A PHILOSOPHER LOOKS AT SCIENCE

by

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Introduction

ONCE UPON A TIME there was an ugly little caterpillar. All the other small animals strutted around, preening their colorful feathers or showing off their glittering coats, while the little caterpillar hid and felt ashamed. Then one day he made up his mind he would not rest until he changed himself into the most beautiful caterpillar in the world. He struggled, he puffed, he almost burst himself trying, but he did succeed. "Look at me," he shouted, "I am truly a lovely caterpillar." But the other animals snickered and laughed at him behind his back. Finally a wise old owl, who had been watching from above, said to the deflated little caterpillar: "The others are not laughing at you because you are not beautiful. Don't you know there is no such thing as a beautiful caterpillar? You have turned yourself into a butterfly."

Every philosopher must learn the lesson of this fable. No matter how hard the philosopher tries to discover the laws of nature, no philosopher can ever do so for the simple reason that, if he succeeds, people will call him a scientist.

It is often pointed out that all of Science grew out of Philosophy. If you read about ancient Greece, you will find a group of philosophers asking questions, the answers to which form the basis of our Science. For a long time they could do no more than ask questions and indulge in more or less ingenious guesses, but slowly the modern scientific method developed, giving definite and well-founded answers to these questions. Slowly the philosopher found himself in a dilemma. If he asked a question, he was a philosopher; if he answered it, he was a scientist.

This book is not a science book. There are many excellent and easily readable books explaining the results of modern Science.

This is a book on the Philosophy of Science. By this time the question will arise: "Are there any questions left for the philosopher to answer?" There certainly are. Roughly speaking, the philosopher deals with those questions that the scientist either does not answer or cannot answer. Fortunately, some of the most interesting questions fall into these categories. But I have restricted myself further; not only is this a philosophy book, but it is a philosophy of science book. Therefore I must restrict myself to such philosophical questions as arise in connection with Science.

In other words, this is not a science book, but a book about Science. This is not nearly so subtle a distinction as one might suppose. A beautiful painting is a piece of art; yet a book explaining the technique used by the artist is *about* Art, not necessarily a piece of art itself. Similarly, if we perform an experiment and write up the results, we are acting as scientists. But when we discuss the general problems involved in experimentation, we are writing about Science. What are these problems that scientists do not write about (unless, of course, they happen to be acting as philosophers)? They include discussions on the scientific method, on the concepts and basic assumptions of Science, its subject matter and its limitations, what it makes use of and what it is used for in turn. In general, it is an attempt to give a unified picture of the nature of Science.

Let me try to illustrate this distinction in terms of the Theory of Relativity. If we ask what this theory can tell us about the motion of planets, that is a scientific question; if we ask why this answer is accepted by scientists, this is a question about scientific method and hence belongs to the Philosophy of Science. If we want to see how the theory is deduced from a few basic assumptions, we go to a science book (and there is certainly none better than Einstein's own account). But if we want to know how these basic assumptions could possibly be justified, we had better ask a philosopher. If we want to learn about mathematical methods used in Physics, any good science department will provide us with the answer, but it is not their job to explain just what the relation is between Mathematics and Science. We would also do well not to ask the average scientist such questions, because it may very well be that he is not in a position to answer us. His whole life is devoted to a task that is all-absorbing and may be more than one human being can accomplish. This is perhaps the reason why many of these tremendously important and interesting questions are left for the philosopher to answer. It is questions of this type that we will try to discuss, and whenever possible we will try to answer them in this book.

Since my basic purpose is to present a unified picture of Science, it is difficult to divide the book into parts. However, I can tell you the basic pattern that I have followed. I start out with certain questions that are presupposed by Science. From this I proceed to a discussion of Science itself and I end up with certain problems that arise out of Science. The first chapter deals with the problem of language and its relevance to the various questions discussed in the book. From this we pass to a discussion of Mathematics, the language that has been found most useful for Science. Since it has often been maintained that the usage of this language involves some basic assumptions, these supposed assumptions are discussed in the third chapter. We are then almost ready to go on to a discussion of Science itself. Just one more tool has to be treated, namely, probabilities. The discussion of Science proper starts with a chapter on the scientific method. After that, four basic questions are asked, questions which arise in a discussion of the scientific method, and a chapter is devoted to each one. This leads us to Chapter 10, which is a discussion of what Science is. Then there are the questions that arise out of Science. Do we live in a completely determined universe? What is Life? What is the nature of our minds? What is the status of values? What is the nature of the social sciences? With this we are brought to the final chapter, which attempts to summarize what has gone before.

As the plan of the book might indicate, it is organized to appeal to the interested layman. I do hope, however, that the unified picture here presented, and the approach of the third chapter (on which the remainder of the book is based), will be of interest even to the expert. A unified picture in so small a space must necessarily be sketchy, so I included a few suggestions for additional reading, which will be found at the end of each chapter. I have tried to arrange these so that references can be quickly found and so that it is easy to see which reference will provide the answer to the question in the reader's mind. If used as an introductory text, it should be supplemented with a good popular book on Science. If it is used in a more advanced course, it should be used in conjunction with some of the books mentioned at the ends of the various chapters. This book could provide the continuity, and the references would fill in details on which the book is too sketchy.

Just one more word of warning. We have already noted the fact that philosophers ask many more questions than they can answer. I believe that asking a good clear question is one of the most important things we can do. We find many instances in the history of both Science and Philosophy where a question was unanswered for centuries until some genius came along and rephrased the question, and all of a sudden it was found that the answer was very simple to find as well. For this reason a great deal of time is spent in this book in clarifying issues. Very often this is the best that I can do.

The book is dedicated to the belief that clarification of a difficult problem is a great step forward. It certainly avoids much fruitless and apparently endless debate, and hence clears the air for fruitful work and the solution of the problem. Nevertheless we are often forced to face one unanswerable question. Have we really learned much? So much that we want to know is still left open, and we remain faced with so many uncertainties about what we do know. Perhaps what we have learned so far is really very little. But when we consider that the pursuit of knowledge for its own sake, the attempt to answer these fundamental questions, has been one of the greatest driving forces for all intellectual pursuits, then it is comforting to note that in all probability these questions, or at least many of them, will forever remain unanswered.

