PART 2

INDUCTION AND CONFIRMATION
The Nature of Scientific Inference

Scientific theories are based on empirical evidence. The data they try to explain have been obtained through observation and experiment, not wishful thinking or divine revelation. But what sort of relationship must exist between data and theory in order for the data to support the theory? When are we justified in believing that a theory is true?

Two means of evidential support have traditionally been recognized by philosophers of science: deduction and induction. A theory is deductively supported by its evidence if it logically follows from that evidence. A theory logically follows from its evidence just in case it's impossible for the evidence to be true and the theory false. For example, the statement "Some birds are animals" logically follows from the statement "Some animals are birds." If the latter statement is true, the former has to be true. Because the truth of the latter statement guarantees the truth of the former, deductive support is said to be "truth preserving." Any theory that can be deduced from true evidence must itself be true.

Most scientific theories cannot be deduced from their evidence. Some can be induced from their evidence, however. If every raven that has ever been observed has been found to be black, we may use induction to arrive at the conclusion that, probably, every raven that ever will be observed will be black. Inductive inference does not guarantee the truth of its conclusion, however, for no matter how many ravens we observe, there is always the chance that there is a nonblack one that we did not observe. Nonetheless, induction is able to establish the truth of certain statements with a high degree of probability. The following problem arises, however: if induction does not guarantee the truth of its conclusions, how can science give us knowledge about the world? This is the problem that David Hume addresses.

Enumerative induction has the form "Every *A* that has been observed has been found to be *F*. Therefore, every *A* that ever will be observed will be found to be *F*." Hume realized that this form of inference assumes that the future will resemble the past. But what justifies our believing that? We can't provide a deductive argument for the claim that the future will resemble the past because there is no more fundamental claim from which it logically follows. Nor can we provide an inductive argument for that claim because such an argument would be circular: it would assume what it is trying to prove. So there appears to be no way to justify induction. The belief that the future will resemble the past is not something we arrived at through a process of inference. Rather, it is a bias that is built into our thinking.

The scientific method is often thought to consist of four steps: (1) Observe and record all the relevant facts, (2) analyze and classify those facts, (3) use induction to derive generalizations from the facts, and (4) test the generalizations. Carl Hempel calls this the "narrow inductivist" conception of scientific inquiry and objects to it on the grounds that scientists do not and cannot follow it. Facts cannot be observed or analyzed in the absence of a hypothesis, and induction is rarely used to generate hypotheses. It can be used to generate certain simple hypotheses, such as "Whenever copper is heated, it expands"; but it can't be used to generate more complex hypotheses, such as "Matter is composed of atoms," because when that hypothesis was introduced, atoms had not been observed. Scientific hypotheses often postulate the existence of unobserved entities to explain the observed. The conclusion of an enumerative inductive inference, however, can't make reference to things not covered by the data. So enumerative induction cannot generate hypotheses that contain novel concepts or ideas.

If scientific hypotheses are not derived from the data, how are they arrived at? Hempel claims that they are invented to account for the data. Since most hypotheses go beyond their data in various ways, there can be no set of rules for generating them. Hypotheses simply represent one's best guess about the way things are. Hempel claims that the scientific method is the "method of hypotheses," or what has come to be known as the "hypothetico-deductive method." This method consists of three steps: (1) Invent a hypothesis, (2) deduce a test implication from the hypothesis, and (3) perform the test. A test implication is a statement that should be true if the hypothesis in question is true. This statement can usually be expressed as a conditional statement saying that if certain conditions are realized, then certain things should be observed. Scientists test their hypotheses by creating those conditions in the laboratory or by locating them in the field and determining whether what they find is what the hypothesis predicted. If things are as the hypothesis says they should be, then the hypothesis has been confirmed. This does not establish the truth of the hypothesis, but the more tests it has successfully passed, the more likely it is to be true.

According to the hypothetico-deductive method, the successful test of a hypothesis has the following form:

If *H*, then *P*.
P.
______
Therefore, (probably) *H*.

where *H* stands for the hypothesis being tested, and *P* stands for the test implication. From a logical point of view, however, this inference is suspect because it commits the fallacy of affirming the consequent. To see this, consider this inference: If it rained, the streets are wet. The streets are wet. Therefore, it rained. The conclusion of this argument doesn't logically follow from the premises because it's possible for the premises to be true and the conclusion false. For example, the streets could have become wet because a water main broke, a spring water truck tipped over, or the street washer came by. Even the claim that it probably rained is problematic because until we know the relative likelihood of the other possibilities, we are not justified in claiming that it probably rained. (In the desert, one of these other possibilities might be more likely.)

Because of the difficulty of assessing the relative probabilities of various possibilities, Karl Popper claims that inductivism, in either the narrow or the wide sense, is untenable. Instead he offers a view that he calls "deductivism." In this view, the job of the scientist is not to confirm hypotheses, but to refute them. He agrees with Hempel that hypotheses are invented rather than discovered. But he disagrees with Hempel's claim that hypotheses are made more likely by successful tests. If a hypothesis successfully passes a number of tests, the best we can say is that it has been "corroborated."

If a theory fails to pass a test, however, we can reject it out of hand. This rejection is justified because it is the result of a valid inference. An unsuccessful test has the following form:

If *H*, then *P*.

Not *P*.

Therefore, not *H*.

This form of argument—known as denying the consequent—is not suspect because the conclusion logically follows from the premises. To see this, consider again the rain example. If it rained, the streets are wet. The streets are not wet. Therefore it didn't rain. In this case, it's impossible for the premises to be true and the conclusion false. In Popper's deductivist conception of science, induction plays no role. Thus Popper's conception avoids the problem of induction.

Popper's deductivism depends on a critical assumption: hypotheses can be tested in isolation from other beliefs we hold. But Pierre Duhem convincingly argues that hypotheses have testable consequences only in the context of certain background assumptions. These background assumptions provide information about the objects under study as well as the apparatus used to study them. If a test is unsuccessful, we can always save the hypothesis by rejecting one or more of the background assumptions. So it appears that hypotheses can neither be conclusively verified nor conclusively falsified.

