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The Problem of Induction

1 | The Problem of Induction

According to a widely accepted view . . . 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 with inductive logic, *i.e.* with the logical analysis of these inductive methods.

It is usual to call an inference 'inductive' if it passes from *singular statements* (sometimes also called 'particular' statements), such as accounts of the results of observations or experiments, to *universal statements*, such as hypotheses or theories.

Now it is far from obvious, from a logical point of view, that we are justified in inferring universal statements from singular ones, no matter how numerous; for any conclusion drawn in this way may always turn out to be false: no matter how many instances of white swans we may have observed, this does not justify the conclusion that *all* swans are white.

The question whether inductive inferences are justified, or under what conditions, is known as *the problem of induction*.

The problem of induction may also be formulated as the question of how to establish the truth of universal statements which are based on experience, such as the hypotheses and theoretical systems of the empirical sciences. For many people believe that the truth of these universal statements is '*known by experience*'; yet it is clear that an account of an experience—of an observation or the result of an experiment—can in the first place be only a singular statement and not a universal one. Accordingly, people who say of a universal statement that we know its truth from experience usually mean that the truth of this universal statement can

FROM Karl Popper, *The Logic of Scientific Discovery* (New York: Basic Books, 1959), 27–34.

somehow be reduced to the truth of singular ones, and that these singular ones are known by experience to be true; which amounts to saying that the universal statement is based on inductive inference. Thus to ask whether there are natural laws known to be true appears to be only another way of asking whether inductive inferences are logically justified.

Yet if we want to find a way of justifying inductive inferences, we must first of all try to establish a *principle of induction*. A principle of induction would be a statement with the help of which we could put inductive inferences into a logically acceptable form. In the eyes of the upholders of inductive logic, a principle of induction is of supreme importance for scientific method: '. . . this principle', says Reichenbach,* 'determines the truth of scientific theories. To eliminate it from science would mean nothing less than to deprive science of the power to decide the truth or falsity of its theories. Without it, clearly, science would no longer have the right to distinguish its theories from the fanciful and arbitrary creations of the poet's mind.'¹

Now this principle of induction cannot be a purely logical truth like a tautology or an analytic statement. Indeed, if there were such a thing as a purely logical principle of induction, there would be no problem of induction; for in this case, all inductive inferences would have to be regarded as purely logical or tautological transformations, just like inferences in deductive logic. Thus the principle of induction must be a synthetic statement; that is, a statement whose negation is not self-contradictory but logically possible. So the question arises why such a principle should be accepted at all, and how we can justify its acceptance on rational grounds.

Some who believe in inductive logic are anxious to point out, with Reichenbach, that 'the principle of induction is unreservedly accepted by the whole of science and that no man can seriously doubt this principle in everyday life either'.² Yet even supposing this were the case—for after all, 'the whole of science' might err—I should still contend that a principle of induction is superfluous, and that it must lead to logical inconsistencies.

That inconsistencies may easily arise in connection with the principle of induction should have been clear from the work of Hume; also, that they can be avoided, if at all, only with difficulty. For the principle of induction must be a universal statement in its turn. Thus if we try to regard its truth as known from experience, then the very same problems which occasioned its introduction will arise all over again. To justify it, we should

* Hans Reichenbach (1891–1953) was a leading figure (along with Carl Hempel) in the Berlin school of philosophy of science in the 1920s and early 1930s. Although closely associated with the Vienna Circle, Reichenbach rejected the verifiability principle of meaning and adopted the phrase *logical empiricism* to distinguish his views from those of the logical positivists. His works on space and time, quantum mechanics, probability, and induction have been an important influence on twentieth-century philosophy of science.

have to employ inductive inferences; and to justify these we should have to assume an inductive principle of a higher order; and so on. Thus the attempt to base the principle of induction on experience breaks down, since it must lead to an infinite regress.

Kant tried to force his way out of this difficulty by taking the principle of induction (which he formulated as the 'principle of universal causation') to be '*a priori* valid'. But I do not think that his ingenious attempt to provide an *a priori* justification for synthetic statements was successful.

My own view is that the various difficulties of inductive logic here sketched are insurmountable. So also, I fear, are those inherent in the doctrine, so widely current today, that inductive inference, although not 'strictly valid', *can attain some degree of 'reliability' or of 'probability'*. According to this doctrine, inductive inferences are 'probable inferences'.³ 'We have described', says Reichenbach, 'the principle of induction as the means whereby science decides upon truth. To be more exact, we should say that it serves to decide upon probability. For it is not given to science to reach either truth or falsity . . . but scientific statements can only attain continuous degrees of probability whose unattainable upper and lower limits are truth and falsity'.⁴

At this stage I can disregard the fact that the believers in inductive logic entertain an idea of probability. . . . I can do so because the difficulties mentioned are not even touched by an appeal to probability. For if a certain degree of probability is to be assigned to statements based on inductive inference, then this will have to be justified by invoking a new principle of induction, appropriately modified. And this new principle in its turn will have to be justified, and so on. Nothing is gained, moreover, if the principle of induction, in its turn, is taken not as 'true' but only as 'probable'. In short, like every other form of inductive logic, the logic of probable inference, or 'probability logic', leads either to an infinite regress, or to the doctrine of *apriorism*.