PART ONE

What Science Presupposes

5

The Method

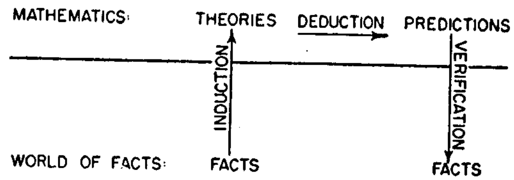
"I should see the garden far better," said Alice to herself, "if I could get to the top of that hill: and here is a path that leads straight to it—at least no, it doesn't do that*—but I suppose it will at last. But how curiously it twists! It's more like a cork-screw than a path! Well* this *turn goes to the hill, I suppose—no it doesn't! This goes straight back to the house! Well then, I'll try it the other way."*

THE FIRST FOUR CHAPTERS covered the preliminaries: what we must know before we can understand Science. Now we are ready to turn to a study of our proper subject matter—the nature of Science. The most characteristic feature of Science is its method, and this is the first thing we want to study. I will maintain the thesis that there is one basic method common to all of Science, and I will try to show just what that method is.

THE CYCLE

As Einstein has repeatedly emphasized, Science must start with facts and end with facts, no matter what theoretical structures it builds in between. First of all the scientist is an observer. Next he tries to describe in complete generality what he saw, and what he expects to see in the future. Next he makes predictions on the basis of his theories, which he checks against facts again.

The most characteristic feature of the method is its cyclic nature. It starts with facts, ends in facts, and the facts ending one cycle are the beginning of the next cycle. A scientist holds his theories tentatively, always prepared to abandon them if the facts do not bear out the predictions. If a series of observations, designed to verify certain predictions, force us to abandon our theory, then we look for a new or improved theory. Thus these facts form the fourth stage for the old theory as well as the first stage of the new theory. Since we expect that Science consists of an endless chain of progress, we may expect this cyclic process to continue indefinitely.



The horizontal line in the diagram separates the world of the experimentalist, the universe of facts, from that of the theoretician, the world of Mathematics. In the world below (the line) we find men peering through microscopes, while above we find an endless string of mathematical formulas. What will interest us most in this chapter is the way we proceed from stage to stage; accordingly we will study three steps. The first step carries us from the original observations to the theories. This is known as "induction," or the formation of theories on the basis of factual knowledge. As we have seen, this means that the scientist finds a mathematical formula which he can interpret to suit the facts that he is trying to incorporate in a theory. Then he asks himself the question: "Is this really what I want?" And he is forced to go back to the world of facts to check his construction. But you cannot check a general law directly; you must first ask what it tells you about particular facts. You cannot observe that the sun rises every day throughout eternity; what you can observe is that it rises today, and that it rises tomorrow, and the next day, etc. Any (finite) number of these can be checked. So the scientist must get from his general laws a prediction as to what will actually happen, say, tomorrow. This step is accomplished by "deduction,"

by logical analysis of what the general law says about a particular event tomorrow. Then he is ready to return to the facts, and see whether he was right in his prediction. This third and final step, consisting of experiments or observations, is the "verification" of the theory.

As an example of this cycle, I will cite one of the most dramatic chapters from the history of Science.

Our story starts in the year 1820. The French astronomer, Alexis Bouvard, was a little-known scientist whose contribution consisted in a painstaking charting of the paths of the planets. He was especially interested in the three large outer planets, Jupiter, Saturn, and Uranus. Bouvard was performing the very important task of accumulating more factual knowledge, enabling us to check and recheck the accepted theories. Newton's theory was accepted without question as a complete explanation of planetary motion. It was, therefore, a great shock that the observed positions of Uranus did not agree with the predictions. The deviation was small, but it was more than one minute of arc—which could not be put down as an error of observation.

This is the last step in one cycle of the scientific method. Many data were accumulated, primarily by Tycho Brahe. Kepler, Galileo, and Newton succeeded in formulating a good theory. Thousands of predictions were made on the basis of this theory, and until this time all of them were verified. But a single (carefully checked) incorrect prediction is sufficient to force us to modify our theories.

Yet all that we have really shown is that some theory, now accepted, is wrong. We still have a choice as to which theory to abandon. In this we must remember not only general theories, but particular ones; for example, our assumptions as to how many planets there are. Newton's theory was so well established that scientists would rather have abandoned any other part of the accepted body of knowledge. Hence, soon they came to guess that they must have been wrong in assuming that Uranus was the outermost planet.

A new, modified body of theories was formed by assuming that there was a planet beyond Uranus. But this was not enough to explain the observed facts. One had to show that a planet of the right size in the right place would account exactly for the observed deviations. Since the size of Uranus was known, and since Newton's theory as to the strength of attraction between planets was still accepted, it was a problem of pure mathematics to deduce the size and position of the hypothetical planet—or to show that a new planet cannot explain the observations.

The French mathematician Leverrier was the one who succeeded in solving this problem. With the mathematics known in those days this was a very difficult problem requiring considerable originality. Today it would be a routine assignment. Leverrier was able to determine both the size and the position of the unknown planet, which enabled astronomers to look for it.

We might be tempted to say that this was unnecessary. Why couldn't astronomers simply scan that portion of the sky until they found a new planet? The answer is that planets are not at all easy to locate. A planet, unless it is very near us or very large, looks no different from the billions of stars. It can be distinguished only by its path. The stars appear to revolve around the earth as if they were attached to a glass sphere (as the ancients thought they were), while planets move in a more irregular path. We would have to chart the position of all the stars in a region and follow these over a period of weeks until we spotted one whose position relative to the rest is changing. This would be a nearly hopeless task.

But with Leverrier's calculations in hand the Berlin Observatory knew the exact position of the sky and the magnitude of the "star" to look for. This simplified their work sufficiently so that in a brief period of intense observations they verified the existence of the hypothetical planet. The newly discovered planet was named Neptune.

This completed the most recent cycle of scientific method. The previous cycle was finished by Bouvard failing to verify predictions. The inductive step was formulated by several scientists who proposed that we modify our theory as to the number of planets. The deduction was Leverrier's; through a difficult mathematical argument he predicted the size and position of Neptune. The verification was accomplished at the Berlin Observatory. The cyclic nature of the process is further emphasized by the fact that it was repeated along similar lines in the twentieth century, when Pluto was discovered.

FACTS VS. THEORIES

Before we discuss the three steps of the scientific method, I must say something about the two "worlds" of the scientist. One is the everyday world we are all familiar with, only the scientist's familiarity with this world derives from careful, accurate observations. The other is the mysterious, fascinating world of the theoretician, the world of ideas, the world of mathematical formulas. Establishing a connection between these two worlds is one of the most difficult tasks a scientist must face.

