An Introduction to the Philosophy of Science

RUDOLPH CARNAP

EDITED BY
MARTIN GARDNER

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(continued on back flap)

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Foreword to the Dover Edition

When I was a freshman at the University of Chicago in 1932, I intended to transfer after two years to the California Institute of Technology to become a physicist. For better or worse, I got sidetracked into philosophy for my bachelor's degree. After one year of graduate work on a scholarship, I decided not to continue for the master's degree but to become a writer instead. I had a job in the university's press-relations office when I enlisted in the Navy.

Back in Chicago after four years of service as a yeoman, I used the G.I. bill in the fall of 1946 to take a seminar with Rudolf Carnap. Titled "Concepts, Theories and Methods in the Physical Sciences," it was the most exciting class I ever attended. It led me into a lifelong interest in the philosophy of science.

After each session I typed the notes I had taken and put them in a looseleaf binder, to which I added an index. Looking over the introductory page in the binder, I find a record reminding me that, during the hour prior to Carnap's class, the same room was used for a course about the "Great Books." (The university was then in its notorious Robert Hutchins–Mortimer Adler phase, which stressed the classics of the Western world.) This often left the blackboard covered with diagrams explaining some aspect of Plato's or Aristotle's metaphysics. Carnap never looked at these diagrams while he erased them. I have likened this sweep of Carnap's arm across the blackboard to his erasure of stale and meaningless metaphysics. One day, before Carnap arrived, a student did the erasing, explaining that it was "so as not to worry Carnap."

Carnap's carefully constructed sentences were delivered slowly, in a low, rich, pleasing voice, giving the impression that he was struggling to simplify ideas too complex for us to fully understand. A favorite phrase, after describing a difficult task, was, "Now how could we do that?" Other often-used phrases included "and we know this for the following reasons," "blurs the distinction," "lacks cognitive content," and "prescientific thinking."

Carnap opened each session with a summary of what he had said at the previous meeting, followed by a period of questions. The philosopher most often cited was Carnap's good friend Hans Reichenbach, with Carl Hempel running second.

I was surprised that Carnap seldom mentioned Bertrand Russell, although I knew he owed Russell a great debt. Later I attended a seminar given by Russell on the Chicago campus. Carnap was in the audience and asking questions. Much of their give and take was beyond me, though I recall Russell saying at one point, "But realism is not a dirty word." They had been arguing over whether it is desirable for a philosopher to assume the reality of an external world as something ontologically certain, or whether realism is no more than the most convenient, indeed indispensable, language for science. Carnap liked to call it the "thing language." Russell soon turned this into a question of whether Carnap's wife was truly "out there" or should be regarded merely as a useful construction within Carnap's experience.

I have written elsewhere about what occurred the next day. I was in the university's post office, talking to philosopher Charles Hartshorne, when Carnap strode in. To my eternal embarrassment, Hartshorne said to Carnap, "Mr. Gardner has been telling me that during Russell's seminar yesterday he tried to persuade you that your wife existed, but you wouldn't admit it."

Carnap glowered at me and said, "But that wasn't the point at all."

Many years later, when Carnap repeated his seminar at the University of California at Los Angeles, I wrote to propose a book. The plan was for someone—who would turn out to be Carnap's wife Ina—to attend the seminar and tape-record each session. She would then type out everything he said, including questions and answers, and send me the pages after each typing. I would edit the material into a coherent volume, working questions and replies into the text as best I could. By then I had started my writing career, and Carnap was familiar with some of my work. He liked the proposal, and the result was the book you now hold. Every idea in the book is Carnap's. Only the phrasing and arrangements are mine. The time I spent working on this book, as copy went back and forth between me and Carnap for corrections and clarifications, was one of the happiest periods of my life.

Although I never knew Carnap personally and never met his wife, I have vivid and fond memories of his class. He was a teacher who always did his best to make a question, no matter how stupid, seem significant, and to extract from it a meaningful comment. His lectures were extemporaneous, though based on notes he carried on file cards.

