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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.

examples 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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*[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

*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.]

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 scimyths 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. . . .

III

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.

17

The Nature of Theories

Rudolf Carnap

1. Theories and Nonobservables

One of the most important distinctions between two types of laws in science is the distinction between what may be called (there is no generally accepted terminology for them) empirical laws and theoretical laws. Empirical laws are laws that can be confirmed directly by empirical observations. The term "observable" is often used for any phenomenon that can be directly observed, so it can be said that empirical laws are laws about observables.

Here, a warning must be issued. Philosophers and scientists have quite different ways of using the terms "observable" and "nonobservable." To a philosopher, "observable" has a very narrow meaning. It applies to such properties as "blue," "hard," "hot." These are properties directly perceived by the senses. To the physicist, the word has a much broader meaning. It includes any quantitative magnitude that can be measured in a relatively simple, direct way. A philosopher would not consider a temperature of, perhaps, 80 degrees centigrade, or a weight of 93½ pounds, an observable because there is no direct sensory perception of such magnitudes. To a physicist, both are observables because they can be measured in an extremely simple way. The object to be weighed is placed on a balance scale. The temperature is measured with a thermometer. The physicist would not say that the mass of a molecule, let alone the mass of an electron, is something observable, because here the procedures of measurement are much more complicated and indirect. But magnitudes that can be established by simple procedures—length with a ruler, time with a clock, or frequency of light waves with a spectrometer—are called observables.

A philosopher might object that the intensity of an electric current is not really observed. Only a pointer position was observed. An ammeter was attached to the circuit and it was noted that the pointer pointed to a mark

labeled 5.3. Certainly the current's intensity was not observed. It was *inferred* from what was observed.

The physicist would reply that this was true enough, but the inference was not very complicated. The procedure of measurement is so simple, so well established, that it could not be doubted that the ammeter would give an accurate measurement of current intensity. Therefore, it is included among what are called observables.

There is no question here of who is using the term "observable" in a right or proper way. There is a continuum which starts with direct sensory observations and proceeds to enormously complex, indirect methods of observation. Obviously no sharp line can be drawn across this continuum; it is a matter of degree. A philosopher is sure that the sound of his wife's voice, coming from across the room, is an observable. But suppose he listens to her on the telephone. Is her voice an observable or isn't it? A physicist would certainly say that when he looks at something through an ordinary microscope, he is observing it directly. Is this also the case when he looks into an electron microscope? Does he observe the path of a particle when he sees the track it makes in a bubble chamber? In general, the physicist speaks of observables in a very wide sense compared with the narrow sense of the philosopher, but, in both cases, the line separating observable from nonobservable is highly arbitrary. It is well to keep this in mind whenever these terms are encountered in a book by a philosopher or scientist. Individual authors will draw the line where it is most convenient, depending on their points of view, and there is no reason why they should not have this privilege.

Empirical laws, in my terminology, are laws containing terms either directly observable by the senses or measurable by relatively simple techniques. Sometimes such laws are called empirical generalizations, as a reminder that they have been obtained by generalizing results found by observations and measurements. They include not only simple qualitative laws (such as, "All ravens are black") but also quantitative laws that arise from simple measurements. The laws relating pressure, volume, and temperature of gases are of this type. Ohm's law, connecting the electric potential difference, resistance, and intensity of current, is another familiar example. The scientist makes repeated measurements, finds certain regularities, and expresses them in a law. These are the empirical laws. As indicated in earlier chapters, they are used for explaining observed facts and for predicting future observable events.

There is no commonly accepted term for the second kind of laws, which I call *theoretical laws.* Sometimes they are called abstract or hypothetical laws. "Hypothetical" is perhaps not suitable because it suggests that the distinction between the two types of laws is based on the degree to which the laws are confirmed. But an empirical law, if it is a tentative hypothesis, confirmed only to a low degree, would still be an empirical law although it might by the fact that it contains terms of a different kind. The terms of a theoretical law do not refer to observables even when the physicist's wide meaning of what can be observed is adopted. They are laws about such entities as molecules, atoms, electrons, protons, electromagnetic fields, and others that cannot be measured in simple, direct ways. . . .

It is true, as shown earlier, that the concepts "observable" and "nonobservable" cannot be sharply defined because they lie on a continuum. In actual practice, however, the difference is usually great enough so there is not likely to be debate. All physicists would agree that the laws relating pressure, volume, and temperature of a gas, for example, are empirical laws. Here the amount of gas is large enough so that the magnitudes to be measured remain constant over a sufficiently large volume of space and period of time to permit direct, simple measurements which can then be generalized into laws. All physicists would agree that laws about the behavior of single molecules are theoretical. Such laws concern a microprocess about which generalizations cannot be based on simple, direct measurements.

Theoretical laws are, of course, more general than empirical laws. It is important to understand, however, that theoretical laws cannot be arrived at simply by taking the empirical laws, then generalizing a few steps further. How does a physicist arrive at an empirical law? He observes certain events in nature. He notices a certain regularity. He describes this regularity by making an inductive generalization. It might be supposed that he could now put together a group of empirical laws, observe some sort of pattern, make a wider inductive generalization, and arrive at a theoretical law. Such is not the case.

To make this clear, suppose it has been observed that a certain iron bar expands when heated. After the experiment has been repeated many times, always with the same result, the regularity is generalized by saying that this bar expands when heated. An empirical law has been stated, even though it has a narrow range and applies only to one particular iron bar. Now further tests are made of other iron objects with the ensuing discovery that every time an iron object is heated it expands. This permits a more general law to be formulated, namely that all bodies of iron expand when heated. In similar fashion, the still more general laws "All metals . . . ," then "All solid bodies . . . ," are developed. These are all simple generalizations, each a bit more general than the previous one, but they are all empirical laws. Why? Because in each case, the objects dealt with are observable (iron, copper, metal, solid bodies); in each case the increases in temperature and length are measurable by simple, direct techniques.

In contrast, a theoretical law relating to this process would refer to the behavior of molecules in the iron bar. In what way is the behavior of the molecules connected with the expansion of the bar when heated? You see at once the atomic theory of matter—and we are quickly plunged into atomic laws involving concepts radically different from those we had before. It is true that these theoretical concepts differ from concepts of length and temperature only in the degree to which they are directly or indirectly observable, but the difference is so great that there is no debate about the radically different nature of the laws that must be formulated.

Theoretical laws are related to empirical laws in a way somewhat analogous to the way empirical laws are related to single facts. An empirical law helps to explain a fact that has been observed and to predict a fact not yet observed. In similar fashion, the theoretical law helps to explain empirical laws already formulated, and to permit the derivation of new empirical laws. Just as the single, separate facts fall into place in an orderly pattern when they are generalized in an empirical law, the single and separate empirical laws fit into the orderly pattern of a theoretical law. This raises one of the main problems in the methodology of science. How can the kind of knowledge that will justify the assertion of a theoretical law be obtained? An empirical law may be justified by making observations of single facts. But to justify a theoretical law, comparable observations cannot be made because the entities referred to in theoretical laws are nonobservables. . . .

How can theoretical laws be discovered? We cannot say: "Let's just collect more and more data, then generalize beyond the empirical laws until we reach theoretical ones." No theoretical law was ever found that way. We observe stones and trees and flowers, noting various regularities and describing them by empirical laws. But no matter how long or how carefully we observe such things, we never reach a point at which we observe a molecule. The term "molecule" never arises as a result of observations. For this reason, no amount of generalization from observations will ever produce a theory of molecular processes. Such a theory must arise in another way. It is stated not as a generalization of facts but as a hypothesis. The hypothesis is then tested in a manner analogous in certain ways to the testing of an empirical law. From the hypothesis, certain empirical laws are derived, and these empirical laws are tested in turn by observation of facts. Perhaps the empirical laws derived from the theory are already known and well confirmed. (Such laws may even have motivated the formulation of the theoretical law.) Regardless of whether the derived empirical laws are known and confirmed, or whether they are new laws confirmed by new observations, the confirmation of such derived laws provides indirect confirmation of the theoretical law.

The point to be made clear is this. A scientist does not start with one empirical law, perhaps Boyle's law for gases, and then seek a theory about molecules from which this law can be derived. The scientist tries to formulate a much more general theory from which a variety of empirical laws can be derived. The more such laws, the greater their variety and apparent lack of connection with one another, the stronger will be the theory that explains them. Some of these derived laws may have been known before, but the theory may also make it possible to derive new empirical laws which can be confirmed by new tests. If this is the case, it can be said that the theory made it possible to predict new empirical laws. The prediction is understood in a hypothetical way. If the theory holds, certain empirical laws will also hold. The predicted empirical law speaks about relations between observables, so it is now possible to make experiments to see if the empirical law holds. If the empirical law is confirmed, it provides indirect confirmation of the theory. Every confirmation of a law, empirical or theoretical, is, of course, only partial, never complete and absolute. But in the case of empirical laws, it is a more direct confirmation. The confirmation of a theoretical law is indirect, because it takes place only through the confirmation of empirical laws derived from the theory.

The supreme value of a new theory is its power to predict new empirical laws. It is true that it also has value in explaining known empirical laws, but this is a minor value. If a scientist proposes a new theoretical system, from which no new laws can be derived, then it is logically equivalent to the set of all known empirical laws. The theory may have a certain elegance, and it may simplify to some degree the set of all known laws, although it is not likely that there would be an essential simplification. On the other hand, every new theory in physics that has led to a great leap forward has been a theory from which new empirical laws could be derived. If Einstein had done no more than propose his theory of relativity as an elegant new theory that would embrace certain known laws—perhaps also simplify them to a certain degree—then his theory would not have had such a revolutionary effect.

Of course it was quite otherwise. The theory of relativity led to new empirical laws which explained for the first time such phenomena as the movement of the perihelion of Mercury, and the bending of light rays in the neighborhood of the sun. These predictions showed that relativity theory was more than just a new way of expressing the old laws. Indeed, it was a theory of great predictive power. The consequences that can be derived from Einstein's theory are far from being exhausted. These are consequences that could not have been derived from earlier theories. Usually a theory of such power does have an elegance, and a unifying effect on known laws. It is simpler than the total collection of known laws. But the great value of the theory lies in its power to suggest new laws that can be confirmed by empirical means.

II. Correspondence Rules

An important qualification must now be added to the discussion of theoretical laws and terms given in section I. The statement that empirical laws are derived from theoretical laws is an oversimplification. It is not possible to derive them directly because a theoretical law contains theoretical terms, whereas an empirical law contains only observable terms. This prevents any direct deduction of an empirical law from a theoretical one.

To understand this, imagine that we are back in the nineteenth century, preparing to state for the first time some theoretical laws about molecules in a gas. These laws are to describe the number of molecules per unit volume of the gas, the molecular velocities, and so forth. To simplify matters, we assume that all the molecules have the same velocity. (This was indeed the original assumption; later it was abandoned in favor of a certain probability distribution of velocities.) Further assumptions must be made about what happens when molecules collide. We do not know the exact shape of molecules, so let us suppose that they are tiny spheres. How do spheres collide? There are laws about colliding spheres, but they concern large bodies. Since we cannot directly observe molecules, we assume their collisions are analogous to those of large bodies; perhaps they behave like perfect billiard balls on a frictionless table. These are, of course, only assumptions; guesses suggested by analogies with known macrolaws.

But now we come up against a difficult problem. Our theoretical laws deal exclusively with the behavior of molecules, which cannot be seen. How, therefore, can we deduce from such laws a law about observable properties such as the pressure or temperature of a gas or properties of sound waves that pass through the gas? The theoretical laws contain only theoretical terms. What we seek are empirical laws containing observable terms. Obviously, such laws cannot be derived without having something else given in addition to the theoretical laws.

