypotheses of the empirical sciences as being certain for all time. Rather we must be ready to modify them or even, on occasion, to abandon them.

To summarize: According to the view we have presented, we judge a law, hypothesis, theory, or enterprise to be scientific if it meets all five of the above criteria. If it fails to meet all five, it is judged to be unscientific or at least nonscientific. To return to our earlier example, it seems clear that Newton's theory thus passes the test. Astrological theory or Greek mythology does not.

It should be noted that, in presenting Professor Feigl's criteria for the reader's consideration, we do not claim that they are correct or free from defects. Indeed, as we shall see in the readings which follow, many writers have rejected some (or all) of those criteria. The reader should once again attempt to seek an acceptable criterion or set of criteria, if such can be found.

IV. What is Science?

A common characterization of science (or sometimes of scientific method) runs as follows. Science is knowledge obtained by: (1) making observations as accurate and definite as possible; (2) recording these intelligibly; (3) classifying them according to the subject matter being studied; (4) extracting from them, by induction, general statements (laws) which assert regularities; (5) deducing other statements from these; (6) verifying those statements by further observation; and (7) propounding theories which connect and so account for the largest possible number of laws. It is further maintained that this process runs from (1) through (7) *in that order.*

The conception of science has been challenged in recent years. Its most severe critic is Sir Karl Popper. (See the selection in part 1 of this volume.) We shall not repeat Popper's criticisms. Instead we offer a characterization of science which some believe to be more adequate than the one mentioned above and which they deem to be free from the defects it possesses.

According to this view, the following is at least a minimal characterization of (factual) science (or of a science).

Science is a body of knowledge which consists of the following, coherently organized in a systematic way:

(a) Statements which record and classify observations which are relevant for the solution of a problem in as accurate and definite a way as possible.

(b) General statements—laws or hypotheses—which assert regularities among certain classes of observed or observable phenomena.

(c) Theoretical statements which connect and account for the largest possible number of laws.

(d) Other general or specific statements which are deducible from the initial descriptions and from laws and theories and which are confirmed by further observation and testing.

At least two things should be noted about this characterization. First, it indicates the role of the formal sciences in the empirical sciences. Mathematics is important for (a); logic is important for (d). Second, nothing is said in this characterization about the *method* of obtaining knowledge or of obtaining laws. It may be induction, but it may also be a guess, intuition, hunch, or whatever.

Since a number of the readings in part 1 deal with the question "What is science?" we shall not attempt to provide a "final" answer. Instead, we encourage the student to come up with the best answer possible, based on his or her reading and reflection.

V. The Readings in Part 1

Since the essays contained in part 1 are clearly written and since they are accessible to the beginning student or ordinary reader, no detailed summaries will be presented here. We urge the student to prepare his or her own summaries and to make use of the Study Questions at the end. However, a few brief remarks may be helpful.

Throughout many of his works, Sir Karl Popper has been concerned with the problem of how to distinguish between science and pseudoscience (or nonscience). He claims to have solved that problem by having provided a criterion of demarcation, a criterion by which to distinguish theories which are genuinely scientific from those which are not. By means of this criterion—of falsifiability or refutability—he attempts to show that Einstein's theory of gravitation satisfies the criterion (and hence is scientific) whereas astrology, the Marxist theory of history, and various psychoanalytic theories—for varying reasons—are not scientific. He also wishes to separate the problem from the problem of meaning and maintains that the latter is a pseudoproblem. (The reader should reflect upon why he holds that it is a pseudoproblem and whether he has succeeded in showing that it is.)

In the middle sections of Popper's essay, he claims that the problem of demarcation has provided a key for solving a number of philosophical problems, especially the problem of induction. Since this issue does not pertain to the main topics of part 1, most of those sections of the essay have been deleted here. The problem of induction is: How, if at all, can we justify our knowledge-claims concerning matters of fact which we have not yet experienced or are not now experiencing? In the eighteenth century, David Hume maintained that we cannot provide any rational justification. Popper agrees with Hume's logical refutation of induction but disagrees with his psychological explanation of induction (in terms of custom or habit).