It was during this course that Carnap shocked us all by revealing that his friend and former associate Moritz Schlick had just been murdered by a psychotic student. There were, however, moments of comedy as well. I remember one confusing interchange with a woman mathematics teacher before it was discovered that she was using the word "pear," the fruit: Carnap had taken it to be "pair"—or maybe it was the other way around.

Basic Books published our book in 1966 under Carnap's preferred title *The Philosophical Foundations of Physics: An Introduction to the Philosophy of Science*. After Carnap's death in 1970, Basic reissued the book in paper covers, and the original subtitle became the new title, which has been retained in the Dover edition. Many corrections for the Basic edition were generously supplied by Carnap's friend Carl G. Hempel. I had asked Hempel to write a foreword, but he declined because he considered it inappropriate to write a foreword to a book by a person so much more eminent than he.

The book received good reviews, and was adopted in the classroom by Wesley Salmon and a few other noted philosophers of science here and abroad. Translations were published in Germany, France, Italy, Japan, and Argentina. It should be emphasized that this is the only book by Carnap on a level sufficiently nontechnical to be understood by readers with no expertise in mathematics, physics, or logic.

For the Dover edition I am indebted to Dover president Hayward Cirker and to editor John Grafton for recognizing the book's merit, as well as to retired philosopher of science Arthur J. Benson for dozens of corrections that have been made throughout. It was Professor Benson who had compiled the bibliography for *The Philosophy of Rudolph Carnap*, edited by Paul Arthur Schilpp as the eleventh volume of the distinguished Library of Living Philosophers series.

I also wish to thank Carnap's daughter Hanna Carnap Thost for allowing Dover to reissue this book and for putting me in touch with Dr. Benson.

It was a great privilege to have attended Carnap's seminar and to have been given the honor of editing his book. Although Carnap's reputation is not as high now as it was then, I have no doubt that it will steadily rise again. More and more younger philosophers of science will surely discover the greatness of his contributions and his influence, and how right he was in his notable quarrels (in my opinion largely verbal quibbles) with Karl Popper and Willard Van Orman Quine.

MARTIN GARDNER
Hendersonville, N.C.

Foreword to the Basic Books paperback edition, 1974

One of the most memorable privileges of my life was to have attended Rudolf Carnap's seminar on "philosophical foundations of physics" when he was at the University of Chicago. It was an even greater privilege, many years later, to be allowed to shape those seminar lectures (after Carnap had repeated them at the University of California) into the present volume. Although not exactly an elementary or "popular" book, it is certainly much less technical than any of Carnap's other works. In my opinion it is the best first introduction to the views of one of this century's great creative philosophers, as well as one of the clearest and soundest of modern introductions to the philosophy of science.

The book originally bore the title Carnap had often used for his seminar, followed by the subtitle: "An introduction to the philosophy of science." Wesley C. Salmon is mainly responsible for this edition's switch of the two phrases. Salmon had given the book a splendid review (*Science,* March 10, 1967), and for several years had used the volume for assigned reading in his classes. Two years ago he made two suggestions: first, that the book be reprinted in a paperback edition students could afford; second, that its formidable title, which conveys the false impression of a highly technical work, be changed. Both proposals have now been adopted.

Aside from a few trivial corrections, the only important textual changes are on pages 255 and 256. In response to a friendly letter from Grover Maxwell, Carnap agreed (shortly before his death in 1970) that his all-too-brief comments on the conflict between instrumentalism and realism, with respect to the nature of scientific theory, be clarified. With this in mind, he made certain alterations on the two pages, and added a new footnote referring to a 1950 paper which gives his views in more detail.

I have resisted a temptation to update the Bibliography, preferring to leave it as Carnap wanted it in 1966 rather than make my own selections from the many excellent books that have appeared since.

MARTIN GARDNER

Preface

This book grew out of a seminar that I have given many times, with varying content and form. It was called "Philosophical foundations of physics" or "Concepts, theories, and methods of the physical sciences." Although the content often changed, the general philosophical point of view remained constant; the course emphasized the logical analysis of the concepts, statements, and theories of science, rather than metaphysical speculation.