We would like to think of a scientist as starting with "hard facts," and building theories on these. But I doubt that we can state a fact entirely divorced from theoretical interpretations. You might feel that when you see a table, you have a hard fact, but you have actually made use of certain theories you have so thoroughly accepted and assimilated that you use them subconsciously. I certainly do not deny that it is a "hard fact" that you have a sensation which we commonly describe as seeing a table, but this is not all that you mean when you state that you see a table. Suppose I ask you whether you could stick your fist through the object you see; you indignantly reply that the answer is most certainly "No"; after all you just said that it was a table that you saw. But there is nothing in your visual image that makes it logically certain that you see a solid object.

As a matter of fact, under certain circumstances, as in dreams and mirages, you can "put your hand" through a seen "table." It is a theory based on past experience that certain visual images are associated with solid objects. You will also assume that the top of the table looks four-sided from all points of view, but that while it looks like a rectangle from above, the angles will vary as you walk around it; in other words you assume certain primitive optical laws. While there are primitive "hard facts" in your experience, your report of your experience always contains an interpretation of what you think you saw. Sometimes the laws that you assume are far from elementary. When the biologist looks through a microscope and reports seeing a minute living creature, he makes use of advanced laws of optics (in connecting up what he sees through the microscope with what there is) and of laws of Biology (in inferring that the image is that of a living being).

In order to make his facts as reliable as possible, the scientist performs experiments. He arranges a physical situation with specified details, and then he reports nothing but what his instruments "tell" him. It has sometimes been said that all of Science is based on pointer-readings. This is somewhat exaggerated, because the readings tell us nothing unless we know what the meter reads, but it does bring out an important technique. The situation is further complicated by the fact that the scientist himself may serve as the instrument—for example, when he counts the number of albino guinea pigs, or when he "records" the reaction of his subject to a question.

Remembering that this is an oversimplification, let us accept that the scientist starts out with reports of what there really is. (We will return to this problem in the Chapter 7 on concepts.) But what the theoretician gets out of this is a statement like: $x = 3.25$, $y = 2.97$, $z = -4.00$, $t = 49.32$; or he may end up with points of a graph, or a "table" (say of "yes" and "no" against various questions). These are the so-called facts which the theoretician must unite into a theory. His x may be a reading on a ruler, his y the height of a column of mercury on a fixed scale, his z the elevation above sea level measured by a complicated surveying technique, and t the reading on a clock. These are the technical representations of observations, which record one or a series of events. His theories, on the other hand, will be generalized mathematical statements. A theory may be an equation like $xy - z^3 = t - 21.35$, or it may be a complete graph, or a rule of how tables will look in all cases.

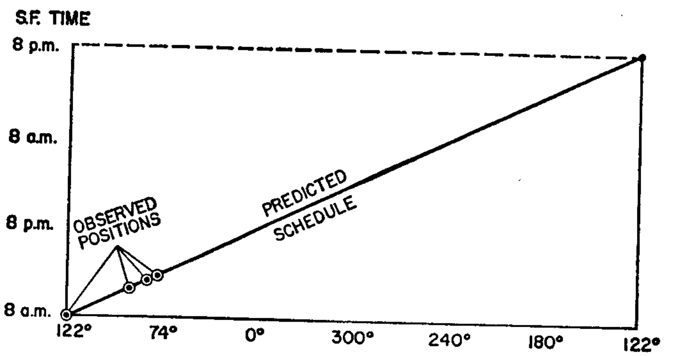
It will be convenient to speak of the mathematical record of a fact (like $x = 3.25$, etc.) as a fact. If we allow this, then the only difference between a fact and a theory is that a fact is something that we already know, while a theory also states things not yet observed (and possibly never to be observed). There is a second difference which holds in most, but not all, cases: A fact reports a single event, while a theory reports an unlimited, perhaps infinite, number of events. The reason this does not always hold is that the latter may be false. A fact is always a single thing, like "there is a sun in the sky right now." Although a theory is generally a statement, like "the sun rises every 24 hours," the statement "the sun will rise at 8:00 A.M. tomorrow" also has the status of a theory. The reason for this double possibility is that, while a genuine theory is universal (rather than particular, like a fact), it has logical consequences, which are particular statements. For example, the particular statement "the sun will rise tomorrow" follows from the universal "the sun rises every day."

After making these subtle distinctions, I will ignore them. I will normally use the word "theory" only to apply to universal statements. Hence there will be a twofold difference between facts and theories: Facts are known and particular (refer to a single event), whereas theories are universal and hence can never be known to be entirely true. It is because of this universal nature of theories, because they apply (in principle) to an infinity of events, that Mathematics is almost always indispensable for their statement.

So the scientist makes some observations (perhaps as the result of a planned experiment), and records these in the mathematical language devised by the theoretician. The theoretician tries to formulate a general mathematical proposition, incorporating these facts. Then he develops this theory mathematically, deriving certain predictions of facts. These predictions are, of course, still mathematical propositions, and must be translated back into everyday language before they can be checked.

Let us take an example. We observe a plane that has taken off for a nonstop flight around the world. We note its position at various moments, and record these. What we see is that the plane is over San Francisco at a certain time, over St. Louis at a later time, over Pittsburgh still later, over Newark still later. For simplicity let us record only its longitude and the time (in hours counted from take-off). The table shows that it is traveling at a constant speed (roughly) of about 10 degrees longitude in an hour.

Longitude	Hours elapsed
122°	0
90°	3
80°	4
74°	4¾



What we actually have done has been to plot the four reports as points of a graph, and then drawn the simplest smooth curve fitting these points well. It happened to be a straight line. This line is our theory; it is universal since it tells us where the plane will be at every moment from take-off to landing. From it we can read off that the plane will reach the original longitude in 36 hours, and hence we predict that (assuming that the plane left at 8:00 A.M. Monday) it will arrive back in San Francisco at about 8:00 P.M. Tuesday. We started in this by observations (of positions of the plane) and ended with another such observation. In between, these observations were translated into the language of Mathematics (as points on a graph), incorporated into a mathematical theory (a curve that is supposed to hold in general), and the last observation was predicted (from the terminal point on the graph).

This example is typical of the interplay between facts and theories, except that the mathematical propositions are generally

much more complicated, and the connection between theory and facts is quite a bit less direct. With these few remarks as to the two "worlds" of Science, we are prepared to discuss the passage from stage to stage in detail.

INDUCTION

Induction is the process by which the scientist forms a theory to explain the observed facts. Two steps can be distinguished within this procedure: the formation of possible theories and the selection of one of these.

