The Logical Character of the Principle of Induction[1]

BY

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THE purpose of this paper is to make clear (1) that the widely recognized formulations of the principle of induction do not express the most fundamental rule of induction; (2) that the current view concerning the probability of induction must be revised in terms of a frequency theory of probability; (3) that on this basis the problem of induction in its traditional form is a pseudo-problem; and (4) that the principle of induction must be interpreted as a *pragmatic* or *operational* maxim.

I

Let us begin with a brief summary of those views concerning the problem of induction which seem to have received the most general approval among contemporary logicians and philosophers.

(1) Induction is essentially different from deductive inference. It can never attain certainty. All attempts to transform inductive into deductive inference fail because they necessitate the introduction of *inductive* premises. Hume has shown that induc-

[1] This paper was read at a meeting of the western division of the American Philosophical Association at Ann Arbor, March, 1932. In what is presented as the constructive part, I am indebted to the criticisms and suggestions of Professor P. W. Bridgman and Professor C. I. Lewis of Harvard University, as well as to my friends of the Vienna circle, especially Professor R. Carnap and Professor M. Schlick.

tion can be proved certain neither on logical grounds nor on the basis of its own success.

(2) Induction is the indispensable foundation of all factual science, although it is admitted that the more advanced factual sciences do not actually proceed by inductive generalization. Their method consists rather in the construction of hypothetico-deductive systems. The strength of such systems lies in the high degree of internal connectness by which the various parts of a system reinforce one another. But logically, if not genetically, a theory is inductive. This is clear from the fact that any verification establishes the truth only of singular or particular propositions but not of general hypotheses.

(3) The principle of induction expresses the increase of the *probability* of inductions in dependence upon the accumulation of factual evidence. Such evidence consists in the elimination of irrelevant circumstances, as well as in the positive confirmation of a specific connection, uniformity, or regularity.

(4) This principle of induction is not a consequence of the purely logical axioms of the calculus of probabilities. It can be demonstrated only on the basis of *assumptions concerning the general constitution of nature.* Thus, Jevons took nature to be something like an urn to which we can apply Bayes' Theorem. Peirce, similarly, assumed that our observations represent "fair samples" of a thoroughly statistical world. Zilsel, Broad, Keynes and Nicod introduce more refined formulations of the "Principle of the Uniformity of Nature" such as the "Principle of Limited Depth and Variety," or at least the antecedent probability of such assumptions.[2]

(5) The quantitative value of probabilities and their convergence toward certainty can be derived only from the presupposition of rather arbitrary and artificial conditions whose fulfilment is by no means warranted in any case of scientifically significant induction. The idea of determining the numerical value of the probability of scientific theories seems preposterous.

[2] E. Zilsel: *Das Anwendungsproblem*, Leipzig, 1916. C. D. Broad: "The Principles of Problematic Induction," Proc. Aristot. Soc., 1927–8. J. M. Keynes: *A Treatise on Probability*, 1921. Nicod: *Foundations of Geometry and Induction*, 1930.

(6) Since these general hypotheses underlying induction are interpreted as significant assumptions concerning the structure of reality, they must be logically synthetic and, therefore, themselves *inductive*. This is the fundamental difficulty. *What can be meant by the probability of these presuppositions of the probability of all particular inductions?* It is held that even these presuppositions can be rendered increasingly probable by the verification of their consequences. Keynes and Nicod believe that they have proved this argument to be free from circularity. To many thinkers the whole issue appears highly problematic.

II

The unsatisfactory state of the problem of induction seems to me to be due to the preoccupation with the problem of the *validity* of induction. The more fundamental question concerning the *meaning* of the principle of induction is rather neglected and it is this which needs a careful, logical analysis.