The idea of presenting the substance of my (rather informal) seminar talks in a book was suggested by Martin Gardner, who had attended my course in 1946 at the University of Chicago. He inquired in 1958 whether a typescript of the seminar existed or could be made; if so, he offered to edit it for publication. I have never had typescripts of my lectures or seminar talks, and I was not willing to take the time to write one. It just happened that this course was announced for the next semester, Fall 1958, at the University of California at Los Angeles. It was suggested that my talks and the discussions be recorded. Conscious of the enormous distance between the spoken word and a formulation suitable for publication, I was first rather skeptical about the plan. But my friends urged me to do it, because not many of my views on problems in the philosophy of science had been published. The decisive encouragement came from my wife, who volunteered to record the whole semester course on tape and transcribe it. She did this and also gave me invaluable help in the later phases of the working process. The book owes much to her; but she did not live to see it published.

A corrected version of the transcript was sent to Martin Gardner. Then he began his difficult task, which he carried out with great skill and sensitivity. He not only smoothed out the style, but found ways of making the reading easier by rearranging some of the topics and by improving examples or contributing new ones. The chapters went back and forth several times. Now and then, I made extensive changes or additions or suggested to Gardner that he make them. Although the seminar was for advanced graduate students in philosophy who were familiar with symbolic logic and had some knowledge of college mathematics and physics, we decided to make the book accessible to a wider circle of readers. The number of logical, mathematical, and physical formulas was considerably reduced, and the remaining ones were explained wherever it seemed advisable.

No attempt is made in this book to give a systematic treatment of all the important problems in the philosophical foundations of physics. In my seminar—therefore also in the book—I have preferred to restrict myself to a small number of fundamental problems (as indicated by the headings of the six parts) and to discuss them more thoroughly, instead of including a cursory discussion of many other subjects. Most of the topics dealt with in this book (except for Part III, on geometry, and Chapter 30, on quantum physics) are relevant to all branches of science, including the biological sciences, psychology, and the social sciences. I believe, therefore, that this book may also serve as a general introduction to the philosophy of science.

My first thanks go to my faithful and efficient collaborator, Martin Gardner. I am grateful for his excellent work and also for his inexhaustible patience when I made long delays in returning some chapters or asked for still more changes.

My friends Herbert Feigl and Carl G. Hempel I wish to thank for suggestive ideas they presented in conversations through many years and especially for their helpful comments on parts of the manuscript. I thank Abner Shimony for generous expert help on questions concerning quantum mechanics. And, further, I am grateful to many friends and colleagues for their stimulating influence and to my students who attended one or another version of this seminar and whose questions and comments prompted some of the discussions in this book.

I acknowledge with thanks the kind permission of Yale University Press for extensive quotations from Kurt Riezler's book, *Physics and Reality* (1940).

RUDOLF CARNAP

University of California at Los Angeles

February 1966

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Part I

LAWS, EXPLANATION, AND PROBABILITY

CHAPTER 1

The Value of Laws: Explanation and Prediction

THE OBSERVATIONS we make in everyday life as well as the more systematic observations of science reveal certain repetitions or regularities in the world. Day always follows night; the seasons repeat themselves in the same order; fire always feels hot; objects fall when we drop them; and so on. The laws of science are nothing more than statements expressing these regularities as precisely as possible.

If a certain regularity is observed at all times and all places, without exception, then the regularity is expressed in the form of a "universal law". An example from daily life is, "All ice is cold." This statement asserts that any piece of ice—at any place in the universe, at any time, past, present, or future—is (was, or will be) cold. Not all laws of science are universal. Instead of asserting that a regularity occurs in *all* cases, some laws assert that it occurs in only a certain percentage of cases. If the percentage is specified or if in some other way a quantitative statement is made about the relation of one event to another, then the statement is called a "statistical law". For example: "Ripe apples are usually red", or "Approximately half the children born each year are boys." Both types of law—universal and statistical—are needed in science. The universal laws are logically simpler, and for this reason we shall consider them first. In the early part of this discussion "laws" will usually mean universal laws.