The theory to be developed in the following pages stands directly opposed to all attempts to operate with the ideas of inductive logic. It might be described as the theory of *the deductive method of testing*, or as the view that a hypothesis can only be empirically *tested*—and only *after* it has been advanced.

Before I can elaborate this view (which might be called 'deductivism', in contrast to 'inductivism'⁵) I must first make clear the distinction between the *psychology of knowledge* which deals with empirical facts, and the *logic of knowledge* which is concerned only with logical relations. For the belief in inductive logic is largely due to a confusion of psychological problems with epistemological ones. It may be worth noticing, by the way, that this confusion spells trouble not only for the logic of knowledge but for its psychology as well.

2 | Elimination of Psychologism

I said above that the work of the scientist consists in putting forward and testing theories.

The initial stage, the act of conceiving or inventing a theory, seems to me neither to call for logical analysis nor to be susceptible of it. The question how it happens that a new idea occurs to a man—whether it is a musical theme, a dramatic conflict, or a scientific theory—may be of great interest to empirical psychology; but it is irrelevant to the logical analysis of scientific knowledge. This latter is concerned not with *questions of fact* (Kant's *quid facti?*), but only with questions of *justification or validity* (Kant's *quid juris?*). Its questions are of the following kind. Can a statement be justified? And if so, how? Is it testable? Is it logically dependent on certain other statements? Or does it perhaps contradict them? In order that a statement may be logically examined in this way, it must already have been presented to us. Someone must have formulated it, and submitted it to logical examination.

Accordingly I shall distinguish sharply between the process of conceiving a new idea, and the methods and results of examining it logically. As to the task of the logic of knowledge—in contradistinction to the psychology of knowledge—I shall proceed on the assumption that it consists solely in investigating the methods employed in those systematic tests to which every new idea must be subjected if it is to be seriously entertained.

Some might object that it would be more to the purpose to regard it as the business of epistemology to produce what has been called a '*rational reconstruction*' of the steps that have led the scientist to a discovery—to the finding of some new truth. But the question is: what, precisely, do we want to reconstruct? If it is the processes involved in the stimulation and release of an inspiration which are to be reconstructed, then I should refuse to take it as the task of the logic of knowledge. Such processes are the concern of empirical psychology but hardly of logic. It is another matter if we want to reconstruct rationally the *subsequent tests* whereby the inspiration may be discovered to be a discovery, or become known to be knowledge. In so far as the scientist critically judges, alters, or rejects his own inspiration we may, if we like, regard the methodological analysis undertaken here as a kind of '*rational reconstruction*' of the corresponding thought-processes. But this reconstruction would not describe these processes as they actually happen: it can give only a logical skeleton of the procedure of testing. Still, this is perhaps all that is meant by those who speak of a '*rational reconstruction*' of the ways in which we gain knowledge.

. . . My view of the matter, for what it is worth, is that there is no such thing as a logical method of having new ideas, or a logical reconstruction of this process. My view may be expressed by saying that every

discovery contains 'an irrational element', or 'a creative intuition', in Bergson's sense.* In a similar way Einstein speaks of the 'search for those highly universal laws . . . from which a picture of the world can be obtained by pure deduction. There is no logical path', he says, 'leading to these . . . laws. They can only be reached by intuition, based upon something like an intellectual love (*Einführung*) of the objects of experience'.⁶

3 | Deductive Testing of Theories

According to the view that will be put forward here, the method of critically testing theories, and selecting them according to the results of tests, always proceeds on the following lines. From a new idea, put up tentatively, and not yet justified in any way—an anticipation, a hypothesis, a theoretical system, or what you will—conclusions are drawn by means of logical deduction. These conclusions are then compared with one another and with other relevant statements, so as to find what logical relations (such as equivalence, derivability, compatibility, or incompatibility) exist between them.

We may if we like distinguish four different lines along which the testing of a theory could be carried out. First there is the logical comparison of the conclusions among themselves, by which the internal consistency of the system is tested. Secondly, there is the investigation of the logical form of the theory, with the object of determining whether it has the character of an empirical or scientific theory, or whether it is, for example, tautological. Thirdly, there is the comparison with other theories, chiefly with the aim of determining whether the theory would constitute a scientific advance should it survive our various tests. And finally, there is the testing of the theory by way of empirical applications of the conclusions which can be derived from it.