The selection by John Ziman consists of extracts from his book on science. In the first part he discusses and rejects various definitions of science which have been held. And he attempts to formulate a more accurate and tenable characterization based on what he takes to be the goal or objective of science, namely, consensus of rational opinion "over the widest possible field." In the second part, he provides his answer to the question "What distinguishes science from nonscience?" The reader should attempt to decide whether his "criterion of demarcation" is an improvement over Popper's and, if so, why. Since this selection is unusually clear and readable, no further comments are required.

Feyerabend's essay is, no doubt, one of the most controversial ones in this volume. Feyerabend claims that he wishes to defend society and its inhabitants from all ideologies, including science. He likens them (again, including science) to fairytales "which have lots of interesting things to say but which also contain wicked lies." He goes on to consider an argument designed to defend the exceptional status which science has in society today. According to this argument "(1) science has finally found the correct method for achieving results and (2) . . . there are many results to prove the excellence of the method." In the next sections he argues against both (1) and (2). He concludes his essay with a provocative discussion of education and myth. We urge the reader to reflect seriously upon Feyerabend's somewhat unorthodox views and to ask whether Feyerabend has adequately defended them.

Paul R. Thagard's essay constitutes both a further discussion of some of the above-mentioned topics (such as the criterion of demarcation) and an example of the application of them. Most scientists and philosophers agree that astrology is a pseudoscience. Thagard attempts to show why it is. After presenting a brief description of astrology, he attempts to show that the major objections which have been provided do not show that it is a pseudoscience. Thagard then proposes his principle of demarcation and, upon the basis of it, claims to show that and why astrology is unscientific.

In his important essay, Philip Kitcher specifies various criteria which must be met before a view or a criticism can be scientific. He then applies this to the views of Creationists and also to their criticisms of evolutionary theory. He attempts to show that their views and criticisms are either fallacious or totally unsupported.

There is a kind of dialogue which runs through the essays in this part. We urge the reader to critically evaluate the various positions presented and attempt to come to his or her own conclusion with regard to the questions "What is science?" "By what criteria can we distinguish science from nonscience or pseudoscience?" and so on. The Study Questions should provide assistance in gauging the reader's understanding of the selections and in grappling with these and related questions.

E. D. K.

1

Science: Conjectures and Refutations*

Sir Karl Popper

> Mr. Turnbull had predicted evil consequences, . . . and was now doing the best in his power to bring about the verification of his own prophecies.
>
> ANTHONY TROLLOPE

I

When I received the list of participants in this course and realized that I had been asked to speak to philosophical colleagues I thought, after some hesitation and consultation, that you would probably prefer me to speak about those problems which interest me most, and about those developments with which I am most intimately acquainted. I therefore decided to do what I have never done before: to give you a report on my own work in the philosophy of science, since the autumn of 1919 when I first began to grapple with the problem, *"When should a theory be ranked as scientific?"* or *"Is there a criterion for the scientific character or status of a theory?"*

The problem which troubled me at the time was neither, "When is a theory true?" nor, "When is a theory acceptable?" My problem was different. I *wished to distinguish between science and pseudoscience*; knowing very well that science often errs, and that pseudoscience may happen to stumble on the truth.

I knew, of course, the most widely accepted answer to my problem: that science is distinguished from pseudoscience—or from "metaphysics"—by its empirical method, which is essentially *inductive*, proceeding from observation or experiment. But this did not satisfy me. On the contrary, I often formulated my problem as one of distinguishing between a genuinely empirical method and a nonempirical or even a pseudoempirical method—that is to say, a method which, although it appeals to observation and experiment, nevertheless does not come up to scientific standards. The latter method may be exemplified by astrology with its stupendous mass of empirical evidence based on observation—on horoscopes and on biographies.