The something else that must be given is this: a set of rules connecting the theoretical terms with the observable terms. Scientists and philosophers of science have long recognized the need for such a set of rules, and their nature has often been discussed. An example of such a rule is: "If there is an electromagnetic oscillation of a specified frequency, then there is a visible greenish-blue color of a certain hue." Here something observable is connected with a nonobservable microprocess.

Another example is: "The temperature (measured by a thermometer and, therefore, an observable in the wider sense explained earlier) of a gas is proportional to the mean kinetic energy of its molecules." This rule connects a nonobservable in molecular theory, the kinetic energy of molecules, with an observable, the temperature of the gas. If statements of this kind did not exist, there would be no way of deriving empirical laws about observables from theoretical laws about nonobservables. Different writers have different names for these rules. I call them "correspondence rules." P. W. Bridgman calls them operational rules. Norman R. Campbell speaks of them as the "Dictionary."[1] Since the rules connect a term in one terminology with a term in another terminology, the use of the rules is analogous to the use of a French-English dictionary. What does the French word *cheval* mean? You look it up in the dictionary and find that it means "horse." It is not really that simple when a set of rules is used for connecting nonobservables with observables; nevertheless, there is an analogy here that makes Campbell's "Dictionary" a suggestive name for the set of rules.

There is a temptation at times to think that the set of rules provides a means for defining theoretical terms, whereas just the opposite is really true. A theoretical term can never be explicitly defined on the basis of observable terms, although sometimes an observable can be defined in theoretical terms. For example, "iron" can be defined as a substance consisting of small crystalline parts, each having a certain arrangement of atoms and each atom being a configuration of particles of a certain type. In theoretical terms then, it is possible to express what is meant by the observable term "iron," but the reverse is not true.

There is no answer to the question: "Exactly what is an electron?" Later we shall come back to this question, because it is the kind that philosophers are always asking scientists. They want the physicist to tell them just what he means by "electricity," "magnetism," "gravity," "a molecule." If the physicist explains them in theoretical terms, the philosopher may be disappointed. "That is not what I meant at all," he will say. "I want you to tell me, in ordinary language, what those terms mean." Sometimes the philosopher writes a book in which he talks about the great mysteries of nature. "No one," he writes, "has been able so far, and perhaps no one ever will be able, to give us a straightforward answer to the question: 'What is electricity?' And so electricity remains forever one of the great, unfathomable mysteries of the universe."

There is no special mystery here. There is only an improperly phrased question. Definitions that cannot, in the nature of the case, be given, should not be demanded. If a child does not know what an elephant is, we can tell him it is a huge animal with big ears and a long trunk. We can show him a picture of an elephant. It serves admirably to define an elephant in observable terms that a child can understand. By analogy, there is a temptation to believe that, when a scientist introduces theoretical terms, he should also be able to define them in familiar terms. But this is not possible. There is no way a physicist can show us a picture of electricity in the way he can show his child a picture of an elephant. Even the cell of an organism, although it cannot be seen with the unaided eye, can be represented by a picture because the cell can be seen when it is viewed through a microscope. But we do not possess a picture of the electron. We cannot say how it looks or how it feels, because it cannot be seen or touched. The best we can do is to say that it is an extremely small body that behaves in a certain manner. This may seem to be analogous to our description of an elephant. We can describe an elephant as a large animal that behaves in a certain manner. Why not do the same with an electron?

The answer is that a physicist can describe the behavior of an electron only by stating theoretical laws, and these laws contain only theoretical terms. They describe the field produced by an electron, the reaction of an electron to a field, and so on. If an electron is in an electrostatic field, its velocity will accelerate in a certain way. Unfortunately, the electron's acceleration is an unobservable. It is not like the acceleration of a billiard ball, which can be studied by direct observation. There is no way that a theoretical concept can be defined in terms of observables. We must, therefore, resign ourselves to the fact that definitions of the kind that can be supplied for observable terms cannot be formulated for theoretical terms.

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It is true that some authors, including Bridgman, have spoken of the rules as "operational definitions." Bridgman had a certain justification, because he used his rules in a somewhat different way, I believe, than most physicists use them. He was a great physicist and was certainly aware of his departure from the usual use of rules, but he was willing to accept certain forms of speech that are not customary, and this explains his departure. It was pointed out . . . that Bridgman preferred to say that there is not just one concept of intensity of electric current, but a dozen concepts. Each procedure by which a magnitude can be measured provides an operational definition for that magnitude. Since there are different procedures for measuring current, there are different concepts. For the sake of convenience, the physicist speaks of just one concept of current. Strictly speaking, Bridgman believed, he should recognize many different concepts, each defined by a different operational procedure of measurement.

We are faced here with a choice between two different physical languages. If the customary procedure among physicists is followed, the various concepts of current will be replaced by one concept. This means, however, that you place the concept in your theoretical laws, because the operational rules are just correspondence rules, as I call them, which connect the theoretical terms with the empirical ones. Any claim to possessing a definition—that is, an operational definition—of the theoretical concept must be given up. Bridgman could speak of having operational definitions for his theoretical terms only because he was not speaking of a general concept. He was speaking of partial concepts, each defined by a different empirical procedure.

Even in Bridgman's terminology, the question of whether his partial concepts can be adequately defined by operational rules is problematic. Reichenbach speaks often of what he calls "correlative definitions." . . . Perhaps correlation is a better term than definition for what Bridgman's rules actually do. In geometry, for instance, Reichenbach points out that the axiom system of geometry, as developed by David Hilbert, for example, is an uninterpreted axiom system. The basic concepts of point, line, and plane could just as well be called "class alpha," "class beta," and "class gamma." We must not be seduced by the sound of familiar words, such as "point" and "line," into thinking they must be taken in their ordinary meaning. In the axiom system, they are uninterpreted terms. But when geometry is applied to physics, these terms must be connected with something in the physical world. We can say, for example, that the lines of the geometry are exemplified by rays of light in a vacuum or by stretched cords. In order to connect the uninterpreted terms with observable physical phenomena, we must have rules for establishing the connection.

What we call these rules is, of course, only a terminological question; we should be cautious and not speak of them as definitions. They are not definitions in any strict sense. We cannot give a really adequate definition of the geometrical concept of "line" by referring to anything in nature. Light rays, stretched strings, and so on are only approximately straight; moreover, they are not lines, but only segments of lines. In geometry, a line is infinite in length and absolutely straight. Neither property is exhibited by any phenomenon in nature. For that reason, it is not possible to give an operational definition, in the strict sense of the word, of concepts in theoretical geometry. The same is true of all the other theoretical concepts of physics. Strictly speaking, there are no "definitions" of such concepts. I prefer not to speak of "operational definitions" or even to use Reichenbach's term "correlative definitions." In my publications (only in recent years have I written about this question), I have called them "rules of correspondence" or, more simply, "correspondence rules."

Campbell and other authors often speak of the entities in theoretical physics as mathematical entities. They mean by this that the entities are related to each other in ways that can be expressed by mathematical functions. But they are not mathematical entities of the sort that can be defined in pure mathematics. In pure mathematics, it is possible to define various kinds of numbers, the function of logarithm, the exponential function, and so forth. It is not possible, however, to define such terms as "electron" and "temperature" by pure mathematics. Physical terms can be introduced only with the help of nonlogical constants, based on observations of the actual world. Here we have an essential difference between an axiomatic system in mathematics and an axiomatic system in physics.

If we wish to give an interpretation of a term in a mathematical axiom system, we can do it by giving a definition in logic. Consider, for example, the term "number" as it is used in Peano's axiom system. We can define it in logical terms, by the Frege-Russell method, for example. In this way the concept of "number" acquires a complete, explicit definition on the basis of pure logic. There is no need to establish a connection between the number 5 and such observables as "blue" and "hot." The terms have only a logical interpretation; no connection with the actual world is needed. Sometimes an axiom system in mathematics is called a theory. Mathematicians speak of set theory, group theory, matrix theory, probability theory. Here the word "theory" is used in a purely analytic way. It denotes a deductive system that makes no reference to the actual world. We must always bear in mind that such a use of the word "theory" is entirely different from its use in reference to empirical theories such as relativity theory, quantum theory, psychoanalytical theory, and Keynesian economic theory.

A postulate system in physics cannot have, as mathematical theories have, a splendid isolation from the world. Its axiomatic terms "electron," "field," and so on—must be interpreted by correspondence rules that connect the terms with observable phenomena. This interpretation is necessarily incomplete. Because it is always incomplete, the system is left open to make it possible to add new rules of correspondence. Indeed, this is what continually happens in the history of physics. I am not thinking now of a revolution in physics, in which an entirely new theory is developed, but of less radical changes that modify existing theories. Nineteenth-century physics provides a good example, because classical mechanics and electromagnetics had been established, and, for many decades, there was relatively little change in fundamental laws. The basic theories of physics remained unchanged. There was, however, a steady addition of new correspondence rules, because new procedures were continually being developed for measuring this or that magnitude.

Of course, physicists always face the danger that they may develop correspondence rules that will be incompatible with each other or with the theoretical laws. As long as such incompatibility does not occur, however, they are free to add new correspondence rules. The procedure is never-ending. There is always the possibility of adding new rules, thereby increasing the amount of interpretation specified for the theoretical terms; but no matter how much this is increased, the interpretation is never final. In a mathematical system, it is otherwise. There a logical interpretation of an axiomatic term is complete. Here we find another reason for reluctance in speaking of theoretical terms as "defined" by correspondence rules. It tends to blur the important distinction between the nature of an axiom system in pure mathematics and one in theoretical physics.

Is it not possible to interpret a theoretical term by correspondence rules so completely that no further interpretation would be possible? Perhaps the actual world is limited in its structure and laws. Eventually a point may be reached beyond which there will be no room for strengthening the interpretation of a term by new correspondence rules. Would not the rules then provide a final, explicit definition for the term? Yes, but then the term would no longer be theoretical. It would become part of the observation language. The history of physics has not yet indicated that physics will become complete; there has been only a steady addition of new correspondence rules and a continual modification in the interpretations of theoretical terms. There is no way of knowing whether this is an infinite process or whether it will eventually come to some sort of end.

It may be looked at this way. There is no prohibition in physics against making the correspondence rules for a term so strong that the term becomes explicitly defined and therefore ceases to be theoretical. Neither is there any basis for assuming that it will always be possible to add new correspondence rules. Because the history of physics has shown such a steady, unceasing modification of theoretical concepts, most physicists would advise against correspondence rules so strong that a theoretical term becomes explicitly defined. Moreover, it is a wholly unnecessary procedure. Nothing is gained by it. It may even have the adverse effect of blocking progress.

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Of course, here again we must recognize that the distinction between observables and nonobservables is a matter of degree. We might give an explicit definition, by empirical procedures, to a concept such as length, because it is so easily and directly measured, and is unlikely to be modified by new observations. But it would be rash to seek such strong correspondence rules that "electron" would be explicitly defined. The concept "electron" is so far removed from simple, direct observations that it is best to keep it theoretical, open to modifications by new observations.

III. How New Empirical Laws Are Derived from Theoretical Laws

. . . The [previous] discussion concerned the ways in which correspondence rules are used for linking the nonobservable terms of a theory with the observable terms of empirical laws. This can be made clearer by a few examples of the manner in which empirical laws have actually been derived from the laws of a theory.

The first example concerns the kinetic theory of gases. Its model, or schematic picture, is one of small particles called molecules, all in constant agitation. In its original form, the theory regarded these particles as little balls, all having the same mass and, when the temperature of the gas is constant, the same constant velocity. Later it was discovered that the gas would not be in a stable state if each particle had the same velocity; it was necessary to find a certain probability distribution of velocities that would remain stable. This was called the Boltzmann-Maxwell distribution. According to this distribution, there was a certain probability that any molecule would be within a certain range on the velocity scale.