The chief difficulty, undoubtedly, lies in the interpretation of the concept of probability. To Hume, the probability of induction was a subjective or psychological matter. It was a degree of belief or an intensity of expectation, based on habit. In contrast with this reduction of probability to something irrational and in opposition to the classical *subjective* interpretation of mathematical probability, two types of *objective* interpretations have been advanced:

(1) *Probability as a Logical Relation*—the theory of Leibniz, Bolzano and W. E. Johnson, most fully expounded in Keynes' *Treatise*, and accepted by C. D. Broad, Nicod and others.

(2) *Probability as the Limit of a Statistical Frequency*—the theory of Venn and Peirce, rejected by Keynes, but recently restated, defended and mathematically systematized by the Berlin mathematician, R. v. Mises.

According to the logical interpretation, which still seems generally favored, probability is the relation of partial or inconclusive implication between one proposition and another. But this account is for Keynes merely a characterization, not a definition, of the fundamentally unanalyzable and indefinable probability

relation. Here, I believe, Keynes is fundamentally in error. I do not mean to deny that the probability relation can be chosen as a primitive notion in an axiomatization of the probability calculus. That can of course be done, and has in fact been done by Keynes, and more recently by Reichenbach.[3] But probability is also applied to empirical facts and in this case we need rules in order to determine the value of the applied probability. These rules, if they can be stated at all, are then the *definition of the empirical or applied probability concept.* I have not time here to prove in detail that in any significant application of the probability concept its essential meaning is statistical. However, I shall discuss a few of the relevant points.

If the principle of indifference operates not on the basis of equal ignorance, as did its predecessor, the "principle of insufficient reason," but on positive grounds, it must inevitably make use of statistical assumptions. Often these assumptions are tacit and in many cases their statistical character is not recognized, but they are the true source of every fruitful probability argument. The "indifference" or "irrelevancy," which is the crucial concept in the principle, means generally *causal irrelevance.* Causal irrelevance, however, is identical with random distribution, and random distribution is a fact which can be established only by statistical investigation. The essential and final test for the correctness of any estimate of probabilities is always the comparison with the statistical frequencies. Confronted with an "a priori" probability, one can take only one reasonable attitude, and that is to ask the direct question: What bearing does it have on observable facts? It is understandable that Peirce, who introduced this pragmatic question as a general criterion of meaning, was at the same time perhaps the most convincing advocate of the frequency theory.

III

If probability is to be a significant guide for our expectations and predictions, inductive probability must be interpreted in terms of the frequency theory. Once it is granted that induction

[3] H. Reichenbach: Axiome d. Wahrsch. Math. Zs. 1932.

is not an infallible procedure, all our care must be directed toward attaining success at least with a maximal frequency. Mill's famous question—Why are the experimental methods (as stated in his own canons) much more efficient than induction by pure enumeration?—means precisely: Why are the experimental methods successful more frequently than simple induction in the discovery of a law? The fact that they are more successful has stimulated the desire for a more profound explanation in terms of assumptions about the structure of the world. The assumption that there are at the bottom of nature strictly deterministic laws, on the one hand, and complete independencies on the other, seemed to account for the superior reliability of experimental methods. For if we follow the simpler method of pure enumeration, we can always be deceived by strong statistical correlations which we may mistake for laws. Only the experimental methods are capable of splitting up such correlations into their strictly causal and strictly random components.

This hypothesis of the "All or None" character of nature has been one of the most fundamental and fruitful guiding principles in almost every field of science. But the recent development of Quantum Physics has proved that it is not an a priori or necessary truth. According to Quantum Physics some of the elementary laws of nature are statistical correlations which will probably never be reduced to a deterministic scheme. The change in attitude is fundamental, and even if determinism should be reestablished as a result of surprising new discoveries, the lesson taught by modern physics would remain of great importance to the theory of induction. Any assumption or "Inductive Hypothesis" (in the sense of Broad and Keynes) about the general constitution of nature is subject to possible correction in the light of new experimental facts, and can therefore be regarded only as a tentative frame-work for more special research. Any such assumption is simply one of an infinity of possibilities, and unless it is accompanied by still more general and precarious suppositions, it can not be assigned a finite probability. On this point the theories of Keynes, Broad and Nicod are seriously in error. Even on the basis of the logical interpretation of probability, the as-

sumption of the finite antecedent probability of an "Inductive Hypothesis" is untenable. These able thinkers are mistaken when they assert that a singular fact can confer a finite probability upon a general assumption. This is possible only by the exclusion of alternative assumptions. Therefore, the whole issue is prejudged. It can never be demonstrated that the principle of induction has the faintest probability except by a petitio principii.