For any set of data, it is possible in principle to construct an infinite number of explanations to account for those data. For example, think of all the different lines that can be drawn through a set of data points on a graph. So when we ask the question "Why did this happen?" it may be difficult to know how to go about answering it. But if we ask the question "Why did this happen rather than that?" we've narrowed the field of possible answers and provided a focus for our inquiry. Peter Lipton claims that the method of inference to the best contrastive explanation more accurately reflects the actual practice of scientists than either Hempel's or Popper's model.

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7

DAVID HUME

The Problem of Induction

When it is asked, *What is the nature of all our reasonings concerning matter of fact?* the proper answer seems to be, that they are founded on the relation of cause and effect. When again it is asked, *What is the foundation of all our reasonings and conclusions concerning that relation?* it may be replied in one word, Experience. But if we still carry on our sifting humor, and ask, *What is the foundation of all conclusions from experience?* this implies a new question, which may be of more difficult solution and explication. Philosophers, that give themselves airs of superior wisdom and sufficiency, have a hard task when they encounter persons of inquisitive dispositions, who push them from every corner to which they retreat, and who are sure at last to bring them to some dangerous dilemma. The best expedient to prevent this confusion, is to be modest in our pretensions; and even to discover the difficulty ourselves before it is objected to us. By this means, we may make a kind of merit of our very ignorance.

I shall content myself, in this section, with an easy task, and shall pretend only to give a negative answer to the question here proposed. I say then, that, even after we have experience of the operations of cause and effect, our conclusions from that experience are *not* founded on reasoning, or any process of the understanding. This answer we must endeavor both to explain and to defend.

It must certainly be allowed, that nature has kept us at a great distance from all her secrets, and has afforded us only the knowledge of a few superficial qualities of objects; while she conceals from us those powers and principles on which the influence of those objects entirely depends. Our senses inform us of the color, weight, and consistence of bread; but neither sense nor reason can ever inform us of those qualities which fit it for the nourishment and support of a human body. Sight or feeling conveys an idea of the actual motion of bodies; but as to that wonderful force or power, which would carry on a moving body for ever in a continued change of place, and which bodies never lose but by communicating it to others; of this we cannot form the most distant conception. But notwithstanding this ignorance of natural powers[1] and principles, we always presume, when we see like sensible qualities, that they have like secret powers, and expect that effects, similar to those which we have experienced, will follow from them. If a body of like color and consistence with that bread, which we have formerly eaten, be presented to us, we make no scruple of repeating the experiment, and foresee, with certainty, like nourishment and support. Now this is a process of the mind or thought, of which I would willingly know the foundation. It is allowed on all hands that there is no known connection between the sensible qualities and the secret powers; and consequently, that the mind is not led to form such a conclusion concerning their constant and regular conjunction, by anything which it knows of their nature. As to past *Experience,* it can be allowed to give *direct* and *certain* information of those precise objects only, and that precise period of time, which fell under its cognizance: but why this experience should be extended to future times, and to other objects, which for aught we know, may be only in appearance similar; this is the main question on which I would insist. The bread, which I formerly ate, nourished me; that is, a body of such sensible qualities was, at that time, endued with such secret powers: but does it follow, that other bread must also nourish me at another time, and that like sensible qualities must always be attended with

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An Inquiry Concerning Human Understanding, Section IV, Part II.

like secret powers? The consequence seems nowise necessary. At least, it must be acknowledged that there is here a consequence drawn by the mind; that there is a certain step taken; a process of thought, and an inference, which wants to be explained. These two propositions are far from being the same, *I have found that such an object has always been attended with such an effect,* and *I foresee, that other objects, which are, in appearance, similar, will be attended with similar effects.* I shall allow, if you please, that the one proposition may justly be inferred from the other: I know, in fact, that it always is inferred. But if you insist that the inference is made by a chain of reasoning, I desire you to produce that reasoning. The connection between these propositions is not intuitive. There is required a medium, which may enable the mind to draw such an inference, if indeed it be drawn by reasoning and argument. What that medium is, I must confess, passes my comprehension; and it is incumbent on those to produce it, who assert that it really exists, and is the origin of all our conclusions concerning matter of fact.

This negative argument must certainly, in process of time, become altogether convincing, if many penetrating and able philosophers shall turn their inquiries this way and no one be ever able to discover any connecting proposition or intermediate step, which supports the understanding in this conclusion. But as the question is yet new, every reader may not trust so far to his own penetration, as to conclude, because an argument escapes his inquiry, that therefore it does not really exist. For this reason it may be requisite to venture upon a more difficult task; and enumerating all the branches of human knowledge, endeavor to show that none of them can afford such an argument.

All reasonings may be divided into two kinds, namely, demonstrative reasoning, or that concerning relations of ideas, and moral reasoning, or that concerning matter of fact and existence. That there are no demonstrative arguments in the case seems evident; since it implies no contradiction that the course of nature may change, and that an object, seemingly like those which we have experienced, may be attended with different or contrary effects. May I not clearly and distinctly conceive that a body, falling from the clouds, and which, in all other respects, resembles snow, has yet the taste of salt or feeling of fire? Is there any more intelligible proposition than to affirm, that all the trees will flourish in December and January, and decay in May and June? Now whatever is intelligible, and can be distinctly conceived, implies no contradiction, and can never be proved false by any demonstrative argument or abstract reasoning *à priori.*

If we be, therefore, engaged by arguments to put trust in past experience, and make it the standard of our future judgment, these arguments must be probable only, or such as regard matter of fact and real existence, according to the division above mentioned. But that there is no argument of this kind, must appear, if our explication of that species of reasoning be admitted as solid and satisfactory. We have said that all arguments concerning existence are founded on the relation of cause and effect; that our knowledge of that relation is derived entirely from experience; and that all our experimental conclusions proceed upon the supposition that the future will be conformable to the past. To endeavor, therefore, the proof of this last supposition by probable arguments, or arguments regarding existence, must be evidently going in a circle, and taking that for granted, which is the very point in question.