But as it was not the example of astrology which led me to my problem I should perhaps briefly describe the atmosphere in which my problem arose and the examples by which it was stimulated. After the collapse of the Austrian Empire there had been a revolution in Austria: the air was full of revolutionary slogans and ideas, and new and often wild theories. Among the theories which interested me Einstein's theory of relativity was no doubt by far the most important. Three others were Marx's theory of history, Freud's psychoanalysis, and Alfred Adler's so-called "individual psychology."

There was a lot of popular nonsense talked about these theories, and especially about relativity (as still happens even today), but I was fortunate in those who introduced me to the study of this theory. We all—the small circle of students to which I belonged—were thrilled with the result of Eddington's eclipse observations which in 1919 brought the first important confirmation of Einstein's theory of gravitation. It was a great experience for us, and one which had a lasting influence on my intellectual development.

The three other theories I have mentioned were also widely discussed among students at that time. I myself happened to come into personal contact with Alfred Adler, and even to cooperate with him in his social work among the children and young people in the working-class districts of Vienna where he had established social guidance clinics.

It was during the summer of 1919 that I began to feel more and more dissatisfied with these three theories—the Marxist theory of history, psychoanalysis, and individual psychology; and I began to feel dubious about their claims to scientific status. My problem perhaps first took the simple form, "What is wrong with Marxism, psychoanalysis, and individual psychology? Why are they so different from physical theories, from Newton's theory, and especially from the theory of relativity?"

To make this contrast clear I should explain that few of us at the time would have said that we believed in the truth of Einstein's theory of gravitation. This shows that it was not my doubting the truth of those other three theories which bothered me, but something else. Yet neither was it that I merely felt mathematical physics to be more exact than the sociological or psychological type of theory. Thus what worried me was neither the problem of truth, at that stage at least, nor the problem of exactness or measurability. It was rather that I felt that these other three theories, though posing as sci-

*A lecture given at Peterhouse, Cambridge, in Summer 1953, as part of a course on developments and trends in contemporary British philosophy, organized by the British Council, originally published under the title "Philosophy of Science. a Personal Report" in *British Philosophy in Mid-Century,* ed. C. A. Mace, 1957. [Portions have been deleted by the editors for this publication.]

… myths than with science; that they resembled astrology rather than astronomy.

I found that those of my friends who were admirers of Marx, Freud, and Adler, were impressed by a number of points common to these theories, and especially by their apparent explanatory power. These theories appeared to be able to explain practically everything that happened within the fields to which they referred. The study of any of them seemed to have the effect of an intellectual conversion or revelation, opening your eyes to a new truth hidden from those not yet initiated. Once your eyes were thus opened you saw confirming instances everywhere: the world was full of *verifications* of the theory. Whatever happened always confirmed it. Thus its truth appeared manifest; and unbelievers were clearly people who did not want to see the manifest truth; who refused to see it, either because it was against their class interest, or because of their repressions which were still "unanalyzed" and crying aloud for treatment.

The most characteristic element in this situation seemed to me the incessant stream of confirmations, of observations which "verified" the theories in question; and this point was constantly emphasized by their adherents. A Marxist could not open a newspaper without finding on every page confirming evidence for his interpretation of history; not only in the news, but also in its presentation—which revealed the class bias of the paper—and especially of course in what the paper *did not* say. The Freudian analysts emphasized that their theories were constantly verified by their "clinical observations." As for Adler, I was much impressed by a personal experience. Once, in 1919, I reported to him a case which to me did not seem particularly Adlerian, but which he found no difficulty in analyzing in terms of his theory of inferiority feelings, although he had not even seen the child. Slightly shocked, I asked him how he could be so sure. "Because of my thousandfold experience," he replied; whereupon I could not help saying: "And with this new case, I suppose, your experience has become thousand-and-one-fold."