When the kinetic theory was first developed, many of the magnitudes occurring in the laws of the theory were not known. No one knew the mass of a molecule, or how many molecules a cubic centimeter of gas at a certain temperature and pressure would contain. These magnitudes were expressed by certain parameters written into the laws. After the equations were formulated, a dictionary of correspondence rules was prepared. These correspondence rules connected the theoretical terms with observable phenomena in a way that made it possible to determine indirectly the values of the parameters in the equations. This, in turn, made it possible to derive empirical laws. One correspondence rule states that the temperature of the gas corresponds to the mean kinetic energy of the molecules. Another correspondence rule connects the pressure of the gas with the impact of molecules on the confining wall of a vessel. Although this is a continuous process involving discrete molecules, the total effect can be regarded as a constant force pressing on the wall. Thus, by means of correspondence rules, the pressure that is measured macroscopically by a manometer (pressure gauge) can be expressed in terms of the statistical mechanics of molecules.

What is the density of the gas? Density is mass per unit volume, but how do we measure the mass of a molecule? Again our dictionary—a very simple dictionary—supplies the correspondence rule. The total mass M of the gas is the sum of the masses m of the molecules. M is observable (we simply weigh the gas), but m is theoretical. The dictionary of correspondence rules gives the connection between the two concepts. With the aid of this dictionary, empirical tests of various laws derived from our theory are possible. On the basis of the theory, it is possible to calculate what will happen to the pressure of the gas when its volume remains constant and its temperature is increased. We can calculate what will happen to a sound wave produced by striking the side of the vessel, and what will happen if only part of the gas is heated. These theoretical laws are worked out in terms of various parameters that occur within the equations of the theory. The dictionary of correspondence rules enables us to express these equations as empirical laws, in which concepts are measurable, so that empirical procedures can supply values for the parameters. If the empirical laws can be confirmed, this provides indirect confirmation of the theory. Many of the empirical laws for gases were known, of course, before the kinetic theory was developed. For these laws, the theory provided an explanation. In addition, the theory led to previously unknown empirical laws.

The power of a theory to predict new empirical laws is strikingly exemplified by the theory of electromagnetism, which was developed about 1860 by two great physicists, Michael Faraday and James Clerk Maxwell. (Faraday did most of the experimental work, and Maxwell did most of the mathematical work.) The theory dealt with electric charges and how they behaved in electrical and magnetic fields. The concept of the electron—a tiny particle with an elementary electric charge—was not formulated until the very end of the century. Maxwell's famous set of differential equations, for describing electromagnetic fields, presupposed only small discrete bodies of unknown nature, capable of carrying an electric charge or a magnetic pole. What happens when a current moves along a copper wire? The theory's dictionary made this observable phenomenon correspond to the actual movement along the wire of little charged bodies. From Maxwell's theoretical model, it became possible (with the help of correspondence rules, of course) to derive many of the known laws of electricity and magnetism.

The model did much more than this. There was a certain parameter c in Maxwell's equations. According to his model, a disturbance in an electromagnetic field would be propagated by waves having the velocity c. Electrical experiments showed the value of c to be approximately 3×10^{10} centimeters per second. This was the same as the known value for the speed of light, and it seemed unlikely that it was an accident. Is it possible, physicists asked themselves, that light is simply a special case of the propagation of an electromagnetic oscillation? It was not long before Maxwell's equations were providing explanations for all sorts of optical laws, including refraction, the velocity of light in different media, and many others.

Physicists would have been pleased enough to find that Maxwell's model explained known electrical and magnetic laws; but they received a double bounty. The theory also explained optical laws! Finally, the great strength of the new model was revealed in its power to predict, to formulate empirical laws that had not been previously known.

The first instance was provided by Heinrich Hertz, the German physicist. About 1890, he began his famous experiments to see whether electromagnetic waves of low frequency could be produced and detected in the laboratory. Light is an electromagnetic oscillation and propagation of waves at very high frequency. But Maxwell's laws made it possible for such waves to have *any* frequency. Hertz's experiments resulted in his discovery of what at first were called Hertz waves. They are now called radio waves. At first, Hertz was able to transmit these waves from one oscillator to another over only a small distance—first a few centimeters, then a meter or more. Today a radio broadcasting station sends its waves many thousands of miles.

The discovery of radio waves was only the beginning of the derivation of new laws from Maxwell's theoretical model. X-rays were discovered and were thought to be particles of enormous velocity and penetrative power. Then it occurred to physicists that, like light and radio waves, these might be electromagnetic waves, but of extremely high frequency, much higher than the frequency of visible light. This also was later confirmed, and laws dealing with X-rays were derived from Maxwell's fundamental field equations. X-rays proved to be waves of a certain frequency range within the much broader frequency band of gamma rays. The X-rays used today in medicine are simply gamma rays of certain frequency. All this was largely predictable on the basis of Maxwell's model. His theoretical laws, together with the correspondence rules, led to an enormous variety of new empirical laws.

The great variety of fields in which experimental confirmation was found contributed especially to the strong overall confirmation of Maxwell's theory. The various branches of physics had originally developed for practical reasons; in most cases, the divisions were based on our different sense organs. Because the eyes perceive light and color, we call such phenomena optics; because our ears hear sounds, we call a branch of physics acoustics; and because our bodies feel heat, we have a theory of heat. We find it useful to construct simple machines based on the movements of bodies, and we call it mechanics. Other phenomena, such as electricity and magnetism, cannot be directly perceived, but their consequences can be observed.

In the history of physics, it is always a big step forward when one branch of physics can be explained by another. Acoustics, for instance, was found to be only a part of mechanics, because sound waves are simply elasticity waves in solids, liquids, and gases. We have already spoken of how the laws of gases were explained by the mechanics of moving molecules. Maxwell's theory was another great leap forward toward the unification of physics. Optics was found to be a part of electromagnetic theory. Slowly the notion grew that the whole of physics might some day be unified by one great theory. At present there is an enormous gap between electromagnetism on the one side and gravitation on the other. Einstein made several attempts to develop a unified field theory that might close this gap; more recently, Heisenberg and others have made similar attempts. So far, however, no theory has been devised that is entirely satisfactory or that provides new empirical laws capable of being confirmed.

Physics originally began as a descriptive macrophysics, containing an enormous number of empirical laws with no apparent connections. In the beginning of a science, scientists may be very proud to have discovered hundreds of laws. But, as the laws proliferate, they become unhappy with this state of affairs; they begin to search for unifying principles. In the nineteenth century, there was considerable controversy over the question of underlying principles. Some felt that science must find such principles, because otherwise it would be no more than a description of nature, not a real explanation. Others thought that that was the wrong approach, that underlying principles belong only to metaphysics. They felt that the scientist's task is merely to describe, to find out *how* natural phenomena occur, not *why*.

Today we smile a bit about the great controversy over description versus explanation. We can see that there was something to be said for both sides, but that their way of debating the question was futile. There is no real opposition between explanation and description. Of course, if description is taken in the narrowest sense, as merely describing what a certain scientist did on a certain day with certain materials, then the opponents of mere description were quite right in asking for more, for a real explanation. But today we see that description in the broader sense, that of placing phenomena in the context of more general laws, provides the only type of explanation that can be given for phenomena. Similarly, if the proponents of explanation mean a metaphysical explanation, not grounded in empirical procedures, then their opponents were correct in insisting that science should be concerned only with description. Each side had a valid point. Both description and explanation, rightly understood, are essential aspects of science.

The first efforts at explanation, those of the Ionian natural philosophers, were certainly partly metaphysical; the world is all fire, or all water, or all change. Those early efforts at scientific explanation can be viewed in two different ways. We can say: "This is not science, but pure metaphysics. There is no possibility of confirmation, no correspondence rules for connecting the theory with observable phenomena." On the other hand, we can say: "These Ionian theories are certainly not scientific, but at least they are pictorial visions of theories. They are the first primitive beginnings of science."

It must not be forgotten that, both in the history of science and in the psychological history of a creative scientist, a theory has often first appeared as a kind of visualization, a vision that comes as an inspiration to a scientist long before he has discovered correspondence rules that may help in confirming his theory. When Democritus said that everything consists of atoms, he certainly had not the slightest confirmation for this theory. Nevertheless, it was a stroke of genius, a profound insight, because two thousand years later his vision was confirmed. We should not, therefore, reject too rashly any anticipatory vision of a theory, provided it is one that may be tested at some future time. We are on solid ground, however, if we issue the warning that no hypothesis can claim to be scientific unless there is the *possibility* that it can be tested. It does not have to be confirmed to be a hypothesis, but there must be correspondence rules that will permit, in principle, a means of confirming or disconfirming the theory. It may be enormously difficult to think of experiments that can test the theory; this is the case today with various unified field theories that have been proposed. But if such tests are possible in principle, the theory can be called a scientific one. When a theory is first proposed, we should not demand more than this.

The development of science from early philosophy was a gradual, step-by-step process. The Ionian philosophers had only the most primitive theories. In contrast, the thinking of Aristotle was much clearer and on more solid scientific ground. He made experiments, and he knew the importance of experiments, although in other respects he was an apriorist. This was the beginning of science. But it was not until the time of Galileo Galilei, about 1600, that a really great emphasis was placed on the experimental method in preference to aprioristic reasoning about nature. Even though many of Galileo's concepts had previously been stated as theoretical concepts, he was the first to place theoretical physics on a solid empirical foundation. Certainly Newton's physics (about 1670) exhibits the first comprehensive, systematic theory, containing unobservables as theoretical concepts: the universal force of gravitation, a general concept of mass, theoretical properties of light rays, and so on. His theory of gravity was one of great generality. Between any two particles, small or large, there is a force proportional to the square of the distance between them. Before Newton advanced this theory, science provided no explanation that applied to both the fall of a stone and the movements of planets around the sun.

It is very easy for us today to remark how strange it was that it never occurred to anyone before Newton that the same force might cause the apple to drop and the moon to go around the earth. In fact, this was not a thought likely to occur to anyone. It is not that the *answer* was so difficult to give; it is that nobody had asked the *question.* This is a vital point. No one had asked: "What is the relation between the force that heavenly bodies exert upon each other and terrestrial forces that cause objects to fall to the ground?" Even to speak in such terms as "terrestrial" and "heavenly" is to make a bipartition, to cut nature into two fundamentally different regions. It was Newton's great insight to break away from this division, to assert that there is no such fundamental cleavage. There is one nature, one world. Newton's universal law of gravitation was the theoretical law that explained for the first time both the fall of an apple and Kepler's laws for the movements of planets. In Newton's day, it was a psychologically difficult, extremely daring adventure to think in such general terms.

Later, of course, by means of correspondence rules, scientists discovered how to determine the masses of astronomical bodies. Newton's theory also said that two apples, side by side on a table, attract each other. They do not move toward each other because the attracting force is extremely small and the friction on the table very large. Physicists eventually succeeded in actually measuring the gravitational forces between two bodies in the laboratory. They used a torsion balance consisting of a bar with a metal ball on each end, suspended at its center by a long wire attached to a high ceiling. (The longer and thinner the wire, the more easily the bar would turn.) Actually, the bar never came to an absolute rest but always oscillated a bit. But the mean point of the bar's oscillation could be established. After the exact position of the mean point was determined, a large pile of lead bricks was constructed near the bar. (Lead was used because of its great specific gravity. Gold had an even higher specific gravity, but gold bricks are expensive.) It was found that the mean of the oscillating bar had shifted a tiny amount to bring one of the balls on the end of the bar nearer to the lead pile. The shift was only a fraction of a millimeter, but it was enough to provide first observation of a gravitational effect between two bodies in a laboratory—an effect that had been predicted by Newton's theory of gravitation.