In reality, all arguments from experience are founded on the similarity which we discover among natural objects, and by which we are induced to expect effects similar to those which we have found to follow from such objects. And though none but a fool or madman will ever pretend to dispute the authority of experience, or to reject that great guide of human life, it may surely be allowed a philosopher to have so much curiosity at least as to examine the principle of human nature, which gives this mighty authority to experience, and makes us draw advantage from that similarity which nature has placed among different objects. From causes which appear *similar* we expect similar effects. This is the sum of all our experimental conclusions. Now it seems evident that, if this conclusion were formed by reason, it would be as perfect at first, and upon one instance, as after ever so long a course of experience. But the case is far otherwise. Nothing so like as eggs; yet no one, on account of this appearing similarity, expects the same taste and relish in all of them. It is only after a long course of uniform experiments in any kind, that we attain a firm reliance and security with regard to a particular event. Now where is that process of reasoning which, from one instance, draws a conclusion, so different from that which it infers from a hundred instances that are nowise different from that single one? This question I propose as much for the sake of information, as with an intention of raising difficulties. I cannot find, I cannot imagine any such reasoning. But I keep my mind still open to instruction, if any one will vouchsafe to bestow it on me.

Should it be said that, from a number of uniform experiments, we *infer* a connection between the sensible qualities and the secret powers; this, I must confess, seems the same difficulty, couched in different terms. The question still recurs, on what process of argument this *inference* is founded? Where is the medium, the interposing ideas, which join propositions so very wide of each other? It is confessed that the color, consistence, and other sensible qualities of bread appear not, of themselves, to have any connection with the secret powers of nourishment and support. For otherwise we could infer these secret powers from the first appearance of these sensible qualities, without the aid of experience; contrary to the sentiment of all philosophers, and contrary to plain matter of fact. Here, then, is our natural state of ignorance with regard to the powers and influence of all objects. How is this remedied by experience? It only shows us a number of uniform effects, resulting from certain objects, and teaches us that those particular objects, at that particular time, were endowed with such powers and forces. When a new object, endowed with similar sensible qualities, is produced, we expect similar powers and forces, and look for a like effect. From a body of like color and consistence with bread we expect like nourishment and support. But this surely is a step or progress of the mind, which wants to be explained. When a man says, *I have found, in all past instances, such sensible qualities conjoined with such secret powers:* And when he says, *Similar sensible qualities will always be conjoined with similar secret powers,* he is not guilty of a tautology, nor are these propositions in any respect the same. You say that the one proposition is an inference from the other. But you must confess that the inference is not intuitive; neither is it demonstrative: Of what nature is it, then? To say it is experimental, is begging the question. For all inferences from experience suppose, as their foundation, that the future will resemble the past, and that similar powers will be conjoined with similar sensible qualities. If there be any suspicion that the course of nature may change, and that the past may be no rule for the future, all experience becomes useless, and can give rise to no inference or conclusion. It is impossible, therefore, that any arguments from experience can prove this resemblance of the past to the future; since all these arguments are founded on the supposition of that resemblance. Let the course of things be allowed hitherto ever so regular; that alone, without some new argument or inference, proves not that, for the future, it will continue so. In vain do you pretend to have learned the nature of bodies from your past experience. Their secret nature, and consequently all their effects and influence, may change, without any change in their sensible qualities. This happens sometimes, and with regard to some objects: Why may it not happen always, and with regard to all objects? What logic, what process of argument secures you against this supposition? My practice, you say, refutes my doubts. But you mistake the purport of my question. As an agent, I am quite satisfied in the point; but as a philosopher, who has some share of curiosity, I will not say skepticism, I want to learn the foundation of this inference. No reading, no inquiry has yet been able to remove my difficulty, or give me satisfaction in a matter of such importance. Can I do better than propose the difficulty to be public, even though, perhaps, I have small hopes of obtaining a solution? We shall at least, by this means, be sensible of our ignorance, if we do not augment our knowledge.

I must confess that a man is guilty of unpardonable arrogance who concludes, because an argument has escaped his own investigation, that therefore it does not really exist. I must also confess that, though all the learned, for several ages, should have employed themselves in fruitless search upon any subject, it may still, perhaps, be rash to conclude positively that the subject must, therefore, pass all human comprehension. Even though we examine all the sources of our knowledge, and conclude them unfit for such a subject, there may still remain a suspicion, that the enumeration is not complete, or the examination not accurate. But with regard to the present subject, there are some considerations which seem to remove all this accusation of arrogance or suspicion of mistake.

It is certain that the most ignorant and stupid peasants—nay infants, nay even brute beasts—improve by experience, and learn the qualities of natural objects, by observing the effects which result from them. When a child has felt the sensation of pain from touching the flame of a candle, he will be careful not to put his hand near any candle; but will expect a similar effect from a cause which is similar in its sensible qualities and appearance. If you assert, therefore, that the understanding of the child is led into this conclusion by any process of argument or ratiocination, I may justly require you to produce that argument; nor have you any pretense to refuse so equitable a demand. You cannot say that the argument is abstruse, and may possibly escape your inquiry; since you confess that it is obvious to the capacity of a mere infant. If you hesitate, therefore, a moment, or if, after reflection, you produce any intricate or profound argument, you, in a manner, give up the question, and confess that it is not reasoning which engages us to suppose the past resembling the future, and to expect similar effects from causes which are, to appearance, similar. This is the proposition which I intended to enforce in the present section. If I be right, I pretend not to have made any mighty discovery. And if I be wrong, I must acknowledge myself to be indeed a very backward scholar; since I cannot now discover an argument which, it seems, was perfectly familiar to me long before I was out of my cradle.