What I had in mind was that his previous observations may not have been much sounder than this new one; that each in its turn had been interpreted in the light of "previous experience," and at the same time counted as additional confirmation. What, I asked myself, did it confirm? No more than that a case could be interpreted in the light of the theory. But this meant very little, I reflected, since every conceivable case could be interpreted in the light of Adler's theory, or equally of Freud's. I may illustrate this by two very different examples of human behavior: that of a man who pushes a child into the water with the intention of drowning it; and that of a man who sacrifices his life in an attempt to save the child. Each of these two cases can be explained with equal ease in Freudian and in Adlerian terms. According to Freud the first man suffered from repression (say, of some component of his Oedipus complex), while the second man had achieved sublimation. According to Adler the first man suffered from feelings of inferiority (producing perhaps the need to prove to himself that he dared to commit some crime), and so did the second man (whose need was to prove to himself that he dared to rescue the child). I could not think of any human behavior which could not be interpreted in terms of either theory. It was precisely this fact—that they always fitted, that they were always confirmed—which in the eyes of their admirers constituted the strongest argument in favor of these theories. It began to dawn on me that this apparent strength was in fact their weakness.

With Einstein's theory the situation was strikingly different. Take one typical instance—Einstein's prediction, just then confirmed by the findings of Eddington's expedition. Einstein's gravitational theory had led to the result that light must be attracted by heavy bodies (such as the sun), precisely as material bodies were attracted. As a consequence it could be calculated that light from a distant fixed star whose apparent position was close to the sun would reach the earth from such a direction that the star would seem to be slightly shifted away from the sun; or, in other words, that stars close to the sun would look as if they had moved a little away from the sun, and from one another. This is a thing which cannot normally be observed since such stars are rendered invisible in daytime by the sun's overwhelming brightness; but during an eclipse it is possible to take photographs of them. If the same constellation is photographed at night one can measure the distances on the two photographs, and check the predicted effect.

Now the impressive thing about this case is the *risk* involved in a prediction of this kind. If observation shows that the predicted effect is definitely absent, then the theory is simply refuted. The theory is *incompatible with certain possible results of observation*—in fact with results which everybody before Einstein would have expected.[1] This is quite different from the situation I have previously described, when it turned out that the theories in question were compatible with the most divergent human behavior, so that it was practically impossible to describe any human behavior that might not be claimed to be a verification of these theories.

These considerations led me in the winter of 1919–20 to conclusions which I may now reformulate as follows.

(1) It is easy to obtain confirmations, or verifications, for nearly every theory—if we look for confirmations.

(2) Confirmations should count only if they are the result of *risky predictions*; that is to say, if, unenlightened by the theory in question, we should have expected an event which was incompatible with the theory—an event which would have refuted the theory.

(3) Every "good" scientific theory is a prohibition: it forbids certain things to happen. The more a theory forbids, the better it is.

(4) A theory which is not refutable by any conceivable event is nonscientific. Irrefutability is not a virtue of theory (as people often think) but a vice.

(5) Every genuine test of a theory is an attempt to falsify it, or to refute it. Testability is falsifiability; but there are degrees of testability; some theories are more testable, more exposed to refutation, than others; they take, as it were, greater risks.

(6) Confirming evidence should not count *except when it is the result of a genuine test of the theory*; and this means that it can be presented as a serious but unsuccessful attempt to falsify the theory. (I now speak in such cases of "corroborating evidence.")

(7) Some genuinely testable theories, when found to be false, are still upheld by their admirers—for example by introducing ad hoc some auxiliary assumption, or by reinterpreting the theory ad hoc in such a way that it escapes refutation. Such a procedure is always possible, but it rescues the theory from refutation only at the price of destroying, or at least lowering, its scientific status. (I later described such a rescuing operation as a "*conventionalist twist*" or a "*conventionalist stratagem.*")

One can sum up all this by saying that the *criterion of the scientific status of a theory is its falsifiability, or refutability, or testability.*

II

I may perhaps exemplify this with the help of the various theories so far mentioned. Einstein's theory of gravitation clearly satisfied the criterion of falsifiability. Even if our measuring instruments at the time did not allow us to pronounce on the results of the tests with complete assurance, there was clearly a possibility of refuting the theory.