Universal laws are expressed in the logical form of what is called in formal logic a "universal conditional statement". (In this book, we shall occasionally make use of symbolic logic, but only in a very elementary way.) For example, let us consider a law of the simplest possible type. It asserts that, whatever x may be, if x is P, then x is also Q. This is written symbolically as follows:

$$(x)(Px \supset Qx).$$

The expression "(x)" on the left is called a "universal quantifier." It tells us that the statement refers to *all* cases of x, rather than to just a certain percentage of cases. "Px" says that x is P, and "Qx" says that x is Q. The symbol "$\supset$" is a connective. It links the term on its left to the term on its right. In English, it corresponds roughly to the assertion, "If . . . then . . ."

If "x" stands for any material body, then the law states that, for any material body x, if x has the property P, it also has the property Q. For instance, in physics we might say: "For every body x, if that body is heated, that body will expand." This is the law of thermal expansion in its simplest, nonquantitative form. In physics, of course, one tries to obtain quantitative laws and to qualify them so as to exclude exceptions; but, if we forget about such refinements, then this universal conditional statement is the basic logical form of all universal laws. Sometimes we may say that, not only does Qx hold whenever Px holds, but the reverse is also true; whenever Qx holds, Px holds also. Logicians call this a biconditional statement—a statement that is conditional in both directions. But of course this does not contradict the fact that in all universal laws we deal with universal conditionals, because a biconditional may be regarded as the conjunction of two conditionals.

Not all statements made by scientists have this logical form. A scientist may say: "Yesterday in Brazil, Professor Smith discovered a new species of butterfly." This is not the statement of a law. It speaks about a specified single time and place; it states that something happened at that time and place. Because statements such as this are about single facts, they are called "singular" statements. Of course, all our knowledge has its origin in singular statements—the particular observations of particular individuals. One of the big, perplexing questions in the philosophy of science is how we are able to go from such singular statements to the assertion of universal laws.

When statements by scientists are made in the ordinary word language, rather than in the more precise language of symbolic logic, we must be extremely careful not to confuse singular with universal statements. If a zoologist writes in a textbook, "The elephant is an excellent swimmer", he does not mean that a certain elephant, which he observed a year ago in a zoo, is an excellent swimmer. When he says "the elephant", he is using "the" in the Aristotelian sense; it refers to the entire class of elephants. All European languages have inherited from the Greek (and perhaps also from other languages) this manner of speaking in a singular way when actually a class or type is meant. The Greeks said, "Man is a rational animal." They meant, of course, all men, not a particular man. In a similar way, we say "the elephant" when we mean all elephants or "tuberculosis is characterized by the following symptoms . . ." when we mean, not a singular case of tuberculosis, but all instances.

It is unfortunate that our language has this ambiguity, because it is a source of much misunderstanding. Scientists often refer to universal statements—or rather to what is expressed by such statements—as "facts". They forget that the word "fact" was originally applied (and we shall apply it exclusively in this sense) to singular, particular occurrences. If a scientist is asked about the law of thermal expansion, he may say: "Oh, thermal expansion. That is one of the familiar, basic facts of physics." In a similar way, he may speak of the fact that heat is generated by an electric current, the fact that magnetism is produced by electricity, and so on. These are sometimes considered familiar "facts" of physics. To avoid misunderstandings, we prefer not to call such statements "facts". Facts are particular events. "This morning in the laboratory, I sent an electric current through a wire coil with an iron body inside it, and I found that the iron body became magnetic." That is a fact unless, of course, I deceived myself in some way. However, if I was sober, if it was not too foggy in the room, and if no one has tinkered secretly with the apparatus to play a joke on me, then I may state as a factual observation that this morning that sequence of events occurred.