NOTE

1. The word, Power, is here used in a loose and popular sense. The more accurate explication of it would give additional evidence to this argument.

8

CARL HEMPEL

The Role of Induction in Scientific Inquiry

As a simple illustration of some important aspects of scientific inquiry let us consider Semmelweis' work on childbed fever. Ignaz Semmelweis, a physician of Hungarian birth, did this work during the years from 1844 to 1848 at the Vienna General Hospital. As a member of the medical staff of the First Maternity Division in the hospital, Semmelweis was distressed to find that a large proportion of the women who were delivered of their babies in that division contracted a serious and often fatal illness known as puerperal fever or childbed fever. In 1844, as many as 260 out of 3,157 mothers in the First Division, or 8.2 percent, died of the disease; for 1845, the death rate was 6.8 percent, and for 1846, it was 11.4 percent. These figures were all the more alarming because in the adjacent Second Maternity Division of the same hospital, which accommodated almost as many women as the First, the death toll from childbed fever was much lower: 2.3, 2.0, and 2.7 percent for the same years. In a book that he wrote later on the causation and the prevention of childbed fever, Semmelweis describes his efforts to resolve the dreadful puzzle.[1]

He began by considering various explanations that were current at the time; some of these he rejected out of hand as incompatible with well-established facts; others he subjected to specific tests.

One widely accepted view attributed the ravages of puerperal fever to "epidemic influences," which were vaguely described as "atmospheric-cosmic-telluric changes" spreading over whole districts and causing childbed fever in women in confinement. But how, Semmelweis reasons, could such influences have

Philosophy of Natural Science (Upper Saddle River, NJ: Prentice-Hall, 1996), pp. 11–18.

plagued the First Division for years and yet spared the Second? And how could this view be reconciled with the fact that while the fever was raging in the hospital, hardly a case occurred in the city of Vienna or in its surroundings: a genuine epidemic, such as cholera, would not be so selective. Finally, Semmelweis notes that some of the women admitted to the First Division, living far from the hospital, had been overcome by labor on their way and had given birth in the street: yet despite these adverse conditions, the death rate from childbed fever among these cases of "street birth" was lower than the average for the First Division.

On another view, overcrowding was a cause of mortality in the First Division. But Semmelweis points out that in fact the crowding was heavier in the Second Division, partly as a result of the desperate efforts of patients to avoid assignment to the notorious First Division. He also rejects two similar conjectures that were current, by noting that there were no differences between the two Divisions in regard to diet or general care of the patients.

In 1846, a commission that had been appointed to investigate the matter attributed the prevalence of illness in the First Division to injuries resulting from rough examination by the medical students, all of whom received their obstetrical training in the First Division. Semmelweis notes in refutation of this view that (*a*) the injuries resulting naturally from the process of birth are much more extensive than those that might be caused by rough examination; (*b*) the midwives who received their training in the Second Division examined their patients in much the same manner but without the same ill effects; (*c*) when, in response to the commission's report, the number of medical students was halved and their examinations of the women were reduced to a minimum, the mortality, after a brief decline, rose to higher levels than ever before.

Various psychological explanations were attempted. One of them noted that the First Division was so arranged that a priest bearing the last sacrament to a dying woman had to pass through five wards before reaching the sickroom beyond: the appearance of the priest, preceded by an attendant ringing a bell, was held to have a terrifying and debilitating effect upon the patients in the wards and thus to make them more likely victims of childbed fever. In the Second Division, this adverse factor was absent, since the priest had direct access to the sickroom. Semmelweis decided to test this conjecture. He persuaded the priest to come by a roundabout route and without ringing of the bell, in order to reach the sick chamber silently and unobserved. But the mortality in the First Division did not decrease.

A new idea was suggested to Semmelweis by the observation that in the First Division the women were delivered lying on their backs; in the Second Division, on their sides. Though he thought it unlikely, he decided "like a drowning man clutching at a straw", to test whether this difference in procedure was significant. He introduced the use of the lateral position in the First Division, but again, the mortality remained unaffected.

At last, early in 1847, an accident gave Semmelweis the decisive clue for his solution of the problem. A colleague of his, Kolletschka, received a puncture wound in the finger, from the scalpel of a student with whom he was performing an autopsy, and died after an agonizing illness during which he displayed the same symptoms that Semmelweis had observed in the victims of childbed fever. Although the role of microorganisms in such infections had not yet been recognized at the time, Semmelweis realized that "cadaveric matter" which the student's scalpel had introduced into Kolletschka's blood stream had caused his colleague's fatal illness. And the similarities between the course of Kolletschka's disease and that of the women in his clinic led Semmelweis to the conclusion that his patients had died of the same kind of blood poisoning: he, his colleagues, and the medical students had been the carriers of the infectious material, for he and his associates used to come to the wards directly from performing dissections in the autopsy room, and examine the women in labor after only superficially washing their hands, which often retained a characteristic foul odor.

Again, Semmelweis put his idea to a test. He reasoned that if he were right, then childbed fever could be prevented by chemically destroying the infectious material adhering to the hands. He therefore issued an order requiring all medical students to wash their hands in a solution of chlorinated lime before making an examination. The mortality from childbed fever promptly began to decrease, and for the year

1848 it fell to 1.27 percent in the First Division, compared to 1.33 in the Second.

In further support of his idea, or of his *hypothesis*, as we will also say, Semmelweis notes that it accounts for the fact that the mortality in the Second Division consistently was so much lower: the patients there were attended by midwives, whose training did not include anatomical instruction by dissection of cadavers.

The hypothesis also explained the lower mortality among "street births": women who arrived with babies in arms were rarely examined after admission and thus had a better chance of escaping infection.

Similarly, the hypothesis accounted for the fact that the victims of childbed fever among the newborn babies were all among those whose mothers had contracted the disease during labor; for then the infection could be transmitted to the baby before birth, through the common bloodstream of mother and child, whereas this was impossible when the mother remained healthy.