But even if we accept the "Principle of Limited Variety" as a necessary condition of induction, it is easy to see that it is by no means sufficient. Unless we are allowed to infer from the probabilities of the chance coincidences of causally independent characters (or events) something concerning their corresponding frequencies, the principle of induction can have no significance. It is precisely the assumption of the stability of statistical frequencies which is necessary here. But of this assumption we can never be sure. The occurrence of a long chain of extremely improbable coincidences can always mislead our inductions. And there is no way to make sure that we are not living in just such an unfavorable world epoch. If we actually believed that we were so situated we would terminate all investigations and wait until the world passed into a more propitious stage. But the peculiar fact is that we are optimists, and refuse to abandon the belief that we can obtain "fair samples" of the world.

The probability of induction is therefore established on the basis of generalizations for which there is no probability at all. These generalizations extrapolate statistical frequencies, but only more special hypotheses can acquire probability with reference to such frequencies. The probability of a natural law is determined, roughly speaking, by the success-frequency of the inductive method by which it was discovered. The principle of induction, formulated in terms of the frequency theory, states simply that those regularities which have held so far without exception will be found to hold most frequently in the future.[4] According to this analysis, the probability of induction is always secondary and

[4] It should be noted that the frequency theory is still in process of completion and reconciliation with the logical theory. There are difficulties, but they do not appear insurmountable.

hypothetical, and can never be a genuine attribute of pure generalization.

But if, as we have seen, this most general presupposition of all induction cannot be shown to be appreciably probable, is there any other justification for accepting it? The usual reply—and here the influence of Kant is noticeable—is that such assumptions are necessary conditions for the possibility of knowledge in general. It is true, of course, that knowledge of nature would be impossible if there were not a certain amount of order and simplicity. But what are we to infer from this? That in our scientific investigations we must always begin with the postulate or demand of order and simplicity? But obviously it does not follow from the fact that we demand something that we get what we demand!

Our critique must seem very destructive, and it is destructive as regards illusory solutions of the problem. After the failure of all these attempts to achieve anything like an objective vindication of induction are we finally driven back to Hume's scepticism?

There are thinkers, however, who deny that Hume's analysis of causality and induction has any sceptical consequences. R. E. Hobart[5] has most convincingly shown that Hume's arguments appear sceptical only to those who desire to prove what cannot possibly be proved. Moreover, he has shown that almost everything that we call reasonable, rational or justifiable in our active life is absolutely dependent upon belief as the ultimate basis of all our significant knowledge. This ultimate belief, according to Hobart, is present as an immediate fact in every cognitive situation. Although I fully agree with him, yet I feel that for the purposes of a logical analysis of knowledge his formulations are not adequate. For, as he admits, to speak of belief is to speak in terms of psychology. Yet psychology itself is possible only through the belief in induction. Every explanation of belief in the context of psychological or biological theories presupposes induction, because it is by induction that we establish explanations. It is quite legitimate to study the phenomenon of belief from the scientific point of view, but in a systematic logical account of the

[5] "Hume Without Scepticism," *Mind*, 1930.

structure of knowledge the principle of induction is prior to the recognition of its embodiments in psychological or biological processes.

IV

What then is the nature of this principle? Its peculiarly elusive character is startling. If it is a meaningful assumption about the world, then it is no longer the most general principle of induction, it is itself inductive. And if it is stated in such terms that it can never be verified or proven false, then it does not say anything at all. How can we escape this dilemma?