Astrology did not pass the test. Astrologers were greatly impressed, and misled, by what they believed to be confirming evidence—so much so that they were quite unimpressed by any unfavorable evidence. Moreover, by making their interpretations and prophecies sufficiently vague they were able to explain away anything that might have been a refutation of the theory had the theory and the prophecies been more precise. In order to escape falsification they destroyed the testability of their theory. It is a typical soothsayer's trick to predict things so vaguely that the predictions can hardly fail: that they become irrefutable.

The Marxist theory of history, in spite of the serious efforts of some of its founders and followers, ultimately adopted this soothsaying practice. In some of its earlier formulations (for example in Marx's analysis of the character of the "coming social revolution") their predictions were testable, and in fact falsified.[2] Yet instead of accepting the refutations the followers of Marx reinterpreted both the theory and the evidence in order to make them agree. In this way they rescued the theory from refutation; but they did so at the price of adopting a device which made it irrefutable. They thus gave a "conventionalist twist" to the theory; and by this stratagem they destroyed its much advertised claim to scientific status.

The two psychoanalytic theories were in a different class. They were simply nontestable, irrefutable. There was no conceivable human behavior which could contradict them. This does not mean that Freud and Adler were not seeing certain things correctly: I personally do not doubt that much of what they say is of considerable importance, and may well play its part one day in a psychological science which is testable. But it does mean that those "clinical observations" which analysts naively believe confirm their theory cannot do this any more than the daily confirmations which astrologers find in their practice.[3] And as for Freud's epic of the ego, the superego, and the id, no substantially stronger claim to scientific status can be made for it than for Homer's collected stories from Olympus. These theories describe some facts, but in the manner of myths. They contain most interesting psychological suggestions, but not in a testable form.

At the same time I realized that such myths may be developed, and become testable; that historically speaking all—or very nearly all—scientific theories originate from myths, and that a myth may contain important anticipations of scientific theories. Examples are Empedocles' theory of evolution by trial and error, or Parmenides' myth of the unchanging block universe in which nothing ever happens and which, if we add another dimension, becomes Einstein's block universe (in which, too, nothing ever happens, since everything is, four-dimensionally speaking, determined and laid down from the beginning). I thus felt that if a theory is found to be nonscientific, or "metaphysical" (as we might say), it is not thereby found to be unimportant, or insignificant, or "meaningless," or "nonsensical."[4] But it cannot claim to be backed by empirical evidence in the scientific sense—although it may easily be, in some genetic sense, the "result of observation."

(There were a great many other theories of this prescientific or pseudoscientific character, some of them, unfortunately, as influential as the Marxist interpretation of history; for example, the racialist interpretation of history—another of those impressive and all-explanatory theories which act upon weak minds like revelations.)

Thus the problem which I tried to solve by proposing the criterion of falsifiability was neither a problem of meaningfulness or significance, nor a problem of truth or acceptability. It was the problem of drawing a line (as well as this can be done) between the statements, or systems of statements, of the empirical sciences, and all other statements—whether they are of a religious or of a metaphysical character, or simply pseudoscientific. Years later—it must have been in 1928 or 1929—I called this first problem of mine the "*problem of demarcation.*" The criterion of falsifiability is a solution to this problem of demarcation, for it says that statements or systems of statements, in order to be ranked as scientific, must be capable of conflicting with possible, or conceivable, observations. . . .

Let us now turn from our logical criticism of the *psychology of experience* to our real problem—the problem of the *logic of science.* Although some of the things I have said may help us here, insofar as they may have eliminated certain psychological prejudices in favor of induction, my treatment of the *logical problem of induction* is completely independent of this criticism, and of all psychological considerations. Provided you do not dogmatically believe in the alleged psychological fact that we make inductions, you may now forget my whole story with the exception of two logical points: my logical remarks on testability or falsifiability as the criterion of demarcation; and Hume's logical criticism of induction.