It had been known before Newton that apples fall to the ground and that the moon moves around the earth. Nobody before Newton could have predicted the outcome of the experiment with the torsion balance. It is a classic instance of the power of a theory to predict a new phenomenon not previously observed.

NOTE

1. See Percy W. Bridgman, *The Logic of Modern Physics* (New York: Macmillan, 1927), and Norman R. Campbell, *Physics: The Elements* (Cambridge: Cambridge University Press, 1920). Rules of correspondence are discussed by Ernest Nagel, *The Structure of Science* (New York: Harcourt, Brace, & World, 1961), pp. 97–105.

18

What Theories Are Not

Hilary Putnam

The announced topic for this symposium was the role of models in empirical science; however, in preparing for this symposium, I soon discovered that I had first to deal with a different topic, and this different topic is the one to which this paper actually will be devoted. The topic I mean is the role of *theories* in empirical science; and what I do in this paper is attack what may be called the "received view" on the role of theories—that theories are to be thought of as "partially interpreted calculi" in which only the "observation terms" are "directly interpreted" (the theoretical terms being only "partially interpreted," or, some people even say, "partially understood").

To begin, let us review this received view. The view divides the nonlogical vocabulary of science into two parts:

OBSERVATION TERMS	THEORETICAL TERMS
such terms as	such terms as
"red,"	"electron,"
"touches,"	"dream,"
"stick," etc.	"gene," etc.

The basis for the division appears to be as follows: the observation terms apply to what may be called publicly observable things and signify observable qualities of these things, while the theoretical terms correspond to the remaining unobservable qualities and things.

This division of terms into two classes is then allowed to generate a division of statements into two[1] classes as follows:

OBSERVATIONAL STATEMENTS	THEORETICAL STATEMENTS
statements containing only observation terms and logical vocabulary	statements containing theoretical terms

Lastly, a scientific theory is conceived of as an axiomatic system which may be thought of as initially uninterpreted, and which gains "empirical meaning" as a result of a specification of meaning *for the observation terms alone.* A kind of partial meaning is then thought of as drawn up to the theoretical terms, by osmosis, as it were.

One can think of many distinctions that are crying out to be made ("new" terms vs. "old" terms, technical terms vs. nontechnical ones, terms more-or-less peculiar to one science vs. terms common to many, for a start). My contention here is simply:

(1) The *problem* for which this dichotomy was invented ("how is it possible to interpret theoretical terms?") does not exist.

(2) A basic reason some people have given for introducing the dichotomy is false: namely, justification in science does *not* proceed "down" in the direction of observation terms. In fact, justification in science proceeds in any direction that may be handy—more observational assertions sometimes being justified with the aid of more theoretical ones, and vice versa. Moreover, as we shall see, while the notion of an *observation report* has some importance in the philosophy of science, such reports cannot be identified on the basis of the vocabulary they do or do not contain.

(3) In any case, whether the reasons for introducing the dichotomy were good ones or bad ones, the double distinction (observation terms–theoretical terms, observation statements–theoretical statements) presented above is, in fact, completely broken-backed. This I shall try to establish now.

In the first place, it should be noted that the dichotomy under discussion was intended as an explicative and not merely a stipulative one. That is, the words "observational" and "theoretical" are not having arbitrary new meanings bestowed upon them; rather, preëxisting uses of these words (especially in the philosophy of science) are presumably being sharpened and made clear. And, in the second place, it should be recalled that we are dealing with a double, not just a single, distinction. That is to say, part of the contention I am criticizing is that, once the distinction between observational and theoretical *terms* has been drawn as above, the distinction between theoretical statements and observational reports or assertions (in something like the sense usual in methodological discussions) can be drawn in terms of it. What I mean when I say that the dichotomy is "completely broken-backed" is this:

(A) If an "observation term" is one that cannot apply to an unobservable, then there are no observation terms.[2]

(B) Many terms that refer primarily to what Carnap would class as "unobservables" are not theoretical terms; and at least some theoretical terms refer primarily to observables.

(C) Observational reports can and frequently do contain theoretical terms.

(D) A scientific theory, properly so-called, may refer only to observables.

Really? (Darwin's theory of evolution, as originally put forward, is one example.)

To start with the notion of an "observation term": Carnap's formulation in *Testability and Meaning* [1] was that for a term to be an observation term not only must it correspond to an observable quality, but the determination whether the quality is present or not must be able to be made by the observer in a relatively short time, and with a high degree of confirmation. In his most recent authoritative publication [2], Carnap is rather brief. He writes, "the terms of V_0 [the 'observation vocabulary'—H.P.] are predicates designating observable properties of events or things (e.g., 'blue', 'hot', 'large', etc.) or observable relations between them (e.g., 'x is warmer than y', 'x is contiguous to y', etc.)" [2, p. 41]. The only other clarifying remarks I could find are the following: "The name 'observation language' may be understood in a narrower or in a wider sense; the observation language in the wider sense includes the disposition terms. In this article I take the observation language L_0 in the narrower sense" [2, p. 63]. "An observable property may be regarded as a simple special case of a testable disposition: for example, the operation for finding out whether a thing is blue or hissing or cold, consists simply in looking or listening or touching the thing, respectively. Nevertheless, *in the reconstruction of the language* [italics mine—H.P.] it seems convenient to take some properties for which the test procedure is extremely simple (as in the examples given) as directly observable, and use them as primitives in L_0" [2, p. 63].

These paragraphs reveal that Carnap, at least, thinks of observation terms as corresponding to qualities that can be detected without the aid of instruments. But always so detected? Or can an observation term refer sometimes to an observable thing and sometimes to an unobservable? While I have not been able to find any explicit statement on this point, it seems to me that writers like Carnap must be *neglecting* the fact that *all* terms—including the 'observation terms'—have at least the possibility of applying to unobservables. Thus their problem has sometimes been formulated in quasi-historical terms—"How could theoretical terms have been introduced into the language?" And the usual discussion strongly suggests that the following puzzle is meant: if we imagine a time at which people could only talk about observables (had not available any theoretical terms), how did they ever manage to *start* talking about unobservables?

It is possible that I am here doing Carnap and his followers an injustice. However, polemics aside, the following points must be emphasized:

(1) Terms referring to unobservables are *invariably explained*, in the actual history of science, with the aid of already present locutions referring to unobservables. There never was a stage of language at which it was impossible to talk about unobservables. Even a three-year-old child can understand a story about "people too little to see"[3] and not a single "theoretical term" occurs in this phrase.

(2) There is not even a single *term* of which it is true to say that it *could*

not (without changing or extending its meaning) be used to refer to unobservables. "Red," for example, was so used by Newton when he postulated that red light consists of *red corpuscles.*[4]

In short: if an "observation term" is a term which *can,* in principle, only be used to refer to observable things, then *there are no observation terms.* If, on the other hand, it is granted that locutions consisting of just observation terms can refer to unobservables, there is no longer any reason to maintain *either* that theories and speculations about the unobservable parts of the world must contain "theoretical (= nonobservation) terms" *or* that there is any general problem as to how one can introduce terms referring to unobservables. Those philosophers who find a difficulty in how we understand theoretical terms should find an equal difficulty in how we understand "red" and "smaller than."

So much for the notion of an "observation term." Of course, one may recognize the point just made—that the "observation terms" also apply, in some contexts, to unobservables—and retain the class (with a suitable warning as to how the label "observation term" is to be understood). But can we agree that the complementary class—what should be called the "nonobservation terms"—is to be labelled "theoretical terms"? No, for the identification of "theoretical terms" with "term (other than the 'disposition terms,' which are given a special place in Carnap's scheme) designating an unobservable quality" is unnatural and misleading. On the one hand, it is clearly an enormous (and, I believe, insufficiently motivated) extension of common usage to classify such terms as "angry," "loves," and so forth, as "theoretical terms" simply because they allegedly do not refer to public observables. A theoretical term, properly so-called, is one which comes from a scientific *theory* (and the almost untouched problem, in thirty years of writing about "theoretical terms" is what is *really* distinctive about such terms). In this sense (and I think it the sense important for discussions of science) "satellite" is, for example, a theoretical term (although the things it refers to are quite observable[5]) and "dislikes" clearly is not.

Our criticisms so far might be met by relabeling the first dichotomy (the dichotomy of terms) "observation vs. nonobservation," and suitably "hedging" the notion of "observation." But more serious difficulties are connected with the identification upon which the second dichotomy is based—the identification of "theoretical statements" with statements containing nonobservation ("theoretical") terms, and "observation statements" with "statements in the observational vocabulary."

That observation statements may contain theoretical terms is easy to establish. For example, it is easy to imagine a situation in which the following sentence might occur: "We also *observed* the creation of two electron-positron pairs."

This objection is sometimes dealt with by proposing to "relativize" the observation-theoretical dichotomy to the context. (Carnap, however, rejects this way out in the article we have been citing.) This proposal to "relativize" the dichotomy does not seem to me to be very helpful. In the first place, one can easily imagine a context in which "electron" would occur, in the same text, in *both* observational reports and in theoretical conclusions from those reports. (So that one would have distortions if one tried to put the term in either the "observational term" box or in the "theoretical term" box.) In the second place, for what philosophical problem or point does one require even the relativized dichotomy?

The usual answer is that sometimes a statement A (observational) is offered in support of a statement B (theoretical). Then, in order to explain why A is not itself questioned in the context, we need to be able to say that A is functioning, in that context, as an observation report. But this misses the point I have been making! I do not deny the need for some such notion as "observation report." What I deny is that the distinction between observation reports and, among other things, theoretical statements, can or should be drawn on the basis of vocabulary. In addition, a relativized dichotomy will not serve Carnap's purposes. One can hardly maintain that theoretical terms are only partially interpreted, whereas observational terms are completely interpreted, if no sharp line exists between the two classes. (Recall that Carnap takes his problem to be "reconstruction of the language," not of some isolated scientific context.) . . .

Notes

1. Sometimes a *tripartite* division is used: observation statements, theoretical statements (containing *only* theoretical terms). and "mixed" statements (containing both kinds of terms). This refinement is not considered here, because it avoids none of the objections presented below.

2. I neglect the possibility of trivially constructing terms that refer only to observables: namely, by conjoining "and is an observable thing" to terms that would otherwise apply to some unobservables. "Being an observable thing" is, in a sense, highly theoretical and yet applies only to observables!

3. Von Wright has suggested (in conversation) that this is an *extended* use of language (because we first learn words like "people" in connection with people we *can* see). This argument from "The way we learn to use the word" appears to be unsound however (cf. [4]).

4. Some authors (although not Carnap) explain the intelligibility of such discourse in terms of logically possible submicroscopic observers. But (a) such observers could not see single photons (or light corpuscles) even on Newton's theory; and (b) once such physically impossible (though logically possible) "observers" are introduced, why not go further and have observers with sense organs for electric charge, or the curvature of space, et cetera! Presumably because *we* can see *red,* but not *charge.* But then, this just makes the point that we *understand* "red" even when applied outside our normal "range," even though we learn it ostensively, without *explaining* that fact. (The explanation lies in this: that understanding any term—even "red"—involves at least two elements: internalizing the syntax of a natural language, and

acquiring a background of ideas. Overemphasis on the way "red" is *taught* has led some philosophers to misunderstand how it is *learned.*)

5. Carnap might exclude "satellite" as an observation term, on the ground that it takes a comparatively long time to verify that something is a satellite with the naked eye, even if the satellite is close to the parent body (although this could be debated). However, "satellite" cannot be excluded on the quite different ground that many satellites are too far away to see (which is the ground that first comes to mind) since the same is true of the huge majority of all *red* things.