Further clinical experiences soon led Semmelweis to broaden his hypothesis. On one occasion, for example, he and his associates, having carefully disinfected their hands, examined first a woman in labor who was suffering from a festering cervical cancer; then they proceeded to examine twelve other women in the same room, after only routine washing without renewed disinfection. Eleven of the twelve patients died of puerperal fever. Semmelweis concluded that childbed fever can be caused not only by cadaveric material, but also by "putrid matter derived from living organisms."

We have seen how, in his search for the cause of childbed fever, Semmelweis examined various hypotheses that had been suggested as possible answers. How such hypotheses are arrived at in the first place is an intriguing question which we will consider later. First, however, let us examine how a hypothesis, once proposed, is tested.

Sometimes, the procedure is quite direct. Consider the conjectures that differences in crowding, or in diet, or in general care account for the difference in mortality between the two divisions. As Semmelweis points out, these conflict with readily observable facts. There are no such differences between the divisions; the hypotheses are therefore rejected as false.

But usually the test will be less simple and straightforward. Take the hypothesis attributing the high mortality in the First Division to the dread evoked by the appearance of the priest with his attendant. The intensity of that dread, and especially its effect upon childbed fever, are not as directly ascertainable as are differences in crowding or in diet, and Semmelweis uses an indirect method of testing. He asks himself: Are there any readily observable effects that should occur if the hypothesis were true? And he reasons: *If* the hypothesis were true, *then* an appropriate change in the priest's procedure should be followed by a decline in fatalities. He checks this implication by a simple experiment and finds it false, and he therefore rejects the hypothesis.

Similarly, to test his conjecture about the position of the women during delivery, he reasons: *If* this conjecture should be true, *then* adoption of the lateral position in the First Division will reduce the mortality. Again, the implication is shown false by his experiment, and the conjecture is discarded.

In the last two cases, the test is based on an argument to the effect that *if* the contemplated hypothesis, say H, is true, *then* certain observable events (e.g., decline in mortality) should occur under specified circumstances (e.g., if the priest refrains from walking through the wards, or if the women are delivered in lateral position); or briefly, if H is true, then so is I, where I is a statement describing the observable occurrences to be expected. For convenience, let us say that I is inferred from, or implied by, H; and let us call I a *test implication of the hypothesis H*. (We will later give a more accurate description of the relation between I and H.)

In our last two examples, experiments show the test implication to be false, and the hypothesis is accordingly rejected. The reasoning that leads to the rejection may be schematized as follows:

If H is true, then so is I.

a] But (as the evidence shows) I is not true.

H is not true.

Any argument of this form, called *modus tollens* in logic,[2] is deductively valid; that is, if its premises (the sentences above the horizontal line) are true, then its conclusion (the sentence below the horizontal line) is unfailingly true as well. Hence, if the premises of (*a*) are properly established, the hypothesis H that is being tested must indeed be rejected.

Next, let us consider the case where observation or experiment bears out the test implication I. From his hypothesis that childbed fever is blood poisoning produced by cadaveric matter, Semmelweis infers that suitable antiseptic measures will reduce fatalities from the disease. This time, experiment shows the test implication to be true. But this favorable outcome does not conclusively prove the hypothesis true, for the underlying argument would have the form

If H is true, then so is I.
b] (As the evidence shows) I is true.

H is true.

And this mode of reasoning, which is referred to as the *fallacy of affirming the consequent,* is deductively invalid, that is, its conclusion may be false even if its premises are true.[3] This is in fact illustrated by Semmelweis' own experience. The initial version of his account of childbed fever as a form of blood poisoning presented infection with cadaveric matter essentially as the one and only source of the disease; and he was right in reasoning that if this hypothesis should be true, then destruction of cadaveric particles by antiseptic washing should reduce the mortality. Furthermore, his experiment did show the test implication to be true. Hence, in this case, the premises of (*b*) were both true. Yet, his hypothesis was false, for as he later discovered, putrid material from living organisms, too, could produce childbed fever.

Thus, the favorable outcome of a test, i.e., the fact that a test implication inferred from a hypothesis is found to be true, does not prove the hypothesis to be true. Even if many implications of a hypothesis have been borne out by careful tests, the hypothesis may still be false. The following argument still commits the fallacy of affirming the consequent:

If H is true, then so are $I_1, I_2, \ldots, I_n$.
c] (As the evidence shows) $I_1, I_2, \ldots, I_n$, are all true.

H is true.

This, too, can be illustrated by reference to Semmelweis' final hypothesis in its first version. As we noted earlier, his hypothesis also yields the test implications that among cases of street births admitted to the First Division, mortality from puerperal fever should be below the average for the Division, and that infants of mothers who escape the illness do not contract childbed fever; and these implications, too, were borne out by the evidence—even though the first version of the final hypothesis was false.

But the observation that a favorable outcome of however many tests does not afford conclusive proof for a hypothesis should not lead us to think that if we have subjected a hypothesis to a number of tests and all of them have had a favorable outcome, we are no better off than if we had not tested the hypothesis at all. For each of our tests might conceivably have had an unfavorable outcome and might have led to the rejection of the hypothesis. A set of favorable results obtained by testing different test implications, $I_1, I_2, \ldots, I_n$, of a hypothesis, shows that as far as these particular implications are concerned, the hypothesis has been borne out; and while this result does not afford a complete proof of the hypothesis, it provides at least some support, some partial corroboration or confirmation for it. The extent of this support will depend on various aspects of the hypothesis and of the test data.