Let us start with the second problem. Given a large number of possible theories, how do we select the one we want? Let me introduce the term "hypothesis" to stand for an interpreted mathematical proposition which we are considering as a possible theory; I will call such a proposition a hypothesis while it is still highly in doubt, and a theory when we have accepted it. Given various hypotheses, we must first of all see whether they explain all the known facts. This is not as simple as it sounds, because our facts are never perfectly accurate, and we must face all the problems of the theory of errors (see the last chapter). But we can select those hypotheses which are reasonably well in agreement with the evidence. Then, of the remaining ones we select the simplest hypothesis.

The question that arises is: Must there be several remaining ones? Isn't it true that if we have enough facts, then there is but one theory that could explain all of these? I would like to convince you that no matter what pains you take to accumulate facts, there will always be many possible hypotheses left; as a matter of fact, there will still be an infinite number of possibilities. The best way to think of this is to think of facts as represented by points on a piece of graph paper, and of hypotheses as curves. For the hypothesis to explain all the facts, the curve must go through all the points (or, since the facts are only approximate, it suffices that the curve should pass very near all the points). Now, no matter how hard you work, you will have only a finite number of points, since within a limited existence you can accumulate only a finite number of facts. Put down a number of points on the graph, and try to draw curves through them. You will soon see that there are an infinite number of possibilities.

Of course, it may happen that of the hypotheses that *you* started with all but one will be eliminated, or even that all of these will be eliminated (since you never really consider *all* mathematical propositions as hypotheses). But there is no reason to expect this to be the normal occurrence. So you must choose from several hypotheses, all of which fit the facts. Why choose the simplest one? For the moment I will just say that it is as good a choice as any, and more convenient than the complex hypotheses. Actually there are better reasons for this, but these will have to wait for the next chapter.

In the example of drawing a curve through given points, this means that we draw as simple or smooth a curve going near the various points as possible. For example, in the airplane example this was a straight line. A straight line is always the simplest, only it is not always a possibility. As a matter of fact, scientists are so fond of straight lines that there have been many examples where a scientist has drawn a straight line through points where these points were nowhere near the line. Of course, this is a violation of the scientific method: first the hypothesis must fit the facts; only then can we worry about its simplicity.

Let us return to the discovery of the planet Neptune. What were the competing hypotheses? First of all, we could have tried some modification of Newton's laws. For example, instead of assuming that the force of gravitation always decreases with the square of the distance, we could have modified this rule. The danger in this is that the rule worked so well for the other planets. Nevertheless, we could have said that this was only because they were pretty near the sun, and hence the deviation did not show up until we got to the outermost planet, Uranus. We could have modified the square of the distance rule by a small term, which was negligible until we reached Uranus. I am quite sure that with sufficient mathematical ingenuity this could have been done, but the resulting rule would have been highly complicated, and it was simpler to formulate the hypothesis that there was an unknown planet. Of course, we run into difficulties trying to say just when one hypothesis is simpler than another. How complicated must the rule of gravitation become before we decide that it is simpler to look for another planet? I don't know. But fortunately, in most cases, one hypothesis is much simpler than the others. In our present example the rule would have become terribly complicated, so there was no doubt as to which was simplest. The only question was: Can we explain the deviations by stipulating the existence of a new planet (of the right size and in the right place)? When Leverrier showed that we could, this became the simplest hypothesis and was generally accepted even before the planet was actually sighted. We now believe in some sub-atomic particles, not because we have "seen" them (even indirectly), but because assuming their existence is the simplest hypothesis to explain the observed facts.

But how do we form the many different hypotheses from which we are to choose? To this there is no simple answer, since it is essentially a creative process. As soon as someone told us that a new planet could explain why Uranus misbehaved, it seemed most plausible. But how many of us would have thought of this possibility in the first place? How many of us would have thought that the motion of the moon and the falling of the apple are connected? How many of us would have thought that it takes no force to keep things moving, only to start them and stop them? How many of us would have guessed that the blood circulates in our veins? To select one of many hypotheses (once the facts are given) is a mechanical, even if lengthy, procedure; to think these hypotheses up in the first place is the work of genius.

There is one point on which I may be misleading you. When I emphasize the difficulty of thinking up hypotheses, you may get the impression that we must therefore choose from a very small number of possibilities. This is not so. One original idea may give rise to an infinite number of hypotheses. For example, when the idea of a new planet arises, there are the infinite number of different places where it may be (paths that it might follow), and an infinite number of sizes it may have. Let us just consider the distance of the new planet from Uranus, and its size; this already gives a double infinity of possibilities. Of course these are narrowed down by the facts, but there is still some choice: the farther away it is, the larger it must be to account for the deviations. So we start with an infinity of hypotheses, and it takes the most intricate mathematical argument to find just the right distance and just the right size to account for the path of Uranus. You may be interested in knowing that since Leverrier did not know some of the methods now available for this problem, his predictions were actually off by quite a bit.

So we see that the scientist actually thinks up infinitely many hypotheses (thanks to Mathematics), then notes which of these account for all known facts, and finally accepts the simplest remaining hypothesis as his theory.

DEDUCTION

The key to the verification of theories is that you never verify them. What you do verify are logical consequences of the theory. Verification is the process of seeing whether something predicted is really so. Since we can only observe particular facts, we must verify particular consequences of a theory, not the general theory itself.

In the case of Neptune, we could verify that there was a faint "star" at a certain location, and that the same "star" was at a somewhat different location two weeks later. But we could not verify directly how far it was from Uranus, what its path was, nor how large it was. We had to deduce some particular facts from the theory, which could be checked by direct observations.

In the second chapter we noted that logical deduction is no more than the analysis of the meaning of the theory. When we say that these facts follow, we mean that their truth is contained in the truth of the theory, even though we may not have realized this at the time we asserted the theory. When we assert that the sun rises every day, we understand that this implies its rising tomorrow. But few people, if any, would be able to look briefly at the General Theory of Relativity and see that the bending of light rays follows from it. You may feel that this is due to the fact that the theory is so complex. Then let us take Newton's very simple theory and see if it is obvious that planets move in ellipses. Or

is it obvious from the theory (together with some positions of the planet) where a certain planet must be tomorrow? Certainly not, it takes long chains of mathematical deductions to arrive at these conclusions.

I have stated that infinitely many facts are contained in a theory. But it often takes intricate mathematical analysis to bring these out. So the deductive step is designed to derive observable facts from the general theories. The theoretician starts with known facts and with the accepted theories, and finds out just what follows from them. If the theories are true, then every single statement that follows from them must be true! This gives us an unlimited wealth of facts which we can check, no end to the number of verified facts which we can accumulate in support of a given theory.