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19

Observation

N. R. Hanson

Were the eye not attuned to the Sun,
The sun could never be seen by it.
GOETHE[1]

A

Consider two microbiologists. They look at a prepared slide; when asked what they see, they may give different answers. One sees in the cell before him a cluster of foreign matter: it is an artifact, a coagulum resulting from inadequate staining techniques. This clot has not more to do with the cell, in *vivo,* than the scars left on it by the archaeologist's spade have to do with the original shape of some Grecian urn. The other biologist identifies the clot as a cell organ, a "Golgi body." As for techniques, he argues: "The standard way of detecting a cell organ is by fixing and staining. Why single out this one technique as producing artifacts, while others disclose genuine organs?"

The controversy continues.[2] It involves the whole theory of microscopical technique; nor is it an obviously experimental issue. Yet it affects what scientists say they see. Perhaps there is a sense in which two such observers do not see the same thing, do not begin from the same data, though their eyesight is normal and they are visually aware of the same object.

Imagine these two observing a Protozoan—*Amoeba.* One sees a one-celled animal, the other a noncelled animal. The first sees *Amoeba* in all its analogies with different types of single cells: liver cells, nerve cells, epithelium cells. These have a wall, nucleus, cytoplasm, et cetera. Within this class *Amoeba* is distinguished only by its independence. The other, however, sees *Amoeba*'s homology not with single cells, but with whole animals. Like all animals *Amoeba* ingests its food, digests and assimilates it. It excretes, reproduces, and is mobile—more like a complete animal than an individual tissue cell.

This is not an experimental issue, yet it can affect experiment. What either man regards as significant questions or relevant data can be determined by whether he stresses the first or the last term in "unicellular animal."[3]

Some philosophers have a formula ready for such situations: "Of course they see the same thing. They make the same observation since they begin from the same visual data. But they interpret what they see differently. They construe the evidence in different ways."[4] The task is then to show how these data are molded by different theories or interpretations or intellectual constructions.

Considerable philosophers have wrestled with this task. But in fact the formula they start from is too simple to allow a grasp of the nature of observation within physics. Perhaps the scientists cited above do not begin their inquiries from the same data, do not make the same observations, do not even see the same thing? Here many concepts run together. We must proceed carefully, for wherever it makes sense to say that two scientists looking at x do not see the same thing, there must always be a prior sense in which they do see the same thing. The issue is, then, "Which of these senses is most illuminating for the understanding of observational physics?"

These biological examples are too complex. Let us consider Johannes Kepler: imagine him on a hill watching the dawn. With him is Tycho Brahe. Kepler regarded the sun as fixed: it was the earth that moved. But Tycho followed Ptolemy and Aristotle in this much at least: the earth was fixed and all other celestial bodies moved around it. *Do Kepler and Tycho see the same thing in the east at dawn*?

We might think this an experimental or observational question, unlike the questions "Are there Golgi bodies?" and "Are Protozoa one-celled or non-celled?" Not so in the sixteenth and seventeenth centuries. Thus Galileo said to the Ptolemaist ". . . neither Aristotle nor you can prove that the earth is de facto the centre of the universe. . . ."[5] "Do Kepler and Tycho see the same thing in the east at dawn?" is perhaps not a de facto question either, but rather the beginning of an examination of the concepts of seeing and observation.

The resultant discussion might run

"Yes, they do."

"No, they don't."

"Yes, they do!"

"No, they don't!' . . .

That this is possible suggests that there may be reasons for both contentions.[6] Let us consider some points in support of the affirmative answer.

The physical processes involved when Kepler and Tycho watch the dawn are worth noting. Identical photons are emitted from the sun; these traverse solar space, and our atmosphere. The two astronomers have normal vision; hence these photons pass through the cornea, aqueous humor, iris, lens, and vitreous body of their eyes in the same way. Finally their retinas are affected. Similar electrochemical changes occur in their selenium cells. The same configuration is et ceterahed on Kepler's retina as on Tycho's. So they see the same thing.

Locke sometimes spoke of seeing in this way: a man sees the sun if his is a normally-formed retinal picture of the sun. Dr. Sir W. Russell Brain speaks of our retinal sensations as indicators and signals. Everything taking place behind the retina is, as he says, "an intellectual operation based largely on non-visual experience. . . ."[7] What we see are the changes in the *tunica retina*. Dr. Ida Mann regards the macula of the eye as itself "seeing details in bright light," and the rods as "seeing approaching motor-cars." Dr. Agnes Arber speaks of the eye as itself seeing.[8] Often, talk of seeing can direct attention to the retina. Normal people are distinguished from those for whom no retinal pictures can form: we may say of the former that they can see whilst the latter cannot see. Reporting when a certain red dot can be seen may supply the oculist with direct information about the condition of one's retina.[9]

This need not be pursued, however. These writers speak carelessly: seeing the sun is not seeing retinal pictures of the sun. The retinal images which Kepler and Tycho have are four in number, inverted and quite tiny.[10] Astronomers cannot be referring to these when they say they see the sun. If they are hypnotized, drugged, drunk, or distracted they may not see the sun, even though their retinas register its image in exactly the same way as usual.

Seeing is an experience. A retinal reaction is only a physical state—a photochemical excitation. Physiologists have not always appreciated the differences between experiences and physical states.[11] People, not their eyes, see. Cameras, and eyeballs, are blind. Attempts to locate within the organs of sight (or within the neurological reticulum behind the eyes) some nameable called "seeing" may be dismissed. That Kepler and Tycho do, or do not, see the same thing cannot be supported by reference to the physical states of their retinas, optic nerves, or visual cortices: there is more to seeing than meets the eyeball.

Naturally, Tycho and Kepler see the same physical object. They are both visually aware of the sun. If they are put into a dark room and asked to report when they see something—anything at all—they may both report the same object at the same time. Suppose that the only object to be seen is a certain lead cylinder. Both men see the same thing: namely this object—whatever it is. It is just here, however, that the difficulty arises, for while Tycho sees a mere pipe, Kepler will see a telescope, the instrument about which Galileo has written to him.

Unless both are visually aware of the same object there can be nothing of philosophical interest in the question whether or not they see the same thing. Unless they both see the sun in this prior sense our question cannot even strike a spark.

Nonetheless, both Tycho and Kepler have a common visual experience of some sort. This experience perhaps constitutes their seeing the same thing.

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Locke sometimes spoke of seeing in this way: a man sees the sun if his is a normally-formed retinal picture of the sun. Dr. Sir W. Russell Brain speaks of our retinal sensations as indicators and signals. Everything taking place behind the retina is, as he says, "an intellectual operation based largely on non-visual experience. . . ."[7] What we see are the changes in the *tunica retina*. Dr. Ida Mann regards the macula of the eye as itself "seeing details in bright light," and the rods as "seeing approaching motor-cars." Dr. Agnes Arber speaks of the eye as itself seeing.[8] Often, talk of seeing can direct attention to the retina. Normal people are distinguished from those for whom no retinal pictures can form: we may say of the former that they can see whilst the latter cannot see. Reporting when a certain red dot can be seen may supply the oculist with direct information about the condition of one's retina.[9]

This need not be pursued, however. These writers speak carelessly: seeing the sun is not seeing retinal pictures of the sun. The retinal images which Kepler and Tycho have are four in number, inverted and quite tiny.[10] Astronomers cannot be referring to these when they say they see the sun. If they are hypnotized, drugged, drunk, or distracted they may not see the sun, even though their retinas register its image in exactly the same way as usual.

Seeing is an experience. A retinal reaction is only a physical state—a photochemical excitation. Physiologists have not always appreciated the differences between experiences and physical states.[11] People, not their eyes, see. Cameras, and eyeballs, are blind. Attempts to locate within the organs of sight (or within the neurological reticulum behind the eyes) some nameable called "seeing" may be dismissed. That Kepler and Tycho do, or do not, see the same thing cannot be supported by reference to the physical states of their retinas, optic nerves, or visual cortices: there is more to seeing than meets the eyeball.

Naturally, Tycho and Kepler see the same physical object. They are both visually aware of the sun. If they are put into a dark room and asked to report when they see something—anything at all—they may both report the same object at the same time. Suppose that the only object to be seen is a certain lead cylinder. Both men see the same thing: namely this object—whatever it is. It is just here, however, that the difficulty arises, for while Tycho sees a mere pipe, Kepler will see a telescope, the instrument about which Galileo has written to him.

Unless both are visually aware of the same object there can be nothing of philosophical interest in the question whether or not they see the same thing. Unless they both see the sun in this prior sense our question cannot even strike a spark.

Nonetheless, both Tycho and Kepler have a common visual experience of some sort. This experience perhaps constitutes their seeing the same thing.

Indeed, this may be a seeing logically more basic than anything expressed in the pronouncement "I see the sun" (where each means something different by "sun"). If what they meant by the word "sun" were the only clue, then Tycho and Kepler could not be seeing the same thing, even though they were gazing at the same object.

If, however, we ask, not "Do they see the same thing?" but rather "What is it that they both see?," an unambiguous answer may be forthcoming. Tycho and Kepler are both aware of a brilliant yellow-white disc in a blue expanse over a green one. Such a "sense-datum" picture is single and uninverted. To be unaware of it is not to have it. Either it dominates one's visual attention completely or it does not exist.

If Tycho and Kepler are aware of anything visual, it must be of some pattern of colors. What else could it be? We do not touch or hear with our eyes, we only take in light.[12] This private pattern is the same for both observers. Surely if asked to sket ceterah the contents of their visual fields they would both draw a kind of semicircle on a horizon-line.[13] They say they see the sun. But they do not see every side of the sun at once; so what they really see is discoid to begin with. It is but a visual aspect of the sun. In any single observation the sun is a brilliantly luminescent disc, a penny painted with radium.

So something about their visual experiences at dawn is the same for both: a brilliant yellow-white disc centered between green and blue color patches. Sket ceterahes of what they both see could be identical—congruent. In this sense Tycho and Kepler see the same thing at dawn. The sun appears to them in the same way. The same view, or scene, is presented to them both.

In fact, we often speak in this way. Thus the account of a recent solar eclipse:[14] "Only a thin crescent remains; white light is now completely obscured; the sky appears a deep blue, almost purple, and the landscape is a monochromatic green . . . there are the flashes of light on the disc's circumference and now the brilliant crescent to the left. . . ." Newton writes in a similar way in the *Opticks*: "These Arcs at their first appearance were of a violet and blue Colour, and between them were white Arcs of Circles, which . . . became a little tinged in their inward Limbs with red and yellow. . . ."[15] Every physicist employs the language of lines, color patches, appearances, shadows. Insofar as two normal observers use this language of the same event, they begin from the same data: they are making the same observation. Differences between them must arise in the interpretations they put on these data.

Thus, to summarize, saying that Kepler and Tycho see the same thing at dawn just because their eyes are similarly affected is an elementary mistake. There is a difference between a physical state and a visual experience. Suppose, however, that it is argued as above—that they see the same thing because they have the same sense-datum experience. Disparities in their accounts arise in ex post facto interpretations of what is seen, not in the fundamental visual data. If this is argued, further difficulties soon obtrude.

B

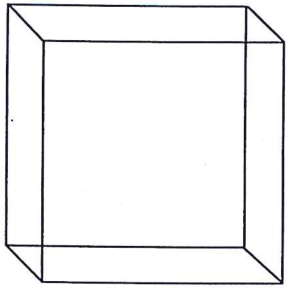


Fig.1

Normal retinas and cameras are impressed similarly by fig. 1.[16] Our visual sense-data will be the same too. If asked to draw what we see, most of us will set out a configuration like fig. 1.