The idea that in scientific inquiry, inductive inference from antecedently collected data leads to appropriate general principles is clearly embodied in the following account of how a scientist would ideally proceed:

> If we try to imagine how a mind of superhuman power and reach, but normal so far as the logical processes of its thought are concerned, . . . would use the scientific method, the process would be as follows: First, all facts would be observed and recorded, *without selection* or *a priori* guess as to their relative importance. Secondly, the observed and recorded facts would be analyzed, compared, and classified, *without hypothesis or postulates* other than those necessarily involved in the logic of thought. Third, from this analysis of the facts generalizations would be inductively drawn as to the relations, classificatory or causal, between them. Fourth, further research would be deductive as well as inductive, employing inferences from previously established generalizations.[4]

This passage distinguishes four stages in an ideal scientific inquiry: (1) observation and recording of all facts, (2) analysis and classification of these facts, (3) inductive derivation of generalizations from them, and (4) further testing of the generalizations. The first two of these stages are specifically assumed not to make use of any guesses or hypotheses as to how the observed facts might be interconnected; this restriction seems to have been imposed in the belief that such preconceived ideas would introduce a bias and would jeopardize the scientific objectivity of the investigation.

But the view expressed in the quoted passage—I will call it *the narrow inductivist conception of scientific inquiry*—is untenable, for several reasons. A brief survey of these can serve to amplify and to supplement our earlier remarks on scientific procedure.

First, a scientific investigation as here envisaged could never get off the ground. Even its first phase could never be carried out, for a collection of *all* the facts would have to await the end of the world, so to speak; and even all the facts *up to now* cannot be collected, since there are an infinite number and variety of them. Are we to examine, for example, all the grains of sand in all the deserts and on all the beaches, and are we to record their shapes, their weights, their chemical composition, their distances from each other, their constantly changing temperature, and their equally changing distance from the center of the moon? Are we to record the floating thoughts that cross our minds in the tedious process? The shapes of the clouds overhead, the changing color of the sky? The construction and the trade name of our writing equipment? Our own life histories and those of our fellow investigators? All these, and untold other things, are, after all, among "all the facts up to now".

Perhaps, then, all that should be required in the first phase is that all the *relevant* facts be collected. But relevant to what? Though the author does not mention this, let us suppose that the inquiry is concerned with a specified *problem*. Should we not then begin by collecting all the facts—or better, all available data—relevant to that problem? This notion still makes no clear sense. Semmelweis sought to solve one specific problem, yet he collected quite different kinds of data at different stages of his inquiry. And rightly so; for what particular sorts of data it is reasonable to collect is not determined by the problem under study, but by a tentative answer to it that the investigator entertains in the form of a conjecture or hypothesis. Given the conjecture that mortality from childbed fever was increased by the terrifying appearance of the priest and his attendant with the death bell, it was relevant to collect data on the consequences of having the priest change his routine; but it would have been totally irrelevant to check what would happen if doctors and students disinfected their hands before examining their patients. With respect to Semmelweis' eventual contamination hypothesis, data of the latter kind were clearly relevant, and those of the former kind totally irrelevant.

Empirical "facts" or findings, therefore, can be qualified as logically relevant or irrelevant only in reference to a given hypothesis, but not in reference to a given problem.

Suppose now that a hypothesis H has been advanced as a tentative answer to a research problem: what kinds of data would be relevant to H? Our earlier examples suggest an answer: A finding is relevant to H if either its occurrence or its nonoccurrence can be inferred from H. Take Torricelli's hypothesis, for example. As we saw, Pascal inferred from it that the mercury column in a barometer should grow shorter if the barometer were carried up a mountain. Therefore, any finding to the effect that this did indeed happen in a particular case is relevant to the hypothesis; but so would be the finding that the length of the mercury column had remained unchanged or that it had decreased and then increased during the ascent, for such findings would refute Pascal's test implication and would thus disconfirm Torricelli's hypothesis. Data of the former kind may be called positively, or favorably, relevant to the hypothesis; those of the latter kind negatively, or unfavorably, relevant.

In sum, the maxim that data should be gathered without guidance by antecedent hypotheses about the connections among the facts under study is self-defeating, and it is certainly not followed in scientific inquiry. On the contrary, tentative hypotheses are needed to give direction to a scientific investigation. Such hypotheses determine, among other things, what data should be collected at a given point in a scientific investigation.

It is of interest to note that social scientists trying to check a hypothesis by reference to the vast store of facts recorded by the U.S. Bureau of the Census, or by other data-gathering organizations, sometimes find to their disappointment that the values of some variable that plays a central role in the hypothesis have nowhere been systematically recorded. This remark is not, of course, intended as a criticism of data gathering: those engaged in the process no doubt try to select facts that might prove relevant to future hypotheses; the observation is simply meant to illustrate the impossibility of collecting "all the relevant data" without knowledge of the hypotheses to which the data are to have relevance.

The second stage envisaged in our quoted passage is open to similar criticism. A set of empirical "facts" can be analyzed and classified in many different ways, most of which will be unilluminating for the purposes of a given inquiry. Semmelweis could have classified the women in the maternity wards according to criteria such as age, place of residence, marital status, dietary habits, and so forth; but information on these would have provided no clue to a patient's prospects of becoming a victim of childbed fever. What Semmelweis sought were criteria that would be significantly connected with those prospects; and for this purpose, as he eventually found, it was illuminating to single out those women who were attended by medical personnel with contaminated hands; for it was with this characteristic, or with the corresponding class of patients, that high mortality from childbed fever was associated.

Thus, if a particular way of analyzing and classifying empirical findings is to lead to an explanation of the phenomena concerned, then it must be based on hypotheses about how those phenomena are connected; without such hypotheses, analysis and classification are blind.

Our critical reflections on the first two stages of inquiry as envisaged in the quoted passage also undercut the notion that hypotheses are introduced only in the third stage, by inductive inference from antecedently collected data. But some further remarks on the subject should be added here.