The only difficulty is that the interesting results are hardly ever the consequences of a single theory, but generally of a large number of theories. So even if the prediction turns out to be false, we are not certain which of the theories is wrong. We are, however, certain that *some* theory is incorrect. It is then again a question of finding the simplest way of improving our body of theories. In the Neptune example, we altered the theory as to the number of planets, but we could have altered Newton's law of gravitation, or even his law of motion, or the laws of optics as they apply to telescopes. We changed that which was simplest to change, but we can never be certain which theory was false. At the price of making the rest of the theories sufficiently complicated, we could rescue any theory. This is the reason why we often hear the claim that each experiment tests our entire body of knowledge.

Consider an everyday example. We have been hearing a great deal about flying saucers. Perhaps by the time you read this book, the mystery will be solved. The hypothesis has been advanced that they are missiles from outer space. What I want to show is that if I am determined to maintain that the saucers have an earthly cause, no amount of evidence can shake my belief. At the moment I can maintain that they are nothing but mass hallucinations. Recently they were spotted on radar. I could try to attribute this to hallucination on the part of the radar operator. If there are too many people who see the image on the screen, I could invent an electronic effect, say, caused by too many television senders, which produce both the "saucers" and the radar images. Of course, this is likely to contradict what we know about electronics, but if I am willing to modify enough theories, I can change the electromagnetic theory, and save my pet hypothesis. If such a missile is actually shot down, I would have to abandon the hypothesis that it is a hallucination, but I could claim that it came from another country on the earth. If it turns out that there is a living being inside the "saucer," different from all we know, I could stipulate that he came from an unexplored island, or even from below the surface of the earth. Of course, to allow for life at the high temperatures below the earth, I would have to modify several theories, but if I am willing to do this, I can still save my pet hypothesis. If one of these "Martians" takes me into the plane and carries me into outer space, I can say that the machine simply took me on a rocket trip and showed me a movie which looked as if I were really looking at the earth disappearing in the distance. Even if we landed on Mars, I could explain this by the great hypnotic power he has over me.

If you got impatient with my skepticism, it was because there comes a point where accepting the fact that we have interplanetary travelers becomes simpler than modifying fundamental theories. But I hope I have convinced you that it is logically possible to save any given theory by giving up others. The reason for this is the following: In checking a theory, we must derive a consequence of the theory, which can be verified by observations. These consequences must make use of several theories, and if they do not check with experience, it is a question whether it is our theory or one of the others that is wrong. We can always suppose the latter, as was shown in the flying-saucer example. This is why the predictions are based on our entire body of knowledge, and why it is best to say that we test this whole body, rather than a particular theory.

VERIFICATION

This third step of the scientific method is similar to the first one: we gather facts. In this case, however, the facts to be observed were predicted, and we "just" see whether they are so or not.

I have tried to show that it is an oversimplification to say that one unfavorable observation can disprove a theory. This is an oversimplification for two reasons: First, the observations and the predictions are only approximate, so that we can make only probability statements (see the last chapter). Secondly, the predictions are based on several theories, and hence there is a choice as to which theory to reject (as was shown in the previous sections). So an unfavorable observation can only make the theory unlikely, or rather it makes the body of theories as a whole unlikely (more precisely, it is unlikely that the whole body is true).

How about favorable observations? First of all, these too are only approximate, so that we are never certain that the prediction was verified. Secondly, the fact that one prediction, or any limited number of predictions, has been verified does not make the theory certain. There always remain an infinity of competing hypotheses, all of which can explain all the known facts. So in this case too we can make only probability statements.

The probabilities that I have discussed here are of the second kind, credibilities. The process of verification consists in checking predictions against observations, and assigning greater or lesser credibility to our body of theories on the basis of the outcome. If the credibility is high, we are satisfied. If it is below a reasonable level, we modify our theories; we must change at least one theory so that our total body will then have a higher credibility. This may mean a relatively minor change, like admitting a new planet, or it may mean replacing the entire structure, as Relativity Theory replaced Newton's System. The decision as to when a change is necessary is complicated, and, in the absence of a good measure of credibility, is highly controversial. It is further confounded by the necessity of taking the simplicity of our theories into account. We consider the credibility of our theories and of competing ones. We abandon our theories either if some other body is much more credible on the given evidence, or if a simpler body can be found which is roughly as credible as ours.

A most interesting illustration of this point is Henri Poincaré's claim that Euclidean Geometry would never be abandoned. Poincaré was an excellent mathematician and philosopher of science. He certainly understood that there was a fundamental difference between pure, abstract Mathematics, and the interpreted version of the same, which is a branch of Science. He also understood the need for selecting the simplest possible theory. But he showed, by an ingenious argument, that one can always salvage Euclidean Geometry (properly interpreted), no matter what facts Physics reveals. He then argued that this Geometry is so much simpler than its competitors that scientists will always stick to this Geometry, and modify their other laws (of Physics) if necessary. I will just give you one example of how this can be done. It seems that there is such a fundamental difference between the finite universe of the Geometry we now favor, and the infinite universe of Euclidean Geometry, that this point should be decidable by experiment. If a ray of light can come back to its origin, then the universe is finite and closed; if it cannot, then it is infinite and open. But we can get around either possibility by modifying our other laws. Suppose a ray of light comes back after a long time (for example, we recognize, somehow, our own gallaxy in the far distance of the universe), this could be explained by stipulating that light travels not in a straight line, but in a very large circle; a circle that is so large that small portions of it seem straight. Then light could come back even in an infinite universe.

If light cannot come back, we can still hang on to our finite, closed universe. We stipulate that the universe expands (as we do stipulate) and that it expands just rapidly enough that light can get closer and closer to our "antipode" (the opposite point of the universe), but can never quite reach it, since it is running away from us with the speed of light. Indeed, some cosmological theories point to this possibility. Then light cannot return even in a finite universe. The latter case is interesting from another standpoint. If in this case we measure lengths as usual, with rulers or their equivalents, the universe is finite. But if we measure distances by

the amount of time it takes light to reach from one end to the other, then the universe becomes infinite—since light can never reach the opposite "pole." Hence we can salvage one or the other Geometry, depending on the way we interpret distances.