Do we all see the same thing?[17] Some will see a perspex cube viewed from below. Others will see it from above. Still others will see it as a kind of polygonally-cut gem. Some people see only criss-crossed lines in a plane. It may be seen as a block of ice, an aquarium, a wire frame for a kite—or any of a number of other things.

Do we, then, all see the same thing? If we do, how can these differences be accounted for?

Here the "formula" re-enters: "These are different *interpretations* of what all observers see in common. Retinal reactions to fig. 1 are virtually identical; so too are our visual sense-data, since our drawings of what we see will have the same content. There is no place in the seeing for these differences, so they must lie in the interpretations put on what we see."

This sounds as if I do two things, not one, when I see boxes and bicycles. Do I put different interpretations on fig. 1 when I see it now as a box from below, and now as a cube from above? I am aware of no such thing. I mean no such thing when I report that the box's perspective has snapped back into the page.[18] If I do not mean this, then the concept of seeing which is natural in this connection does not designate two diaphanous components, one optical the other interpretative. Fig. 1 is simply seen now as a box from below, now as a cube from above; one does not first soak up an optical pattern and then clamp an interpretation on it. Kepler and Tycho just see the sun. That is all. That is the way the concept of seeing works in this connection.

"But," you say, "seeing fig. 1 first as a box from below, then as a cube from above, involves interpreting the lines differently in each case." Then for you and me to have a different interpretation of fig. 1 just *is* for us to see something different. This does not mean we see the same thing and then interpret it differently. When I suddenly exclaim "Eureka—a box from above," I do not refer simply to a different interpretation. (Again, there is a logically prior sense in which seeing fig. 1 as from above and then as from below is seeing the same thing differently, i.e. being aware of the same diagram in different ways. We can refer just to this, but we need not. In this case we do not.)

Besides, the word "interpretation" is occasionally useful. We know where it applies and where it does not. Thucydides presented the facts objectively; Herodotus put an interpretation on them. The word does not apply to everything—it has a meaning. Can interpreting always be going on when we see? Sometimes, perhaps, as when the hazy outline of an agricultural machine looms up on a foggy morning and, with effort, we finally identify it. Is this the "interpretation" which is active when bicycles and boxes are clearly seen? Is it active when the perspective of fig. 1 snaps into reverse? There was a time when Herodotus was half-through with his interpretation of the Greco-Persian wars. Could there be a time when one is half-through interpreting fig. 1 as a box from above, or as anything else?

"But the interpretation takes very little time—it is instantaneous." Instantaneous interpretation hails from the Limbo that produced unsensed sensibilia, unconscious inference, incorrigible statements, negative facts, and *Objektive*. These are ideas which philosophers force on the world to preserve some pet epistemological or metaphysical theory.

Only in contrast to "Eureka" situations (like perspective reversals, where one cannot interpret the data) is it clear what is meant by saying that though Thucydides could have put an interpretation on history, he did not. Moreover, whether or not an historian is advancing an interpretation is an empirical question: we know what would count as evidence one way or the other. But whether we are employing an interpretation when we see fig. 1 in a certain way is not empirical. What could count as evidence? In no ordinary sense of "interpret" do I interpret fig. 1 differently when its perspective reverses for me. If there is some extraordinary sense of that word it is not clear, either in ordinary language, or in extraordinary (philosophical) language. To insist that different reactions to fig. 1 *must* lie in the interpretations put on a common visual experience is just to reiterate (without reasons) that the seeing of *x must* be the same for all observers looking at *x*.

"But 'I see the figure as a box' means: I am having a particular visual experience which I always have when I interpret the figure as a box, or when I look at a box. . . ." ". . . if I meant this, I ought to know it. I ought to be able to refer to the experience directly and not only indirectly. . . ."[19]

Ordinary accounts of the experiences appropriate to fig. 1 do not require visual grist going into an intellectual mill: theories and interpretations are "there" in the seeing from the outset. How can interpretations "be there" in the seeing? How is it possible to see an object according to an interpretation? "The question represents it as a queer fact; as if something were being forced into a form it did not really fit. But no squeezing, no forcing took place here."[20]

Consider now the reversible perspective figures which appear in textbooks on Gestalt psychology: the tea-tray, the shifting (Schröder) staircase, the tunnel. Each of these can be seen as concave, as convex, or as a flat drawing.[21] Do I really see something different each time, or do I only interpret what I see in a different way? To interpret is to think, to do something; seeing is an experiential state.[22] The different ways in which these figures are seen are not due to different thoughts lying behind the visual reactions. What could "spontaneous" mean if these reactions are not spontaneous? When the staircase "goes into reverse" it does so spontaneously. One does not think of anything special; one does not think at all. Nor does one interpret. One just sees, now a staircase as from above, now a staircase as from below.

The sun, however, is not an entity with such variable perspective. What has all this to do with suggesting that Tycho and Kepler may see different things in the east at dawn? Certainly the cases are different. But these reversible perspective figures are examples of different things being seen in the same configuration, where this difference is due neither to differing visual pictures, nor to any "interpretation" superimposed on the sensation. . . .

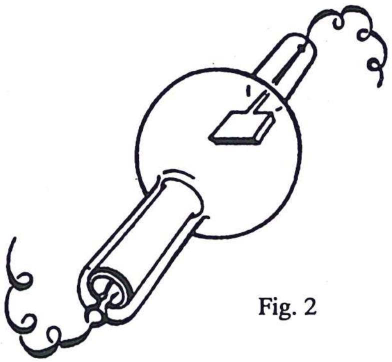


Fig. 2

A trained physicist could see one thing in fig 2: an X-ray tube viewed from the cathode. Would Sir Lawrence Bragg and an Eskimo baby see the same thing when looking at an X-ray tube? Yes, and no. Yes—they are visually aware of the same object. No—the *ways* in which they are visually aware are profoundly different. Seeing is not only the having of a visual experience; it is also the way in which the visual experience is had.

At school the physicist had gazed at this glass-and-metal instrument. Returning now, after years in university and research, his eye lights upon the same object once again. Does he see the same thing now as he did then? Now he sees the instrument in terms of electrical circuit theory, thermodynamic theory, the theories of metal and glass structure, thermionic emission, optical transmission, refraction, diffraction, atomic theory, quantum theory, and special relativity.

Contrast the freshman's view of college with that of his ancient tutor. Compare a man's first glance at the motor of his car with a similar glance ten exasperating years later.

"Granted, one learns all these things," it may be countered, "but it all figures in the interpretation the physicist puts on what he sees. Though the layman sees exactly what the physicist sees, he cannot interpret it in the same way because he has not learned so much."

Is the physicist doing more than just seeing? No, he does nothing over and above what the layman does when he sees an X-ray tube. What are you doing over and above reading these words? Are you interpreting marks on a page? When would this ever be a natural way of speaking? Would an infant see what you see here, when you see words and sentences and he sees but marks and lines? One does nothing beyond looking and seeing when one dodges bicycles, glances at a friend, or notices a cat in the garden.

"The physicist and the layman see the same thing," it is objected, "but they do not make the same thing of it." The layman can make nothing of it. Nor is that just a figure of speech. I can make nothing of the Arab word for *cat*, though my purely visual impressions may be distinguishable from those of the Arab who can. I must learn Arabic before I can see what he sees. The layman must learn physics before he can see what the physicist sees.

If one must find a paradigm case of seeing it would be better to regard as such not the visual apprehension of color patches but things like seeing what time it is, seeing what key a piece of music is written in, and seeing whether a wound is septic.[23]

Pierre Duhem writes:

> Enter a laboratory; approach the table crowded with an assortment of apparatus, an electric cell, silk-covered copper wire, small cups of mercury, spools, a mirror mounted on an iron bar; the experimenter is inserting into small openings the metal ends of ebony-headed pins; the iron oscillates, and the mirror attached to it throws a luminous band upon a celluloid scale; the forward-backward motion of this spot enables the physicist to observe the minute oscillations of the iron bar. But ask him what he is doing. Will he answer "I am studying the oscillations of an iron bar which carries a mirror?" No, he will say that he is measuring the electric resistance of the spools. If you are astonished, if you ask him what his words mean, what relation they have with the phenomena he has been observing and which you have noted at the same time as he, he will answer that your question requires a long explanation and that you should take a course in electricity.[24]

The visitor must learn some physics before he can see what the physicist sees. Only then will the context throw into relief those features of the objects before him which the physicist sees as indicating resistance.

This obtains in all seeing. Attention is rarely directed to the space between the leaves of a tree, save when a Keats brings it to our notice.[25] (Consider also what was involved in Crusoe's seeing a vacant space in the sand as a footprint.) Our attention most naturally rests on objects and events which dominate the visual field. What a blooming, buzzing, undifferentiated confusion visual life would be if we all arose tomorrow without attention capable of dwelling only on what had heretofore been overlooked.[26]

The infant and the layman can see: they are not blind. But they cannot see what the physicist sees; they are blind to what he sees.[27] We may not hear that the oboe is out of tune, though this will be painfully obvious to the trained musician. (Who, incidentally, will not hear the tones and *interpret* them as being out of tune, but will simply hear the oboe to be out of tune.[28] We simply see what time it is; the surgeon simply sees a wound to be septic; the physicist sees the X-ray tube's anode overheating.) The elements of the visitor's visual field, though identical with those of the physicist, are not organized for him as for the physicist; the same lines, colors, shapes are apprehended by both, but not in the same way. There are indefinitely many ways in which a constellation of lines, shapes, patches, may be seen. *Why* a visual pattern is seen differently is a question for psychology, but *that* it may be seen differently is important in any examination of the concepts of seeing and observation. Here, as Wittgenstein might have said, the psychological is a symbol of the logical.

You see a bird, I see an antelope; the physicist sees an X-ray tube, the child a complicated lamp bulb; the microscopist sees coelenterate mesoglea, his new student sees only a gooey, formless stuff. Tycho and Simplicius see a mobile sun, Kepler and Galileo see a static sun.[29]

It may be objected, "Everyone, whatever his state of knowledge, will see fig. 1 as a box or cube, viewed as from above or as from below." True; almost everyone, child, layman, physicist, will see the figure as box-like one way or another. But could such observations be made by people ignorant of the construction of box-like objects? No. This objection only shows that most of us —the blind, babies, and dimwits excluded—have learned enough to be able to see this figure as a three-dimensional box. This reveals something about the sense in which Simplicius and Galileo do see the same thing (which I have never denied)—they both see a brilliant heavenly body. The schoolboy and the physicist both see that the X-ray tube will smash if dropped. Examining how observers see different things in x marks something important about their seeing the same thing when looking at x. If seeing different things involves having different knowledge and theories about x, then perhaps the sense in which they see the same thing involves their sharing knowledge and theories about x. Bragg and the baby share no knowledge of X-ray tubes. They see the same thing only in that if they are looking at x they are both having some visual experience of it. Kepler and Tycho agree on more: they see the same thing in a stronger sense. Their visual fields are organized in much the same way. Neither sees the sun about to break out in a grin, or about to crack into ice cubes. (The baby is not "set" even against these eventualities.) Most people today see the same thing at dawn in an even stronger sense: we share much knowledge of the sun. Hence Tycho and Kepler see different things, and yet they see the same thing. That these things can be said depends on their knowledge, experience, and theories.

. . . The elements of their experiences are identical; but their conceptual organization is vastly different. Can their visual fields have a different organization? Then they can see different things in the east at dawn.

It is the sense in which Tycho and Kepler do not observe the same thing which must be grasped if one is to understand disagreements within microphysics. Fundamental physics is primarily a search for intelligibility—it is philosophy of matter. Only secondarily is it a search for objects and facts (though the two endeavors are as hand and glove). Microphysicists seek new modes of conceptual organization. If that can be done the finding of new entities will follow. Gold is rarely discovered by one who has not got the lay of the land.