From what I have said it is obvious that there was a close link between the two problems which interested me at that time: demarcation, and induction or scientific method. It was easy to see that the method of science is criticism, i.e., attempted falsifications. Yet it took me a few years to notice that the two problems—of demarcation and of induction—were in a sense one. . . .

I recently came across an interesting formulation of this belief in a remarkable philosophical book by a great physicist—Max Born's *Natural Philosophy of Cause and Chance.*[5] He writes: "Induction allows us to generalize a number of observations into a general rule: that night follows day and day follows night. . . . But while everyday life has no definite criterion for the validity of an induction, . . . science has worked out a code, or rule of craft, for its application." Born nowhere reveals the contents of this inductive code (which, as his wording shows, contains a "definite criterion for the validity of an induction"); but he stresses that "there is no logical argument" for its acceptance: "it is a question of faith"; and he is therefore "willing to call induction a metaphysical principle." But why does he believe that such a code of valid inductive rules must exist? This becomes clear when he speaks of the "vast communities of people ignorant of, or rejecting, the rule of science, among them the members of antivaccination societies and believers in astrology. It is useless to argue with them; I cannot compel them to accept the same criteria of valid induction in which I believe: the code of scientific rules." This makes it quite clear that *"valid induction" was here meant to serve as a criterion of demarcation between science and pseudoscience.*

But it is obvious that this rule or craft of "valid induction" is not even metaphysical: it simply does not exist. No rule can ever guarantee that a generalization inferred from true observations, however often repeated, is true. (Born himself does not believe in the truth of Newtonian physics, in spite of its success, although he believes that it is based on induction.) And the success of science is not based upon rules of induction, but depends upon luck, ingenuity, and the purely deductive rules of critical argument.

I may summarize some of my conclusions as follows:

(1) Induction, i.e., inference based on many observations, is a myth. It is neither a psychological fact, nor a fact of ordinary life, nor one of scientific procedure.

(2) The actual procedure of science is to operate with conjectures: to jump to conclusions—often after one single observation (as noticed for example by Hume and Born).

(3) Repeated observations and experiments function in science as tests of our conjectures or hypotheses, i.e., as attempted refutations.

(4) The mistaken belief in induction is fortified by the need for a criterion of demarcation which, it is traditionally but wrongly believed, only the inductive method can provide.

(5) The conception of such an inductive method, like the criterion of verifiability, implies a faulty demarcation.

(6) None of this is altered in the least if we say that induction makes theories only probable rather than certain.

IV

If, as I have suggested, the problem of induction is only an instance or facet of the problem of demarcation, then the solution to the problem of demarcation must provide us with a solution to the problem of induction. This is indeed the case, I believe, although it is perhaps not immediately obvious.

For a brief formulation of the problem of induction we can turn again to Born, who writes: ". . . no observation or experiment, however extended, can give more than a finite number of repetitions"; therefore, "the statement of a law—B depends on A—always transcends experience. Yet this kind of statement is made everywhere and all the time, and sometimes from scanty material."[6]

In other words, the logical problem of induction arises from (a) Hume's discovery (so well expressed by Born) that it is impossible to justify a law by observation or experiment, since it "transcends experience"; (b) the fact that science proposes and uses laws "everywhere and all the time." (Like Hume, Born is struck by the "scanty material," i.e., the few observed instances upon which the law may be based.) To this we have to add (c) *the principle of empiricism* which asserts that in science, only observation and experiment may decide upon the *acceptance or rejection* of scientific statements, including laws and theories.

These three principles, (a), (b), and (c), appear at first sight to clash; and this apparent clash constitutes the *logical problem of induction.*

Faced with this clash, Born gives up (c), the principle of empiricism (as Kant and many others, including Bertrand Russell, have done before him), in favor of what he calls a "metaphysical principle," a metaphysical principle which he does not even attempt to formulate; which he vaguely describes as a "code or rule of craft," and of which I have never seen any formulation which even looked promising and was not clearly untenable.