Poincaré drew the conclusion that for reasons of simplicity Science will always keep Euclidean Geometry. He published his beliefs in a book that appeared in 1904. In 1905 Einstein's first installment of Relativity Theory appeared, a later installment of which caused us to abandon Euclidean Geometry. The dates here are quite ironic, but they do not mean that so great a thinker as Poincaré made a very bad mistake. What he overlooked was that saving the simplest Geometry might be achievable only at the price of a terrible complication in the other theories. The criterion of simplicity must be applied to our entire body of knowledge. When Einstein found that the theory explaining all the known facts (the Special Theory of Relativity) could be considerably simplified by adopting a non-Euclidean Geometry, he did not hesitate to do so. Thus the General Theory of Relativity was born. Newton's theory was abandoned because of its lack of agreement between predictions and observations. The Special Theory was abandoned because there was a simpler theory explaining the same facts. In one case, the credibility became too low; in the other case, the credibility was high enough, but there was a competing theory with about the same credibility, which was much simpler. This brings out clearly the interplay of credibility (probability) and of simplicity in the verification and consequent acceptance or rejection of theories.

A CASE HISTORY

For the concluding illustration of an application of this method, I turn to one of the greatest masters of the scientific method in history, Mr. Sherlock Holmes.

The one regrettable fact about our record of his activities is that we know him only through the eyes of the somewhat imperfect, if most likable, Dr. Watson. Among other shortcomings we find that the good doctor had his terminology twisted as far as the scientific method is concerned. He has the annoying habit of referring to Mr. Holmes' remarkable inductions (forming of far-reaching theories on scant evidence) as deductions, and of describing the scientific method—so nobly practiced by the immortal master—as the science of deduction. But never mind, let us take one of the fascinating cases and forget the terminology.

Almost any one of the adventures would serve as an illustration. I have, quite arbitrarily, selected the case of "The Red-headed League."

First of all Mr. Holmes collects facts, in this case from the narrative of a Mr. Wilson. It seems that Mr. Wilson's attention was drawn by his assistant to a strange advertisement calling for a red-headed person to collect a fairly nice salary for nominal work. Since Mr. Wilson's pawnshop is not doing well, and since he has a fine head of flaming red hair, he jumps at the opportunity. Although he is one of some thousand applicants, he is fortunate enough to get the job. It turns out that an eccentric English-born American millionaire has left a provision in his will providing for fellow redheads. All that Mr. Wilson has to do is to copy the *Encyclopaedia Brittannica,* only he must do this in an office specifically provided for this purpose. He does this, and collects a handsome fee supplementing his income, until one day (some eight weeks later) he finds the office closed and can find no trace of his employer. During Mr. Holmes' interrogation, Mr. Wilson states that his pawn-shop assistant is a most intelligent young man who has agreed to work for half-pay in order to learn the trade. The only unusual trait of his assistant is his love for photography, which causes him to spend a great deal of time in the cellar developing pictures.

Mr. Holmes has to explain the motivation for the strange employment, and also one or two peculiarities in the assistant. He formulates the hypothesis that the motives are criminal, and that the assistant has a hand in whatever crime is planned. The only purpose the job accomplished was to have Mr. Wilson out of the house; not only was he not defrauded, he actually gained a small sum. Yet there is nothing valuable in the house. The theory formed is that the part of the house which interests the assistant is the cellar (photography being an excuse only) and that the

criminals want to dig a tunnel from the cellar in Mr. Wilson's absence. The fact that he was "fired" suggests that the tunnel is complete, and the fact that the crime has not yet been committed suggests that it will take place in the immediate future.

Mr. Holmes is now in the position of making some of his remarkable predictions. He can predict that there never was a will (which is verified), that there must be an important building easily accessible from the pawnshop's cellar (it turns out to be a bank), and that a robbery is to be attempted in the next few days. He further formulates the theory that it will be Saturday night, to give the robbers extra time to escape before detection. He has all his theories verified by catching the criminals just as they break in through the cellar of the bank.

This is an admirable application of the scientific method to the science of detection. A careful accumulation of facts is followed by the formation of ingenious theories. From these facts logical conclusions are drawn, which are verified one by one, until Mr. Holmes is certain of his theory (or as certain as a human can ever be). Then he can safely predict one more event, this prediction leading to the dramatic climax of the case. To those of you who still feel that there is something miraculous in the scientific method, I will give the master's own answer: "Elementary, dear Watson!"

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- Lenzen.
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6

Credibility and Induction

"This conversation is going on a little too fast: let's go back to the last remark but one."

LET US RETURN to the problem raised in the last chapter but one. I have tried to show how fundamental the concept of *credibility* is to the problems of induction and to the whole scientific method. We must now consider this problem in greater detail.

THE PROBLEM OF EXPLICATION

There is no doubt that scientists assign probabilities to theories. They will say that one theory is very probably true, while another is very poorly confirmed and hence not so likely. They will consider two or more alternate hypotheses, and decide which is most likely to be true (on the given evidence). But they have no way of computing these probabilities and there are often considerable arguments as to which of two theories is more probable.

We face the same problem in everyday life. Two racing fans will consult the same dope-sheet, and arrive at different conclusions as to the likelihood of Double Negation winning the fifth race. The first decides that the horse is almost sure to win, and hence bets his shirt, while the latter only assigns an even chance to him, and hence bets on a horse giving longer odds. Double Negation wins by two lengths. Which man was right? The first man has a lot of money to back up his claim, but he has no way of showing that the odds were not even.

10

What Is Science?

"Are we nearly there?" Alice managed to pant out at last.
"Nearly there!" the Queen repeated, "Why we passed it ten minutes ago!"

ONE NATURAL WAY to start a book on the Philosophy of Science is with a definition of Science. However, such a definition would have to be superficial. A much better definition can be given after a great deal of other material has been clarified. By this time the reader is probably ready to ask Alice's question, "Are we nearly there?" We are now in a position to give the same answer that the Queen gave to Alice.

WHAT UNITES SCIENCE

Our alternatives are to define Science by its subject matter, or by its method. But the purpose of Science is to study the whole field of factual knowledge; it has no special topic of its own. Yet we certainly do not classify every study of facts as Science. For example, we refuse to admit Astrology into the family of sciences. Astrology studies facts; it studies the position of stars, and various events in human life, and tries to establish connections between them. The reason that we reject it as a science is not due to its subject matter, but because we consider the methods used by astrologers unscientific. Whenever we find a branch of supposed factual knowledge rejected by Science, it is always on the basis of its method.