To say that Tycho and Kepler, Simplicius and Galileo, Hooke and Newton, Priestley and Lavoisier, Soddy and Einstein, De Broglie and Born, Heisenberg and Bohm all make the same observations but use them differently is too easy.[30] It does not explain controversy in research science. Were there no sense in which they were different observations they could not be used differently. This may perplex some: that researchers sometimes do not appreciate data in the same way is a serious matter. It is important to realize, however, that sorting out differences about data, evidence, observation, may require more than simply gesturing at observable objects. It may require a comprehensive reappraisal of one's subject matter. This may be difficult, but it should not obscure the fact that nothing less than this may do. . . .

Notes

1. Wär' nicht das Auge sonnenhaft,
Die Sonne könnt' es nie erblicken;
Goethe, *Zahme Xenien* (Werke, Weimar, 1887–1918), bk. 3, 1805.

2. Cf. the papers by Baker and Gatonby in *Nature*, 1949–present.

3. This is not a merely conceptual matter, of course. Cf. Wittgenstein, *Philosophical Investigations* (Blackwell, Oxford, 1953), p. 196.

4. (1) G. Berkeley, *Essay Towards a New Theory of Vision* (in *Works*, vol. I [London, T. Nelson, 1948–56]), pp. 51 ff.

(2) James Mill, *Analysis of the Phenomena of the Human Mind* (Longmans, London, 1869), vol. 1, p. 97.

(3) J. Sully, *Outlines of Psychology* (Appleton, New York, 1885).

(4) William James, *The Principles of Psychology* (Holt, New York, 1890–1905), vol. 2, pp. 4, 78, 80 and 81; vol. 1, p. 221.

(5) A. Schopenhauer, *Satz vom Grunde* (in *Sämmtliche Werke*, Leipzig, 1888), ch. 4.

(6) H. Spencer, *The Principles of Psychology* (Appleton, New York, 1897), vol. 4, chs. 9, 10.

(7) E. von Hartmann, *Philosophy of the Unconscious* (K. Paul, London, 1931), B, chs. 7, 8.

(8) W. M. Wundt, *Vorlesungen über die Menschen und Thierseele* (Voss, Hamburg, 1892), 4, 13.

(9) H. L. F. von Helmholtz, *Handbuch der Physiologischen Optik* (Leipzig, 1867), pp. 430, 447.

(10) A. Binet, *La psychologie du raisonnement, recherches expérimentales par l'hypnotisme* (Alcan, Paris, 1886), chs. 3, 5.

(11) J. Grote, *Exploratio Philosophica* (Cambridge, 1900), vol. 2, pp. 201 ff.

(12) B. Russell, in *Mind* (1913), p. 76. *Mysticism and Logic* (Longmans, New York, 1918), p. 209. *The Problems of Philosophy* (Holt, New York, 1912), pp. 73, 92, 179, 203.

(13) Dawes Hicks, *Arist. Soc. Sup.* vol. 2 (1919), pp. 176–8.

(14) G. F. Stout, *A Manual of Psychology* (Clive, London, 1907, 2nd ed.), vol. 2, 1 and 2, pp. 324, 561–4.

(15) A. C. Ewing, *Fundamental Questions of Philosophy* (New York, 1951), pp. 45 ff.

(16) G. W. Cunningham, *Problems of Philosophy* (Holt, New York, 1924), pp. 96–7.

5. Galileo, *Dialogue Concerning the Two Chief World Systems* (California, 1953), "The First Day," p. 33.

6. "'Das ist doch kein Sehen!'—'Das ist doch ein Sehen!' Beide müssen sich begrifflich rechtfertigen lassen" (Wittgenstein, *Phil Inv.* p. 203).

7. Brain, *Recent Advances in Neurology* (with Strauss) (London, 1929), p. 88. Compare Helmholtz: "The sensations are signs to our consciousness, and it is the task of our intelligence to learn to understand their meaning" (*Handbuch der Physiologischen Optik* (Leipzig, 1867), vol. 3, p. 433).

See also Husserl, "Ideen zu einer Reinen Phaenomenologie," in *Jahrbuch für Philosophie*, vol. 1 (1913), pp. 75, 79, and Wagner's *Handwörterbuch der physiologie*, vol. 3, section 1 (1846), p. 183.

8. Mann, *The Science of Seeing* (London, 1949), pp. 48–9. Arber, *The Mind and the Eye* (Cambridge, 1954). Compare Muller: "In any field of vision, the retina sees only itself in its spatial extension during a state of affection. It perceives itself as . . . et cetera." (*Zur vergleichenden Physiologie des Gesichtesinnes des Menschen und der Thiere* (Leipzig, 1826), p. 54).

9. Kolin: "An astigmatic eye when looking at millimeter paper can accommodate to see sharply either the vertical lines or the horizontal lines" (*Physics* (New York, 1950), pp. 570 ff.).

10. Cf. Whewell, *Philosophy of Discovery* (London, 1860), "The Paradoxes of Vision."

11. Cf. e.g., J. Z. Young, *Doubt and Certainty in Science* (Oxford, 1951, The Reith Lectures), and Gray Walter's article in *Aspects of Form*, ed. by L. L. Whyte (London, 1953). Compare Newton: "Do not the Rays of Light in falling upon the bottom of the Eye excite Vibrations in the Tunica Retina? Which Vibrations, being propagated along the solid Fibres of the Nerves into the Brain, cause the Sense of seeing" (*Opticks* (London, 1769), bk. 3, part 1).

12. "Rot und grün kann ich nur sehen, aber nicht hören" (Wittgenstein, *Phil. Inv.* p. 209).

13. Cf. "An appearance is the same whenever the same eye is affected in the same way" (Lambert, *Photometria* (Berlin, 1760)); "We are justified, when different perceptions offer themselves to us, to infer that the underlying real conditions are different" (Helmholtz, *Wissenschaftliche Abhandlungen* (Leipzig, 1882), vol. 2, p. 656), and Hertz: "We form for ourselves images or symbols of the external objects; the manner in which we form them is such that the logically necessary (*denknotwendigen*) consequences of the images in thought are invariably the images of materially necessary (*naturnotwendigen*) consequences of the corresponding objects" (*Principles of Mechanics* (London, 1889). p. 1).

Broad and Price make depth a feature of the private visual pattern. However, Weyl (*Philosophy of Mathematics and Natural Science* (Princeton, 1949), p. 125) notes that a single eye perceives qualities spread out in a *two*-dimensional field, since the latter is dissected by any one-dimensional line running through it. But our conceptual difficulties remain even when Kepler and Tycho keep one eye closed.

Whether or not two observers are having the same visual sense-data reduces directly to the question of whether accurate pictures of the contents of their visual fields are identical, or differ in some detail. We can then discuss the publicly observable pictures which Tycho and Kepler draw of what they see, instead of those private, mysterious entities locked in their visual consciousness. The accurate picture and the sense-datum must be identical; how could they differ?

14. From the B.B.C. report, 30 June 1954.

15. Newton, *Opticks*, bk. 2, part 1. The writings of Claudius Ptolemy sometimes read like a phenomenalist's textbook. Cf. e.g., *The Almagest* (Venice, 1515), 6, section 2, "On the Directions in the Eclipses," "When it touches the shadow's circle from within," "When the circles touch each other from without." Cf. also 7 and 8, 9 (section 4). Ptolemy continually seeks to chart and predict "the appearances"—the points of light on the celestial globe. *The Almagest* abandons any attempt to explain the machinery behind these appearances.

Cf. Pappus: "The (circle) dividing the milk-white portion which owes its colour to the sun, and the portion which has the ashen colour natural to the moon itself is indistinguishable from a great circle" (*Mathematical Collection* (Hultsch, Berlin and Leipzig, 1864), pp. 554–60).

16. This famous illusion dates from 1832, when L. A. Necker, the Swiss naturalist, wrote a letter to Sir David Brewster describing how when certain rhomboidal crystals were viewed on end the perspective could shift in the way now familiar to us. Cf. *Phil. Mag*. 3, no. 1 (1832), 329–37, especially p. 336. It is important to the present argument to note that this observational phenomenon began life not as a psychologist's trick, but at the very frontiers of observational science.

17. Wittgenstein answers: "Denn wir sehen eben wirklich zwei verschiedene Tatsachen" (*Tractatus* 5. 5423).

18. "Auf welche Vorgänge spiele ich an?" (Wittgenstein, *Phil. Inv.*, p. 214).

19. Ibid., p. 194 (top).

20. Ibid., p. 200.

21. This is not due to eye movements, or to local retinal fatigue. Cf. Flugel, *Brit. J. Psychol.* 6 (1913), 60; *Brit. Psychol.* 5 (1913), 357. Cf. Donahue and Griffiths, *Amer. J. Psychol.* (1931), and Luckiesch, *Visual Illusions and their Applications* (London, 1922). Cf. also Peirce, *Collected Papers* (Harvard, 1931), 5, 183. References to psychology should not be misunderstood; but as one's acquaintance with the psychology of perception deepens, the character of the conceptual problems one regards as significant will deepen accordingly. Cf. Wittgenstein, *Phil. Inv.* p. 206 (top). Again, p. 193: "Its causes are of interest to psychologists. We are interested in the concept and its place among the concepts of experience."

22. Wittgenstein, *Phil. Inv.*, p. 212.

23. Often "What do you see?" only poses the question "Can you identify the object before you?" This is calculated more to test one's knowledge than one's eyesight.

24. Duhem, *La theorie physique* (Paris, 1914), p. 218.

25. Chinese poets felt the significance of "negative features" like the hollow of a clay vessel or the central vacancy of the hub of a wheel (cf. Waley, *Three Ways of Thought in Ancient China* (London, 1939), p. 155).

26. Infants are indiscriminate; they take in spaces, relations, objects, and events as being of equal value. They still must learn to organize their visual attention. The camera-clarity of their visual reactions is not by itself sufficient to differentiate elements in their visual fields. Contrast Mr. W. H. Auden who recently said of the poet that he is "bombarded by a stream of varied sensations which would drive him mad if he took them all in. It is impossible to guess how much energy we have to spend every day in not-seeing, not-hearing, not-smelling, not-reacting."

27. "He was blind to the *expression* of a face. Would his eyesight on that account be defective?" (Wittgenstein, *Phil. Inv.*, p. 210) and "Because they seeing see not; and hearing they hear not, neither do they understand" (Matt. 13: 10–13).

28. "Es hort doch jeder nur, was er versteht" (Goethe, *Maxims* (Werke, Weimar, 1887–1918)).

29. Against this Professor H. H. Price has argued: "Surely it appears to both of them to be rising, to be moving upwards, across the horizon . . . they both see a moving sun: they both see a round bright body which appears to be rising." Philip Frank retorts: "Our sense observation shows only that in the morning the distance between horizon and sun is increasing, but it does not tell us whether the sun is ascending or the horizon is descending . . ." (*Modern Science and Its Philosophy* (Harvard, 1949), p. 231). Precisely. For Galileo and Kepler the horizon drops; for Simplicius and Tycho the sun rises. This is the difference Price misses, and which is central to this essay.

30. This parallels the too-easy epistemological doctrine that all normal observers see the same things in *x*, but interpret them differently.