But in fact the principles (a) to (c) do not clash. We can see this the moment we realize that the acceptance by science of a law or of a theory is *tentative only*; which is to say that all laws and theories are conjectures, or tentative *hypotheses* (a position which I have sometimes called "hypotheticism"); and that we may reject a law or theory on the basis of new evidence, without necessarily discarding the old evidence which originally led us to accept it.[7]

The principles of empiricism (c) can be fully preserved, since the fate of a theory, its acceptance or rejection, is decided by observation and experiment by the result of tests. So long as a theory stands up to the severest tests we can design, it is accepted; if it does not, it is rejected. But it is never inferred, in any sense, from the empirical evidence. There is neither a psychological nor a logical induction. *Only the falsity of the theory can be inferred from empirical evidence, and this inference is a purely deductive one.*

Hume showed that it is not possible to infer a theory from observation statements; but this does not affect the possibility of refuting a theory by observation statements. The full appreciation of the possibility makes the relation between theories and observations perfectly clear.

This solves the problem of the alleged clash between the principles (a), (b), and (c), and with it Hume's problem of induction. . . .

Notes

1. This is a slight oversimplification, for about half of the Einstein effect may be derived from the classical theory, provided we assume a ballistic theory of light.

2. See, for example, my *Open Society and Its Enemies,* ch. 15, section iii, and notes 13–14.

3. "Clinical observations," like all other observations, are *interpretations in the light of theories*; and for this reason alone they are apt to seem to support those theories in the light of which they were interpreted. But real support can be obtained only from observations undertaken as tests (by "attempted refutations"); and for this purpose *criteria of refutation* have to be laid down beforehand: it must be agreed which observable situations, if actually observed, mean that the theory is refuted. But what kind of clinical responses would refute to the satisfaction of the analyst not merely a particular analytic diagnosis but psychoanalysis itself? And have such criteria ever been discussed or agreed upon by analysts? Is there not, on the contrary, a whole family of analytic concepts, such as "ambivalence" (I do not suggest that there is no such thing as ambivalence), which would make it difficult, if not impossible, to agree upon such criteria? Moreover, how much headway has been made in investigating the question of the extent to which the (conscious or unconscious) expectations and theories held by the analyst influence the "clinical responses" of the patient? (To say nothing about the conscious attempts to influence the patient by proposing interpretations to him, etc.) Years ago I introduced the term "Oedipus effect" to describe the influence of a theory or expectation or prediction *upon the event which it predicts or describes*: it will be remembered that the causal chain leading to Oedipus' parricide was started by the oracle's prediction of this event. This is a characteristic and recurrent theme of such myths, but one which seems to have failed to attract the interest of the analysts, perhaps not accidentally. (The problem of confirmatory dreams suggested by the analyst is discussed by Freud, for example in *Gesammelte Schriften,* III, 1925, where he says on page 314: "If anybody asserts that most of the dreams which can be utilized in an analysis . . . owe their origin to [the analyst's] suggestion, then no objection can be made from the point of view of analytic theory. Yet there is nothing in this fact," he surprisingly adds, "which would detract from the reliability of our results.")

4. The case of astrology, nowadays a typical pseudoscience, may illustrate this point. It was attacked, by Aristotelians and other rationalists, down to Newton's day, for the wrong reason—for its now accepted assertion that the planets had an "influence" upon terrestrial ("sublunar") events. In fact Newton's theory of gravity, and especially the lunar theory of the tides, was historically speaking an offspring of astrological lore. Newton, it seems, was most reluctant to adopt a theory which came from the same stable as for example the theory that "influenza" epidemics are due to an astral "influence." And Galileo, no doubt for the same reason, actually rejected the lunar theory of the tides; and his misgivings about Kepler may easily be explained by his misgivings about astrology.

5. Max Born, *Natural Philosophy of Cause and Chance,* Oxford, 1949, p. 7.

6. *Natural Philosophy of Cause and Chance,* p. 6.

7. I do not doubt that Born and many others would agree that theories are accepted only tentatively. But the widespread belief in induction shows that the far-reaching implications of this view are rarely seen.