Let us look at the other side of the argument. Is every application of the scientific method really a case of Science? The argument to be presented is that every such case can legitimately be called Science, though it is admitted that sometimes this is disputed. There are two types of applications of the method which many scientists would consider outside Science: every-day-life applications and applications by "nonscientists" like criminologists. Custom prevails in these cases: they are not sufficiently "dignified" to be classed as sciences. Perhaps it would be best to state the definition of "Science" to include only important applications of the method, but there is no good way of defining what an important application is. I find it best to consider Science in the broad sense, and call applications to every-day-life Science on an elementary level.

No one can *prove* what the right way of defining "Science" is, but we can argue about the most useful way. Since there is disagreement about the use of the word, our definition cannot agree with all different uses, but we can have several guiding principles: (1) Whenever there is a consensus as to whether some field belongs to Science, our definition must agree with the accepted verdict. (2) In cases where there is considerable disagreement, our definition must settle the dispute. (3) Of the many different ways of settling the disputes, ours must be one that leads to a useful concept. (4) The definition should be as simple as possible. These are the four conditions for the explication of a vague intuitive concept (see Chapter 6). I feel that defining Science by its method is the only definition satisfying these conditions.

The definition of Science by its method certainly agrees with usage whenever there is a clear-cut agreement; it is a simple definition, and it decides disputed cases according to an important principle (whether the procedure used was according to the rules of scientific method), and hence leads to a useful concept of Science. For this reason I shall use the word "Science" to be all knowledge collected by means of the scientific method.

The principal rival to the definition here given would be a definition according to common usage. In this we would poll the leading experts to tell us how they use the word "Science." Such a definition would suffer from all the prejudices of practicing scientists and would be sufficiently vague to destroy its basic fruitfulness. It is a useful precept in explication to err on the side of simplicity rather than on the side of common usage. For this reason I shall reject the definition by common usage and adopt the definition according to method.

There is an apparent circularity in this definition. Doesn't the definition of "Science" use the term "scientific (method)"? It does, but there is no circularity, since the definition of the method was given without using the term "Science." This is a very common procedure; for example, we are likely to define "Mathematics" as the study of mathematical laws, and then we give an independent definition of what such a law is; or we would define "happiness" as the feeling experienced by a happy person. We defined the scientific method by the cycle of induction, deduction, and verification, and by its eternal search for improvement of theories which are only tentatively held. Nowhere in this definition have we used the term "Science," so we can use the definition in turn to define "Science."

Just as a check on the definition we might inquire whether this method is really used in all branches of Science, and the best way of seeing this is through examples chosen from a variety of branches. The example discussed at length in Chapter 5 (discovery of Neptune) was from Physics. From Chemistry we might select the history of the Phlogiston theory. It was held for some time that burning consists in the giving off of a substance, and this substance was called phlogiston. But if this theory is right, then burning should reduce the weight of the burning material. When Lavoisier showed that, on the contrary, it gains in weight, then the old theory was rejected and the new theory (that a substance is taken in during burning) was formed to explain the facts. From this theory many consequences were deduced, and verified. For example, in a closed space there can be only a limited amount of this substance (oxygen) in the air, so when this is used up nothing more can burn in this space until fresh air is let in. We can easily verify this by putting a candle under an inverted glass, and watching it go out long before the candle is completely burned. This completes one cycle of the scientific method.

From Biology we will choose the discovery of Mendel's laws. Mendel observed over a period of years the variety of plants that he grew in his small garden. From these he formed generalizations concerning the proportion of various traits in the offspring, as determined by the traits of the parents. From these laws we can make sweeping predictions as to the results of breeding experiments, which have been repeatedly confirmed and have led to considerable profit for farmers throughout the world.

It is harder to find good examples of the scientific method in Psychology. However, recent developments in Learning Theory will furnish us with examples. Very interesting theories have been developed by R. R. Bush and F. Mosteller, on the one hand, and by W. K. Estes, on the other hand. These theories start with data collected in simple experiments in which the experimentor attempts to teach a rat, a goldfish, or a human being to learn how to do a task. The two theories provide alternate (and related) models as to how the subject learns to perform these tasks, and from these theories predictions can be made about the outcome of the experiments. Predictions may concern the average time it takes a subject to learn the experiment or the number of errors he will commit before learning how to do the task perfectly, or even whether he will ever learn to do the task perfectly. In the simplest cases these predictions are in excellent agreement with experiments.

The movies provided us with a good case from the Social Sciences. The record of the Kon-Tiki expedition is a gallant testimony to the future the scientific method has in this field. Certain similarities between the ancient traditions of natives in the South Sea islands and of inhabitants of South America led a group of sociologists to form the theory that these natives came from South America, not from the much nearer shores of Asia. This theory was disputed, because it seemed impossible that a thousand years ago these primitive people would have been able to undertake such a journey of several months over the open sea. The scientists deduced from their theories that the type of primitive craft (a loosely constructed raft with poor facilities for steering and only a limited place for storing food) must be capable of completing such a journey. They risked their lives to test this theory, by actually attempting the trip on just such a raft. They found out a great deal not known before. They found a plentiful supply of palatable fish in these unknown waters, they found that the looseness of the raft prevented flooding, and they found that, while steering was impossible, the prevailing currents carried them unerringly to their destination. Over a hundred days later they arrived on these islands and were greeted by the natives who related their old legend according to which the great god Kon-Tiki had brought their ancestors to the islands on just such a craft. Thus they verified their theory (which previously was in general disrepute) and completed a thrilling chapter from the history of the application of the scientific method.

WHAT DIVIDES SCIENCE

We have seen that Science is united not by its subject matter, but by its method. We will now see that it is the subject matter that divides Science into branches.

It is very difficult to explain the reasons for dividing Science the way we do, especially since there is no really good reason. Let us compare the various branches of Science to the various colors. There are five basic colors—red, yellow, green, blue, and violet. Someone else will tell you that there are six or seven, adding orange and/or indigo. That still leaves us with unclassified mixed colors, like pink or brown, not to mention white which is a mixture of all other colors. Even among the basic colors it is difficult to classify all shades. Let us pick a definite shade of blue-green, and we will get quite an argument as to whether it is green or blue. It is even more difficult to distinguish between shades of blue, indigo, and violet.

Quite similarly, we will get an argument as to what the fundamental branches of Science are. Physics, Chemistry, Biology, Psychology, and the Social Sciences form a common list, but others will add Astronomy, and divide the Social Sciences into Economics, Sociology, and Politics. That still leaves us with "mixtures" like Biophysics or like History (insofar as History is made scientific). We have borderline cases too; for example, it is sometimes difficult to say of viruses whether they are alive (and hence belong to the subject-matter of Biology) or whether they are inanimate molecules (and hence belong to Chemistry).