20

Science and the Physical World

W. T. Stace

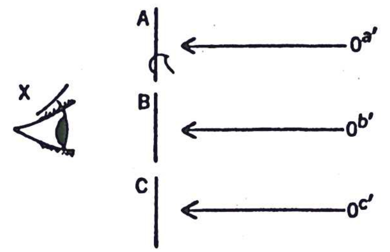
So far as I know scientists still talk about electrons, protons, neutrons, and so on. We never directly perceive these, hence if we ask how we know of their existence the only possible answer seems to be that they are an inference from what we do directly perceive. What sort of an inference? Apparently a causal inference. The atomic entities in some way impinge upon the sense of the animal organism and cause that organism to perceive the familiar world of tables, chairs, and the rest.

But is it not clear that such a concept of causation, however interpreted, is invalid? The only reason we have for believing in the law of causation is that we *observe* certain regularities or sequences. We observe that, in certain conditions, *A* is always followed by *B*. We call *A* the cause, *B* the effect. And the sequence *A-B* becomes a causal law. It follows that all *observed* causal sequences are between sensed objects in the familiar world of perception, and that all known causal laws apply solely to the world of sense and not to anything beyond or behind it. And this in turn means that we have not got, and never could have, one jot of evidence for believing that the law of causation can be applied *outside* the realm of perception, or that that realm can have any causes (such as the supposed physical objects) which are not themselves perceived.

Put the same thing in another way. Suppose there is an observed sequence *A-B-C,* represented by the vertical lines in the diagram below.

The observer X sees, and can see, nothing except things in the familiar world of perception. What *right* has he, and what *reason* has he, to assert causes of *A, B,* and *C,* such as *a′, b′, c′,* which he can never observe, behind the perceived world? He has no *right,* because the law of causation on which he is relying has never been observed to operate outside the series of perceptions, and he can have, therefore, no evidence that it does so. And he has no *reason* because the phenomenon *C* is *sufficiently* accounted for by the

cause *B, B* by *A,* and so on. It is unnecessary and superfluous to introduce a *second* cause *b′* for *B, c′* for *C,* and so forth. To give two causes for each phenomenon, one in one world and one in another, is unnecessary, and perhaps even self-contradictory.



Is it denied, then, it will be asked, that the star causes light waves, that the waves cause retinal changes, that these cause changes in the optic nerve, which in turn causes movement in the brain cells, and so on? No, it is not denied. But the observed causes and effects are all in the world of perception. And no sequences of sense-data can possibly justify going outside that world. If you admit that we never observe anything except sensed objects and their relations, regularities, and sequences, then it is obvious that we are completely shut in by our sensations and can never get outside them. Not only causal relations, but all other observed relations, upon which *any* kind of inferences might be founded, will lead only to further sensible objects and their relations. No inference, therefore, can pass from what is sensible to what is not sensible.

The fact is that atoms are *not* inferences from sensations. No one denies, of course, that a vast amount of perfectly valid inferential reasoning takes place in the physical theory of the atom. But it will not be found to be in any strict logical sense inference from *sense-data to atoms.* An *hypothesis* is set up, and the inferential processes are concerned with the application of the hypothesis, that is, with the prediction by its aid of further possible sensations and with its own internal consistency.

That atoms are not inferences from sensations means, of course, that from the existence of sensations we cannot validly infer the existence of atoms. And this means that we cannot have any reason at all to believe that they exist. And that is why I propose to argue that they do not exist—or at any rate that no one could know it if they did, and that we have absolutely no evidence of their existence.

What status have they, then? Is it meant that they are false and worthless, merely untrue? Certainly not. No one supposes that the entries in the nautical almanac "exist" anywhere except on the pages of that book and in the brains of its compilers and readers. Yet they are "true," inasmuch as they enable us to predict certain sensations, namely, the positions and times of certain perceived objects which we call the stars. And so the formulae of the atomic theory are true in the same sense, and perform a similar function.

I suggest that they are nothing but shorthand formulae, ingeniously worked out by the human mind, to enable it to predict its experience, i.e., to predict what sensations will be given to it. By "predict" here I do not mean to refer solely to the future. To calculate that there was an eclipse of the sun visible in Asia Minor in the year 585 B.C.E. is, in the sense in which I am using the term, to predict.

In order to see more clearly what is meant, let us apply the same idea to another case, that of gravitation. Newton formulated a law of gravitation in terms of "forces." It was supposed that this law—which was nothing but a mathematical formula—governed the operation of these existent forces. Nowadays it is no longer believed that these forces exist at all. And yet the law can be applied just as well without them to the prediction of astronomical phenomena. It is a matter of no importance to the scientific man whether the forces exist or not. That may be said to be a purely philosophical question. And I think the philosopher should pronounce them fictions. But that would not make the law useless or untrue. If it could still be used to predict phenomena, it would be just as true as it was.

It is true that fault is now found with Newton's law, and that another law, that of Einstein, has been substituted for it. And it is sometimes supposed that the reason for this is that forces are no longer believed in. But this is not the case. Whether forces exist or not simply does not matter. What matters is the discovery that Newton's law does *not* enable us accurately to predict certain astronomical facts such as the exact position of the planet Mercury. Therefore another formula, that of Einstein, has been substituted for it which permits correct predictions. This new law, as it happens, is a formula in terms of geometry. It is pure mathematics and nothing else. It does not contain anything about forces. In its pure form it does not even contain, so I am informed, anything about "humps and hills in space-time." And it does not matter whether any such humps and hills exist. It is truer than Newton's law, not because it substitutes humps and hills for forces, but solely because it is a more accurate formula of prediction.

Not only may it be said that forces do not exist. It may with equal truth be said that "gravitation" does not exist. Gravitation is not a "thing," but a mathematical formula, which exists only in the heads of mathematicians. And as a mathematical formula cannot cause a body to fall, so gravitation cannot cause a body to fall. Ordinary language misleads us here. We speak of the law "of" gravitation, and suppose that this law "applies to" the heavenly bodies. We are thereby misled into supposing that there are *two* things, namely, the gravitation and the heavenly bodies, and that one of these things, the gravitation, causes changes in the other. In reality nothing exists except the moving bodies. And neither Newton's law nor Einstein's law is, strictly speaking, a law of gravitation. They are both laws of moving bodies, that is to say, formulae which tell us how these bodies will move.

Now, just as in the past "forces" were foisted into Newton's law (by himself, be it said), so now certain popularizers of relativity foisted "humps and hills in space-time" into Einstein's law. We hear that the reason why the planets move in curved courses is that they cannot go through these humps and hills, but have to go round them! The planets just get "shoved about," not by forces, but by the humps and hills! But these humps and hills are pure metaphors. And anyone who takes them for "existences" gets asked awkward questions as to what "curved space" is curved "in."

It is not irrelevant to our topic to consider *why* human beings invent these metaphysical monsters of forces and bumps in space-time. The reason is that they have never emancipated themselves from the absurd idea that science "explains" things. They were not content to have laws which merely told them *that* the planets will, as a matter of fact, move in such and such ways. They wanted to know "why" the planets move in those ways. So Newton replied, "Forces." "Oh," said humanity, "that explains it. We understand forces. We feel them every time someone pushes or pulls us." Thus the movements were supposed to be "explained" by entities familiar because analogous to the muscular sensations which human beings feel. The humps and hills were introduced for exactly the same reason. They seem so familiar. If there is a bump in the billiard table, the rolling billiard ball is diverted from a straight to a curved course. Just the same with the planets. "Oh, I see!" says humanity, "that's quite simple. That *explains* everything."

But scientific laws, properly formulated, never "explain" anything. They simply state, in an abbreviated and generalized form, *what happens.* No scientist, and in my opinion no philosopher, knows *why* anything happens, or can "explain" anything. Scientific laws do nothing except state the brute fact that "when *A* happens, *B* always happens too." And laws of this kind obviously enable us to predict. If certain scientists substituted humps and hills for forces, then they have just substituted one superstition for another. For my part I do not believe that *science* has done this, though some *scientists* may have. For scientists, after all, are human beings with the same craving for "explanations" as other people.

I think that atoms are in exactly the same position as forces and the humps and hills of space-time. In reality the mathematical formulae which are the scientific ways of stating the atomic theory are simply formulae for calculating what sensations will appear in given conditions. But just as the weakness of the human mind demanded that there should correspond to the formula of gravitation a real "thing" which could be called "gravitation itself" or "force," so the same weakness demands that there should be a real thing corresponding to the atomic formulae, and this real thing is called the atom. In reality the atoms no more cause sensations than gravitation causes apples to fall. The only causes of sensations are other sensations. And the relation of atoms to sensations to be felt is not the relation of cause to effect, but the relation of a mathematical formula to the facts and happenings which it enables the mathematician to calculate.

Some writers have said that the physical world has no color, no sound, no taste, no smell. It has no spatiality. Probably it has not even number. We must not suppose that it is in any way like our world, or that we can understand it by attributing to it the characters of our world. Why not carry this progress to its logical conclusion? Why not give up the idea that it has even the character of "existence" which our familiar world has? We have given up smell, color, taste. We have given up even space and shape. We have given up number. Surely, after all that, mere existence is but a little thing to give up. No? Then is it that the idea of existence conveys "a sort of halo"? I suspect so. The "existence" of atoms is but the expiring ghost of the pellet and billiard-ball atoms of our forefathers. They, of course, had size, shape, weight, hardness. These have gone. But thinkers still cling to their existence, just as their fathers clung to the existence of forces, and for the same reason. Their reason is not in the slightest that science has any use for the existent atom. But the *imagination* has. It seems somehow to explain things, to make them homely and familiar.

It will not be out of place to give one more example to show how common fictitious existences are in science, and how little it matters whether they really exist or not. This example has no strange and annoying talk of "bent spaces" about it. One of the foundations of physics is, or used to be, the law of the conservation of energy. I do not know how far, if at all, this has been affected by the theory that matter sometimes turns into energy. But that does not affect the lesson it has for us. The law states, or used to state, that the amount of energy in the universe is always constant, that energy is never either created or destroyed. This was highly convenient, but it seemed to have obvious exceptions. If you throw a stone up into the air, you are told that it exerts in its fall the same amount of energy which it took to throw it up. But suppose it does not fall. Suppose it lodges on the roof of your house and stays there. What has happened to the energy which you can nowhere perceive as being exerted? It seems to have disappeared out of the universe. No, says the scientist, it still exists as *potential* energy. Now what does this blessed word "potential"—which is thus brought in to save the situation—mean as applied to energy? It means, of course, that the energy does not exist in any of its regular "forms," heat, light, electricity, et cetera. But this is merely negative. What positive meaning has the term? Strictly speaking, none whatever. Either the energy exists or it does not exist. There is no realm of the "potential" half-way between existence and nonexistence. And the existence of energy can only consist in its being exerted. If the energy is not being exerted, then it is not energy and does not exist. Energy can no more exist without energizing than heat can exist without being hot. The "potential" existence of the energy is, then, a fiction. The actual empirically verifiable facts are that if a certain quantity of energy *e* exists in the universe and then disappears out of the universe (as happens when the stone lodges on the roof), the same amount of energy *e* will always reappear, begin to exist again, in certain known conditions. That is the fact which the law of the conservation of energy actually expresses. And the fiction of potential energy is introduced simply because it is convenient and makes the equations easier to work. They could be worked quite well without it, but would be slightly more complicated. In either case the function of the law is the same. Its object is to apprise us that if in certain conditions we have certain perceptions (throwing up the stone), then in certain other conditions we shall get certain other perceptions (heat, light, stone hitting skull, or other such). But there will always be a temptation to hypostatize the potential energy as an "existence," and to believe that it is a "cause" which "explains" the phenomena.

If the views which I have been expressing are followed out, they will lead to the conclusion that, strictly speaking, *nothing exists except sensations* (and the minds which perceive them). The rest is mental construction or fiction. But this does not mean that the conception of a star or the conception of an electron are worthless or untrue. Their truth and value consist in their capacity for helping us to organize our experience and predict our sensations.