When we use the word "fact", we will mean it in the singular sense in order to distinguish it clearly from universal statements. Such universal statements will be called "laws" even when they are as elementary as the law of thermal expansion or, still more elementary, the statement, "All ravens are black." I do not know whether this statement is true, but, assuming its truth, we will call such a statement a law of zoology. Zoologists may speak informally of such "facts" as "the raven is black" or "the octopus has eight arms", but, in our more precise terminology, statements of this sort will be called "laws".

Later we shall distinguish between two kinds of law—empirical and theoretical. Laws of the simple kind that I have just mentioned are sometimes called "empirical generalizations" or "empirical laws". They are simple because they speak of properties, like the color black or the magnetic properties of a piece of iron, that can be directly observed. The law of thermal expansion, for example, is a generalization based on many direct observations of bodies that expand when heated. In contrast, theoretical, nonobservable concepts, such as elementary particles and electromagnetic fields, must be dealt with by theoretical laws. We will discuss all this later. I mention it here because otherwise you might think that the examples I have given do not cover the kind of laws you have perhaps learned in theoretical physics.

To summarize, science begins with direct observations of single facts. Nothing else is observable. Certainly a regularity is not directly observable. It is only when many observations are compared with one another that regularities are discovered. These regularities are expressed by statements called "laws".

What good are such laws? What purposes do they serve in science and everyday life? The answer is twofold: they are used to *explain* facts already known, and they are used to *predict* facts not yet known.

First, let us see how laws of science are used for explanation. No explanation—that is, nothing that deserves the honorific title of "explanation"—can be given without referring to at least one law. (In simple cases, there is only one law, but in more complicated cases a set of many laws may be involved.) It is important to emphasize this point, because philosophers have often maintained that they could explain certain facts in history, nature, or human life in some other way. They usually do this by specifying some type of agent or force that is made responsible for the occurrence to be explained.

In everyday life, this is, of course, a familiar form of explanation. Someone asks: "How is it that my watch, which I left here on the table before I left the room, is no longer here?" You reply: "I saw Jones come into the room and take it." This is your explanation of the watch's disappearance. Perhaps it is not considered a sufficient explanation. Why did Jones take the watch? Did he intend to steal it or just to borrow it? Perhaps he took it under the mistaken impression that it was his own. The first question, "What happened to the watch?", was answered by a statement of fact: Jones took it. The second question, "Why did Jones take it?", may be answered by another fact: he borrowed it for a moment. It seems, therefore, that we do not need laws at all. We ask for an explanation of one fact, and we are given a second fact. We ask for an explanation of the second fact, and we are given a third. Demands for further explanations may bring out still other facts. Why, then, is it necessary to refer to a law in order to give an adequate explanation of a fact?

The answer is that fact explanations are really law explanations in disguise. When we examine them more carefully, we find them to be abbreviated, incomplete statements that tacitly assume certain laws, but laws so familiar that it is unnecessary to express them. In the watch illustration, the first answer, "Jones took it", would not be considered a satisfactory explanation if we did not assume the universal law: whenever someone takes a watch from a table, the watch is no longer on the table. The second answer, "Jones borrowed it", is an explanation because we take for granted the general law: if someone borrows a watch to use elsewhere, he takes the watch and carries it away.

Consider one more example. We ask little Tommy why he is crying, and he answers with another fact: "Jimmy hit me on the nose." Why do we consider this a sufficient explanation? Because we know that a blow on the nose causes pain and that, when children feel pain, they cry. These are general psychological laws. They are so well known that they are assumed even by Tommy when he tells us why he is crying. If we were dealing with, say, a Martian child and knew very little about Martian psychological laws, then a simple statement of fact might not be considered an adequate explanation of the child's behavior. Unless facts can be connected with other facts by means of at least one law, explicitly stated or tacitly understood, they do not provide explanations.