Induction is sometimes conceived as a method that leads, by means of mechanically applicable rules, from observed facts to corresponding general principles. In this case, the rules of inductive inference would provide effective canons of scientific discovery; induction would be a mechanical procedure analogous to the familiar routine for the multiplication of integers, which leads, in a finite number of predetermined and mechanically performable steps, to the corresponding product. Actually, however, no such general and mechanical induction procedure is available at present; otherwise, the much studied problem of the causation of cancer, for example, would hardly have remained unsolved to this day. Nor can the discovery of such a procedure ever be expected. For—to mention one reason—scientific hypotheses and theories are usually couched in terms that do not occur at all in the description of the empirical findings on which they rest, and which they serve to explain. For example, theories about the atomic and subatomic structure of matter contain terms such as 'atom', 'electron', 'proton', 'neutron', 'psi-function', etc.; yet they are based on laboratory findings about the spectra of various gases, tracks in cloud and bubble chambers, quantitative aspects of chemical reactions, and so forth—all of which can be described without the use of those "theoretical terms". Induction rules of the kind here envisaged would therefore have to provide a mechanical routine for constructing, on the basis of the given data, a hypothesis or theory stated in terms of some quite novel concepts, which are nowhere used in the description of the data themselves. Surely, no general mechanical rule of procedure can be expected to achieve this. Could there be a general rule, for example, which, when applied to the data available to Galileo concerning the limited effectiveness of suction pumps, would, by a mechanical routine, produce a hypothesis based on the concept of a sea of air?

To be sure, mechanical procedures for inductively "inferring" a hypothesis on the basis of given data may be specifiable for situations of special, and relatively simple, kinds. For example, if the length of a copper rod has been measured at several different temperatures, the resulting pairs of associated values for temperature and length may be represented by points in a plane coordinate system, and a curve may be drawn through them in accordance with some particular rule of curve fitting. The curve then graphically represents a general quantitative hypothesis that expresses the length of the rod as a specific function of its temperature. But note that this hypothesis contains no novel terms; it is expressible in terms of the concepts of temperature and length, which are used also in describing the data. Moreover, the choice of "associated" values of temperature and length as data already presupposes a guiding hypothesis; namely, that with each value of the temperature, exactly one value of the length of the copper rod is associated, so that its length is indeed a function of its temperature alone. The mechanical curve-fitting routine then serves only to select a particular function as the appropriate one. This point is important; for suppose that instead of a copper rod, we examine a body of nitrogen gas enclosed in a cylindrical container with a movable piston as a lid, and that we measure its volume at several different temperatures. If we were to use this procedure in an effort to obtain from our data a *general* hypothesis representing the volume of the gas as a function of its temperature, we would fail, because the volume of a gas is a function both of its temperature and of the pressure exerted upon it, so that at the same temperature, the given gas may assume different volumes.

Thus, even in these simple cases, the mechanical procedures for the construction of a hypothesis do only part of the job, for they presuppose an antecedent, less specific hypothesis (i.e., that a certain physical variable is a function of one single other variable), which is not obtainable by the same procedure.

There are, then, no generally applicable "rules of induction", by which hypotheses or theories can be mechanically derived or inferred from empirical data. The transition from data to theory requires creative imagination. Scientific hypotheses and theories are not *derived* from observed facts, but *invented* in order to account for them. They constitute guesses at the connections that might obtain between the phenomena under study, at uniformities and patterns that might underlie their occurrence. "Happy guesses"[5] of this kind require great ingenuity, especially if they involve a radical departure from current modes of scientific thinking, as did, for example, the theory of relativity and quantum theory. The inventive effort required in scientific research will benefit from a thorough familiarity with current knowledge in the field. A complete novice will hardly make an important scientific discovery, for the ideas that may occur to him are likely to duplicate what has been tried before or to run afoul of well-established facts or theories of which he is not aware.

Nevertheless, the ways in which fruitful scientific guesses are arrived at are very different from any process of systematic inference. The chemist Kekulé, for example, tells us that he had long been trying unsuccessfully to devise a structural formula for the benzene molecule when, one evening in 1865, he found a solution to his problem while he was dozing in front of his fireplace. Gazing into the flames, he seemed to see atoms dancing in snakelike arrays. Suddenly, one of the snakes formed a ring by seizing hold of its own tail and then whirled mockingly before him. Kekulé awoke in a flash: he had hit upon the now famous and familiar idea of representing the molecular structure of benzene by a hexagonal ring. He spent the rest of the night working out the consequences of this hypothesis.[6]

This last remark contains an important reminder concerning the objectivity of science. In his endeavor to find a solution to his problem, the scientist may give free rein to his imagination, and the course of his creative thinking may be influenced even by scientifically questionable notions. Kepler's study of planetary motion, for example, was inspired by his interest in a mystical doctrine about numbers and a passion to demonstrate the music of the spheres. Yet, scientific objectivity is safeguarded by the principle that while hypotheses and theories may be freely invented and *proposed* in science, they can be *accepted* into the body of scientific knowledge only if they pass critical scrutiny, which includes in particular the checking of suitable test implications by careful observation or experiment.

Interestingly, imagination and free invention play a similarly important role in those disciplines whose results are validated exclusively by deductive reasoning; for example, in mathematics. For the rules of deductive inference do not afford mechanical rules of discovery, either. As illustrated by our statement of *modus tollens* above, those rules are usually expressed in the form of general schemata, any instance of which is a deductively valid argument. If premises of the specified kind are given, such a schema does indeed specify a way of proceeding to a logical consequence. But for any set of premises that may be given, the rules of deductive inference specify an infinity of validly deducible conclusions.