In the case of colors we know that there is a continuous scale, and that explains why there is no natural division into five, seven, or any small number of distinct colors. In addition, we can get an endless variety of secondary shades by mixing the primary ones. We know that it is the difference in wave lengths that differentiates between them, and hence it makes sense to speak of one (pure) shade being closer to a given shade than to another. But any division into colors is highly arbitrary. We do not have as complete a picture of the structure of Science, since our knowledge of theories is incomplete. Scientists will study a group of phenomena which seem related, and try to connect them by means of a theory. Sometimes they fail, and at other times they succeed. In the latter case we have a branch of Science. But there is a great deal of arbitrariness in this procedure. After all, we know that *all* phenomena are connected through The Law of Nature. The laws we are looking for are partial laws, and we are likely to find them where we look for them. Kepler would never have found a connection between the falling of an apple and the motion of the planet Mars, because he never looked for such a connection. It was left to Newton to connect Astronomy with Physics. The ancient Greeks saw a sharp boundary between them; in the heavens circular motion was "the law," while on earth things moved in a straight line. As we learned more, Astronomy became more and more incorporated into Physics. There are still unexplained phenomena, so there is still some excuse for an autonomous Astronomy.

Whenever there is a great deal of arbitrary choice, accidents take a hand in the decision. Whether your university has a separate Astronomy department may depend on whether some rich alumnus has a secret passion for Astrology, but, not being allowed to endow a chair in fortune-telling, he leaves a few millions to finance an Astronomy department. Or, on the other hand, the professor of Astronomy may have the ambition of becoming head of the entire Physics department, and hence there is a merger. I even know of a strange case where Astronomy is part of the Mathematics department.

We witness similar factors in the ordering of shades of color. Suppose you like blue, but dislike green as a rule. If confronted by a shade of green-blue, you are likely to classify it blue or green, according to whether you like the shade or not. Historical factors may have a considerable influence too. If the same person works on two apparently different types of phenomena, these may both be assigned to the same science. Perhaps the fact that Newton studied both Mechanics and Light may account for their becoming branches of Physics, while the fact that he did not contribute to the study of chemical compounds may account for the independent status of Chemistry.

We may summarize the foregoing discussion by stating that Science is divided into branches arbitrarily. If phenomena are connected by known laws, or if some scientist attracts sufficient interest in the study of these phenomena, or for a number of accidental reasons, a group of phenomena is collected into a branch of science. It is dangerous to place too much emphasis on such arbitrary divisions.

REASONS FOR DIVIDING SCIENCE

There are two schools of thought according to which one can motivate the division of Science into branches.

One school believes that eventually a unified Science will be possible and has definite views as to how it will come about. This school will give a division of Science somewhat as follows: Physics, Chemistry, Biology, Psychology, the Social Sciences. They feel that through the progress of Science the "higher" disciplines will become branches of the lower ones and eventually all of Science will be *reduced* to Physics. The Social Sciences are to be reduced to Psychology by explaining the actions of a group on

the basis of the individual psychology of its members. Psychology is to become a part of Biology and the workings of the human mind explained in terms of the workings of the human body. The human body again is to be considered as made up of certain chemicals and subject to various laws of Chemistry, and hence Biology is to become a branch of Chemistry. Finally Chemistry is to be reduced to Physics, a process that is already fairly well completed. In this manner all of Science will be united as a great, expanded Physics.

It is pointed out that in each case the behavior of the whole is explained in terms of the behavior of its parts. This fact is considered significant by this school of thought. Human groups are to be considered as wholes made up of individual parts. These individuals are in turn made up of cells, the cells are made up of chemicals, and these in turn are made of atoms. Many precedents are cited where the behavior of wholes has been explained by Science in terms of the behavior of their parts. It is further pointed out that the lower sciences have wider applicability than the higher ones. For example, the theories of Physics have universal applicability and are used by all sciences. The Law of Gravitation applies equally well to a stone or to a cat, when one of these objects is thrown out of a window. However, Mendel's laws are applicable only to the cat and not to the stone. Thus the ordering given above leads from more general theories to more specialized ones.

We now have a threefold reason for dividing Science, and for ordering the branches in the given manner. First of all there is the expectation of reducing the later branches to the earlier ones. Secondly, the later branches treat wholes whose parts are treated by earlier branches. Finally, there is successively more specialization as we go through the order of the branches.

Although this kind of division certainly is reasonable for many purposes, it must be pointed out that the reasons given are highly oversimplified. For example, the manner in which Biology makes use of the Laws of Gravitation is entirely different from the manner in which the Social Sciences make use of Psychology. The Law of Gravitation will apply to a biological object as a whole, as well as to any one of its parts. However, psychological laws will apply only to the parts of a social group, certainly not to the whole, unless we decide to use "Psychology" in two different senses. Again there is no good reason why the lower sciences should not utilize theories from the higher disciplines. For example, while the Theory of Evolution makes use of Geology (which we may take to be a mixture of Physics and Chemistry), Geology in turn is helped out by results taken from the Theory of Evolution. One of the most useful tools in modern Geology is the dating of rocks by means of the fossils contained in them. The ordering presented is very neat as long as we do not look at too many examples that fail to fit into this order. For example, we might ask whether Nuclear Physics is a separate branch, and we would have a very difficult time placing Astronomy into the neat order. Even the basic principle that wholes are reduced to their parts must be taken as a great oversimplification. In what sense are the objects of Economics reduced to a study of their parts, and how can the reduction of any objects of the higher disciplines to a field theory (such as the Theory of Relativity) fit into this scheme?

Oddly enough, a second school of thought supports a division of Science precisely because they do *not* believe in the possibility of reduction. Vitalists will separate Biology from the lower sciences because they believe that, in some sense, there is a sharp boundary between them. This will be discussed in Chapter 12. Similarly, dualistic philosophers will maintain a division between Psychology and Biology as we will point out in Chapter 13.

I do not want to enter these disputes at the present moment. Let us be satisfied with stating that, for many purposes, a simple division of Science into branches is very useful, but we have found no sufficient reason for assigning deep significance to this classification. We conclude that Science is an enormous area of human research which is united by a common method. Its divisions are for convenience in describing results and do not represent a fundamental feature of Science.

SUGGESTED READING

Complete references will be found in the Bibliography at the end of the book.

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