The general schema involved in all explanation of the deductive variety can be expressed symbolically as follows:

1. $(x)(Px \supset Qx)$
2. Pa
3. Qa

The first statement is the universal law that applies to any object x. The second statement asserts that a particular object a has the property

P. These two statements taken together enable us to derive logically the third statement: object *a* has the property *Q*.

In science, as in everyday life, the universal law is not always explicitly stated. If you ask a physicist: "Why is it that this iron rod, which a moment ago fitted exactly into the apparatus, is now a trifle too long to fit?", he may reply by saying: "While you were out of the room, I heated the rod." He assumes, of course, that you know the law of thermal expansion; otherwise, in order to be understood, he would have added, "and, whenever a body is heated, it expands". The general law is essential to his explanation. If you know the law, however, and he knows that you know it, he may not feel it necessary to state the law. For this reason, explanations, especially in everyday life where common-sense laws are taken for granted, often seem quite different from the schema I have given.

At times, in giving an explanation, the only known laws that apply are statistical rather than universal. In such cases, we must be content with a statistical explanation. For example, we may know that a certain kind of mushroom is slightly poisonous and causes certain symptoms of illness in 90 per cent of those who eat it. If a doctor finds these symptoms when he examines a patient and the patient informs the doctor that yesterday he ate this particular kind of mushroom, the doctor will consider this an explanation of the symptoms even though the law involved is only a statistical one. And it is, indeed, an explanation.

Even when a statistical law provides only an extremely weak explanation, it is still an explanation. For instance, a statistical medical law may state that 5 per cent of the people who eat a certain food will develop a certain symptom. If a doctor cites this as his explanation to a patient who has the symptom, the patient may not be satisfied. "Why", he asks, "am I one of the 5 per cent?" In some cases, the doctor may be able to provide further explanations. He may test the patient for allergies and find that he is allergic to this particular food. "If I had known this", he tells the patient, "I would have warned you against this food. We know that, when people who have such an allergy eat this food, 97 per cent of them will develop symptoms such as yours." That may satisfy the patient as a stronger explanation. Whether strong or weak, these are genuine explanations. In the absence of known universal laws, statistical explanations are often the only type available.

In the example just given, the statistical laws are the best that can be stated, because there is not sufficient medical knowledge to warrant stating a universal law. Statistical laws in economics and other fields of social science are due to a similar ignorance. Our limited knowledge of psychological laws, of the underlying physiological laws, and of how those may in turn rest on physical laws makes it necessary to formulate the laws of social science in statistical terms. In quantum theory, however, we meet with statistical laws that may not be the result of ignorance; they may express the basic structure of the world. Heisenberg's famous principle of uncertainty is the best-known example. Many physicists believe that all the laws of physics rest ultimately on fundamental laws that are statistical. If this is the case, we shall have to be content with explanations based on statistical laws.

What about the elementary laws of logic that are involved in all explanations? Do they ever serve as the universal laws on which scientific explanation rests? No, they do not. The reason is that they are laws of an entirely different sort. It is true that the laws of logic and pure mathematics (not physical geometry, which is something else) are universal, but they tell us nothing whatever about the world. They merely state relations that hold between certain concepts, not because the world has such and such a structure, but only because those concepts are defined in certain ways.

Here are two examples of simple logical laws:

1. If *p* and *q*, then *p*.
2. If *p*, then *p* or *q*.

Those statements cannot be contested because their truth is based on the meanings of the terms involved. The first law merely states that, if we assume the truth of statements *p* and *q*, then we must assume that statement *p* is true. The law follows from the way in which "and" and "if . . . then" are used. The second law asserts that, if we assume the truth of *p*, we must assume that either *p* or *q* is true. Stated in words, the law is ambiguous because the English "or" does not distinguish between an inclusive meaning (either or both) and the exclusive meaning (either but not both). To make the law precise, we express it symbolically by writing:

$$p \supset (p \vee q)$$

The symbol "$\vee$" is understood as "or" in the inclusive sense. Its meaning can be given more formally by writing out its truth table. We do this by listing all possible combinations of truth values (truth or falsity) for the two terms connected by the symbol, then specifying which combinations are permitted by the symbol and which are not.