Take, for example, one simple rule represented by the following schema:

$$\frac{p}{p \text{ or } q}$$

It tells us, in effect, that from the proposition that p is the case, it follows that p or q is the case, where p and q may be any propositions whatever. The word 'or' is here understood in the "nonexclusive" sense, so that 'p or q' is tantamount to 'either p or q or both p and q'. Clearly, if the premise of an argument of this type is true, then so must be the conclusion; hence, any argument of the specified form is valid. But this one rule alone entitles us to infer infinitely many different consequences from any one premise. Thus, from 'the Moon has no atmosphere', it authorizes us to infer any statement of the form 'The Moon has no atmosphere, or q', where for 'q' we may write any statement whatsoever, no matter whether it is true or false; for example, 'the Moon's atmosphere is very thin', 'the Moon is uninhabited', 'gold is denser than silver', 'silver is denser than gold', and so forth. (It is interesting and not difficult to prove that infinitely many different statements can be formed in English; each of these may be put in the place of the variable 'q'.) Other rules of deductive inference add, of course, to the variety of statements derivable from one premise or set of premises. Hence, if we are given a set of statements as premises, the rules of deduction give no direction to our inferential procedures. They do not single out one statement as "the" conclusion to be derived from our premises, nor do they tell us how to obtain interesting or systematically important conclusions; they provide no mechanical routine, for example, for deriving significant mathematical theorems from given postulates. The discovery of important, fruitful mathematical theorems, like the discovery of important, fruitful theories in empirical science, requires inventive ingenuity; it calls for imaginative, insightful guessing. But again, the interests of scientific objectivity are safeguarded by the demand for an *objective validation* of such conjectures. In mathematics, this means *proof* by deductive derivation from axioms. And when a mathematical proposition has been proposed as a conjecture, its proof or disproof still requires inventiveness and ingenuity, often of a very high caliber; for the rules of deductive inference do not even provide a general mechanical procedure for constructing proofs or disproofs. Their systematic role is rather the modest one of serving as *criteria of soundness for arguments* offered as proofs: an argument will constitute a valid mathematical proof if it proceeds from the axioms to the proposed theorem by a chain of inferential steps each of which is valid according to one of the rules of deductive inference. And to check whether a given argument is a valid proof in this sense is indeed a purely mechanical task.

Scientific knowledge, as we have seen, is not arrived at by applying some inductive inference procedure to antecedently collected data, but rather by what is often called "the method of hypothesis", i.e., by inventing hypotheses as tentative answers to a problem under study, and then subjecting these to empirical test. It will be part of such test to see whether the hypothesis is borne out by whatever relevant findings may have been gathered before its formulation; an acceptable hypothesis will have to fit the available relevant data. Another part of the test will consist in deriving new test implications from the hypothesis and checking these by suitable observations or experiments. As we noted earlier, even extensive testing with entirely favorable results does not establish a hypothesis conclusively, but provides only more or less strong support for it. Hence, while scientific inquiry is certainly not inductive in the narrow sense we have examined in some detail, it may be said to be *inductive in a wider sense,* inasmuch as it involves the acceptance of hypotheses on the basis of data that afford no deductively conclusive evidence for it, but lend it only more or less strong "inductive support", or confirmation. And any "rules of induction" will have to be conceived, in analogy to the rules of deduction, as canons of validation rather than of discovery. Far from generating a hypothesis that accounts for given empirical findings, such rules will presuppose that both the empirical data forming the "premises" of the "inductive argument" and a tentative hypothesis forming its "conclusion" are *given.* The rules of induction would then state criteria for the soundness of the argument. According to some theories of induction, the rules would determine the strength of the support that the data lend to the hypothesis, and they might express such support in terms of probabilities.

NOTES

1. The story of Semmelweis' work and of the difficulties he encountered forms a fascinating page in the history of medicine. A detailed account, which includes translations and paraphrases of large portions of Semmelweis' writings, is given in W. J. Sinclair, *Semmelweis: His Life and His Doctrine* (Manchester, England: Manchester University Press, 1909). Brief quoted phrases in this chapter are taken from this work. The highlights of Semmelweis' career are recounted in the first chapter of P. de Kruif, *Men Against Death* (New York: Harcourt, Brace & World, Inc., 1932).
2. For details, see another volume in this series: W. Salmon, *Logic*, pp., 24–25.
3. See Salmon, *Logic*, pp. 27–29.
4. A. B. Wolfe, "Functional Economics," in *The Trend of Economics*, ed. R. G. Tugwell (New York: Alfred A. Knopf, Inc., 1924), p. 450 (italics are quoted).
5. This characterization was given already by William Whewell in his work *The Philosophy of the Inductive Sciences*, 2nd ed. (London: John W. Parker, 1847); II, 41. Whewell also speaks of "invention" as "part of induction" (p. 46). In the same vein, K. Popper refers to scientific hypotheses and theories as "conjectures"; see, for example, the essay "Science: Conjectures and Refutations" in his book, *Conjectures and Refutations* (New York and London: Basic Books, 1962). Indeed, A. B. Wolfe, whose narrowly inductivist conception of ideal scientific procedure was quoted earlier, stresses that "the limited human mind" has to use "a greatly modified procedure", requiring scientific imagination and the selection of data on the basis of some "working hypothesis" (p. 450 of the essay cited in note 4).
6. Cf. the quotations from Kekulé's own report in A. Findlay, *A Hundred Years of Chemistry*, 2nd ed. (London: Gerald Duckworth & Co., 1948), p. 37; and W. I. B., Beveridge, *The Art of Scientific Investigation*, 3rd ed. (London: William Heinemann, Ltd., 1957), p. 56.

9

KARL POPPER

The Problem of Induction

A scientist, whether theorist or experimenter, puts forward statements, or systems of statements, and tests them step by step. In the field of the empirical sciences, more particularly, he constructs hypotheses, or systems of theories, and tests them against experience by observation and experiment.

I suggest that it is the task of the logic of scientific discovery, or the logic of knowledge, to give a logical analysis of this procedure; that is, to analyze the method of the empirical sciences.

But what are these 'methods of the empirical sciences'? And what do we call 'empirical science'?

THE PROBLEM OF INDUCTION

According to a widely accepted view—to be opposed in this book—the empirical sciences can be characterized by the fact that they use *'inductive methods'*, as they are called. According to this view, the logic of scientific discovery would be identical

The Logic of Discovery (London: Hutchinson, 1959), pp. 27–33. Reprinted with permission.