The purpose of this last kind of test is to find out how far the new consequences of the theory—whatever may be new in what it asserts—stand up to the demands of practice, whether raised by purely scientific experiments, or by practical technological applications. Here too the procedure of testing turns out to be deductive. With the help of other statements, previously accepted, certain singular statements—which we may call 'predictions'—are deduced from the theory; especially predictions that

* The French philosopher Henri Bergson (1859–1941) attacked materialism and mechanism, rejected science as a complete account of reality, and advocated vitalism, a worldview based on creative forces and intuition. His writings on memory and our subjective experience of time were quite influential, especially on authors such as Marcel Proust, but his postulation of the *élan vital*, a spiritual force that drives biological evolution, discredited Bergson's vitalism in the eyes of most scientists and philosophers.

are easily testable or applicable. From among these statements, those are selected which are not derivable from the current theory, and more especially those which the current theory contradicts. Next we seek a decision as regards these (and other) derived statements by comparing them with the results of practical applications and experiments. If this decision is positive, that is, if the singular conclusions turn out to be acceptable, or *verified*, then the theory has, for the time being, passed its test: we have found no reason to discard it. But if the decision is negative, or in other words, if the conclusions have been *falsified*, then their falsification also falsifies the theory from which they were logically deduced.

It should be noticed that a positive decision can only temporarily support the theory, for subsequent negative decisions may always overthrow it. So long as a theory withstands detailed and severe tests and is not superseded by another theory in the course of scientific progress, we may say that it has 'proved its mettle' or that it is '*corroborated*'.

Nothing resembling inductive logic appears in the procedure here outlined. I never assume that we can argue from the truth of singular statements to the truth of theories. I never assume that by force of 'verified' conclusions, theories can be established as 'true', or even as merely 'probable' . . .

■ | Notes

1. H. Reichenbach, *Erkenntnis* 1, 1930, p. 186 (cf. also p. 64 f.).
2. Reichenbach *ibid.*, p. 67.
3. Cf. J. M. Keynes, *A Treatise on Probability* (1921); O. Külpe, *Vorlesungen über Logik* (ed. by Selz, 1923); Reichenbach (who uses the term 'probability implications'), *Axiomatik der Wahrscheinlichkeitsrechnung*, *Mathem. Zeitschr.* 34 (1932); and in many other places.
4. Reichenbach, *Erkenntnis* 1, 1930, p. 186.
5. Liebig (in *Induktion und Deduktion*, 1865) was probably the first to reject the inductive method from the standpoint of natural science; his attack is directed against Bacon. Duhem (in *La Théorie physique, son objet et sa structure*, 1906; English translation by P. P. Wiener: *The Aim and Structure of Physical Theory*, Princeton, 1954) held pronounced deductivist views. (But there are also inductivist views to be found in Duhem's book, for example in the third chapter, Part One, where we are told that only experiment, induction, and generalization have produced Descartes's law of refraction; cf. the English translation, p. 34.) See also V. Kraft, *Die Grundformen der Wissenschaftlichen Methoden*, 1925; and Carnap, *Erkenntnis* 2, 1932, p. 440.
6. Address on Max Planck's 60th birthday. The passage quoted begins with the words, 'The supreme task of the physicist is to search for those highly universal

laws . . . ,’ etc. (quoted from A. Einstein, *Mein Weltbild*, 1934, p. 168; English translation by A. Harris: *The World As I See It*, 1935, p. 125). Similar ideas are found earlier in Liebig, *op. cit.*; cf. also Mach, *Prinzipien der Wärmelehre* (1896), p. 443 ff. The German word ‘*Einfühlung*’ is difficult to translate. Harris translates: ‘sympathetic understanding of experience’.

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Rational Prediction

A colleague, to whom I shall refer (quite accurately) as “the friendly physicist,” recently recounted the following incident. While awaiting takeoff on an airplane, he noticed a young boy sitting across the aisle holding onto a string to which was attached a helium-filled balloon. He endeavored to pique the child’s curiosity. “If you keep holding the string just as you are now,” he asked, “what do you think the balloon will do when the airplane accelerates before takeoff?” The question obviously had not crossed the youngster’s mind before that moment, but after giving it a little thought, he expressed the opinion that the balloon would move toward the back of the cabin. “I don’t think so,” said the friendly physicist, “I think it will move forward.” The child was now eager to see what would happen when the plane began to move. Several adults in the vicinity were, however, skeptical about the physicist’s prediction; in fact, a stewardess offered to wager a miniature bottle of Scotch that he was mistaken. The friendly physicist was not unwilling and the bet was made. In due course, the airplane began to accelerate, and the balloon moved toward the front of the cabin. The child’s curiosity was satisfied!; the theory—that all objects which are free to move will move toward the back of the cabin when the plane accelerates—was falsified; and the friendly physicist enjoyed a free drink.

I have related this anecdote to point out that there are at least three—probably more—legitimate reasons for making predictions. First, we are sometimes curious about future happenings, and we want to satisfy that curiosity without waiting for the events in question to transpire. To do so,

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