The four possible combinations of values are:

	p	q
1.	true	true
2.	true	false
3.	false	true
4.	false	false

The symbol "$\vee$" is defined by the rule that "$p \vee q$" is true in the first three cases and false in the fourth case. The symbol "$\supset$", which translates roughly into English as "if . . . then", is defined by saying that $p \supset q$ is true in the first, third, and fourth cases, and false in the second. Once we understand the definition of each term in a logical law, we see clearly that the law must be true in a way that is wholly independent of the nature of the world. It is a necessary truth, a truth that holds, as philosophers sometimes put it, in all possible worlds.

This is true of the laws of mathematics as well as those of logic. When we have precisely specified the meanings of "1", "3", "4", "+", and "=", the truth of the law "$1 + 3 = 4$" follows directly from these meanings. This is the case even in the more abstract areas of pure mathematics. A structure is called a "group", for example, if it fulfills certain axioms that define a group. Three-dimensional Euclidean space can be defined algebraically as a set of ordered triples of real numbers that fulfill certain basic conditions. But all this has nothing to do with the nature of the outside world. There is no possible world in which the laws of group theory and the abstract geometry of Euclidean 3-space would not hold, because these laws are dependent only on the meanings of the terms involved, and not on the structure of the actual world in which we happen to be.

The actual world is a world that is constantly changing. Even the most fundamental laws of physics may, for all we can be sure, vary slightly from century to century. What we believe to be a physical constant with a fixed value may be subject to vast cyclic changes that we have not yet observed. But such changes, no matter how drastic, would never destroy the truth of a single logical or arithmetical law.

It sounds very dramatic, perhaps comforting, to say that here at last we have actually found certainty. It is true that we have obtained certainty, but we have paid for it a very high price. The price is that statements of logic and mathematics do not tell us anything about the world. We can be sure that three plus one is four; but, because this holds in any possible world, it can tell us nothing whatever about the world we inhabit.

What do we mean by "possible world"? Simply a world that can be described without contradiction. It includes fairy-tale worlds and dream worlds of the most fantastic kind, provided that they are described in logically consistent terms. For example, you may say: "I have in mind a world in which there are exactly one thousand events, no more, no less. The first event is the appearance of a red triangle. The second is the appearance of a green square. However, since the first event was blue and not red . . .". At this point, I interrupt. "But a moment ago you said that the first event is red. Now you say that it is blue. I do not understand you." Perhaps I have recorded your remarks on tape. I play back the tape to convince you that you have stated a contradiction. If you persist in your description of this world, including the two contradictory assertions, I would have to insist that you are not describing anything that can be called a possible world.

On the other hand, you may describe a possible world as follows: "There is a man. He shrinks in size, becoming smaller and smaller. Suddenly he turns into a bird. Then the bird becomes a thousand birds. These birds fly into the sky, and the clouds converse with one another about what happened." All this is a possible world. Fantastic, yes; contradictory, no.

We might say that possible worlds are conceivable worlds, but I try to avoid the term "conceivable" because it is sometimes used in the more restricted sense of "what can be imagined by a human being". Many possible worlds can be described but not imagined. We might, for example, discuss a continuum in which all points determined by rational coordinates are red and all points determined by irrational coordinates are blue. If we admit the possibility of ascribing colors to points, this is a noncontradictory world. It is conceivable in the wider sense; that is, it can be assumed without contradiction. It is not conceivable in the psychological sense. No one can imagine even an uncolored continuum of points. We can imagine only a crude model of a continuum—a model consisting of very tightly packed points. Possible worlds are worlds that are conceivable in the wider sense. They are worlds that can be described without logical contradiction.

The laws of logic and pure mathematics, by their very nature, cannot be used as a basis for scientific explanation because they tell us nothing that distinguishes the actual world from some other possible world.

When we ask for the explanation of a fact, a particular observation in the actual world, we must make use of *empirical* laws. They do not possess the certainty of logical and mathematical laws, but they do tell us something about the structure of the world.