A glance at the logic of deduction will provide us with an instructive analogy. In any axiomatic, deductive system the starting point of our deductions is a set of primitive propositions or postulates whose truth is either "evident" or assumed. From these we derive other propositions. But in order to do this we must have methods or rules of deduction. Important examples are the Rule of Substitution and the Rule of Inference. The one allows us to substitute logically equivalent terms for each other, the other allows us to drop true premises and assert the conclusion. These rules are not commands, but anyone who wants to perform deductions must employ them. (Professor Sheffer of Harvard calls these rules "prescripts," in contradistinction to the postulates which are "descriptive" either of facts or of logical structures.)

Analogously, the principle of induction is not a bit of knowledge, it is neither analytic nor synthetic, neither a priori nor a posteriori, *it is not a proposition at all. It is, rather, the principle of a procedure, a regulative maxim, an operational rule.*

According to the viewpoint of logical analysis, all empirical knowledge is a construction erected upon immediate experience. What this immediately given really is can be disputed, but that there must be some such "groundfloor" of knowledge is necessary if any empirical proposition is to have a meaning. If it is the possibility of verification which establishes meaning, then verification itself must consist in the comparison of elementary or atomic propositions with the given. These elementary propositions are the

raw material of knowledge. Moreover, as in the case of deductive systems, inductive science too has its prescriptive rules, and the principle of induction is undoubtedly the most significant among them. Its nature, just as the nature of the rules of deduction, can be determined only through the recognition of the function that it fulfills with regard to the goal of science.

Now the ultimate goal of science is not the achievement of a loosely connected miscellany of descriptions, but the establishment of a systematic structure of laws as a basis for explanation and prediction. The prescriptive rule, which is a direct consequence of this objective, is then the real principle of induction. It reads: "*Seek to achieve a maximum of order by logical operations upon elementary propositions. Generalize this order (whatever its form be: causal, statistical or other), with a minimum of arbitrariness, that is, according to the principle of simplicity.*" The condition of simplicity is essential, because it restricts the ambiguity of the procedure. But, since simplicity is measureable, if at all, only with great difficulty, there will usually be several ways of generalizing. This explains the case of competing scientific theories. Only when new experimental evidence is supplied, can it be determined that the one or the other theory is more complicated in that it employs more arbitrary hypotheses.

If foreknowledge is to be distinguished from arbitrary or capricious guessing, if it is to be different from dream and inspiration, no other definition can be given of the procedure of science. However, the principle does not carry in itself the guaranty of its own success. In this it is radically different from the rules of deductive inference. Here the analogy breaks down. Hume's scepticism is irrefutable if it simply emphasizes this difference. But with regard to operational rules doubt has no meaning. As long as there is knowledge in the sense in which we have hitherto understood knowledge, the principle of induction will be its inescapable guiding maxim. This is in itself an analytic proposition, the sheerest tautology, because it merely makes explicit the definition of knowledge. The attempt to know, to grasp an order, to adjust ourselves to the world in which we are embedded, is just as genuine as, indeed, is identical with, the attempt to live. Con-

fronted with a totally different universe, we would nonetheless try again and again to generalize from the known to the unknown. Only if extended and strenuous efforts led invariably to complete failure, would we abandon the hope of finding order. And even that would be an induction.

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NOTE

Questions and criticisms of those who have read the manuscript of this paper made it clear to me that it is too condensed to carry conviction. It would have to be expanded into a little book (which I hope to present sometime) in order to fully substantiate both the critical and the constructive tenets here set forth rather dogmatically. My consolation for the meantime then must be: Sapienti sat.

I wish to state also that essentially the same solution of Hume's problem as suggested here has been expounded most recently by Prof. H. Reichenbach in Erkenntnis, vol. 3, pp. 421–425. It is particularly gratifying to me that Prof. Reichenbach, after an Odyssey of attempts to found induction on probability has finally recognized our (Viennese) criticisms and is joining us now in the pragmatic view of inductive generalization.