In the nineteenth century, certain Germanic physicists, such as Gustav Kirchhoff and Ernst Mach, said that science should not ask "Why?" but "How?" They meant that science should not look for unknown metaphysical agents that are responsible for certain events, but should only describe such events in terms of laws. This prohibition against asking "Why?" must be understood in its historical setting. The background was the German philosophical atmosphere of the time, which was dominated by idealism in the tradition of Fichte, Schelling, and Hegel. These men felt that a description of how the world behaved was not enough. They wanted a fuller understanding, which they believed could be obtained only by finding metaphysical causes that were behind phenomena and not accessible to scientific method. Physicists reacted to this point of view by saying: "Leave us alone with your why-questions. There is no answer beyond that given by the empirical laws." They objected to why-questions because they were usually metaphysical questions.

Today the philosophical atmosphere has changed. In Germany there are a few philosophers still working in the idealist tradition, but in England and the United States it has practically disappeared. As a result, we are no longer worried by why-questions. We do not have to say, "Don't ask why", because now, when someone asks why, we assume that he means it in a scientific, nonmetaphysical sense. He is simply asking us to explain something by placing it in a framework of empirical laws.

When I was young and part of the Vienna Circle, some of my early publications were written as a reaction to the philosophical climate of German idealism. As a consequence, these publications and those by others in the Vienna Circle were filled with prohibitory statements similar to the one I have just discussed. These prohibitions must be understood in reference to the historical situation in which we found ourselves. Today, especially in the United States, we seldom make such prohibitions. The kind of opponents we have here are of a different nature, and the nature of one's opponents often determines the way in which one's views are expressed.

When we say that, for the explanation of a given fact, the use of a scientific law is indispensable, what we wish to exclude especially is the view that metaphysical agents must be found before a fact can be adequately explained. In prescientific ages, this was, of course, the kind of explanation usually given. At one time, the world was thought to be inhabited by spirits or demons who are not directly observable but who *act* to cause the rain to fall, the river to flow, the lightning to flash. In whatever one saw happening, there was something—or, rather, *somebody*—responsible for the event. This is psychologically understandable. If a man does something to me that I do not like, it is natural for me to make him responsible for it and to get angry and hit back at him. If a cloud pours water over me, I cannot hit back at the cloud, but I can find an outlet for my anger if I make the cloud, or some invisible demon behind the cloud, responsible for the rainfall. I can shout curses at this demon, shake my fist at him. My anger is relieved. I feel better. It is easy to understand how members of prescientific societies found psychological satisfaction in imagining agents behind the phenomena of nature.

In time, as we know, societies abandoned their mythologies, but sometimes scientists replace the spirits with agents that are really not much different. The German philosopher Hans Driesch, who died in 1941, wrote many books on the philosophy of science. He was originally a prominent biologist, famed for his work on certain organismic responses, including regeneration in sea urchins. He cut off parts of their bodies and observed in which stages of their growth and under what conditions they were able to grow new parts. His scientific work was important and excellent. But Driesch was also interested in philosophical questions, especially those dealing with the foundations of biology, so eventually he became a professor of philosophy. In philosophy also he did some excellent work, but there was one aspect of his philosophy that I and my friends in the Vienna Circle did not regard so highly. It was his way of *explaining* such biological processes as regeneration and reproduction.

At the time Driesch did his biological work, it was thought that many characteristics of living things could not be found elsewhere. (Today it is seen more clearly that there is a continuum connecting the organic and inorganic worlds.) He wanted to explain these unique organismic features, so he postulated what he called an "entelechy". This term had been introduced by Aristotle, who had his own meaning for it, but we need not discuss that meaning here. Driesch said, in effect: "The entelechy is a certain specific force that causes living things to behave in the way they do. But you must not think of it as a *physical* force such as gravity or magnetism. Oh, no, nothing like that."

The entelechies of organisms, Driesch maintained, are of various