A Treatise on Induction and Probability

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# A TREATISE ON INDUCTION AND PROBABILITY[1]

WE ARE told that this book is an "application of modern symbolic logic to the analysis of inductive reasoning." For the sake of reviewing it will be convenient to divide the book into four quite separate parts. The first (Chapters I–III) is introductory, the second part (Chapters IV–VI) deals with the Logic of Inductive Truth, the third (Chapters VII–X) discusses the Logic of Inductive Probability, and the last part (consisting of Sec. 4 in Chapter I, Sec. 4 in Chapter VI, and the final section of the book) is mainly historical.

The sections described as historical are perhaps the most interesting and valuable in the entire book. The first such section deals with the history of the attempts to justify induction. Von Wright reaches the conclusion that there is no noncircular way of justifying our belief in the truth of inductive conclusions. The other two historical sections deal with the development of the Logic of Inductive Truth and Probability, respectively. There are many illuminating remarks in the discussion, especially as to the work of Bacon and Mill in the former and as to Keynes and Nicod in the latter section. We are told that Carnap's recent work on inductive logic is known to the author "only from preliminary reports," which is presumably the reason why the results of the entire school working on "degree of confirmation" are not discussed in the book.

The first three chapters present the problems to be discussed and the logical tools to be used in the discussion. These tools require a logical system based on the lower functional calculus, plus an elementary segment of arithmetic, plus some properties of sequences. The second chapter, developing the logical tools, is the type of chapter that the reader trained in symbolic logic would feel free to omit; but in this case it would be a mistake, because Von Wright introduces an entirely new set of notations and new terminology, some of which is quite strange. E.g., sentences usually described as equivalent are called identical, and properties are said to exist only if they are not empty.

[1] George Henrik von Wright, *A Treatise on Induction and Probability* (New York, Harcourt, Brace and Co., 1952).

The former has as a consequence that the two apparently different sentences '$a \rightarrow b$' and '$\bar{b} \rightarrow \bar{a}$' are identical; the latter forces one to read '(EP) (x) $\overline{P}$(x)' as "there is a property which does not exist." The chapter is further complicated (as are some later ones) by the fact that frequently it is difficult to distinguish between definitions and theorems.

The problem of induction is given a very narrow formulation. Induction of the second order (forming of theories) is the only type considered as legitimate induction. And the theories to be formed are taken to be of the form $(x)\ [H(x) \rightarrow A(x)]$, or $H \subset A$. These assert that one property is a necessary condition for another (or sufficient condition if we consider *H* in place of *A*), or that one class is included in another. Equivalences (necessary-and-sufficient conditions) are also studied, but these are simply conjunctions of two theories of the previous type. The author does seem to realize that this is a somewhat restrictive formulation of the problem, since he speaks with regret of the way quantitative induction has been neglected by philosophers. It would appear, however, that universal implications between properties is so special a case of theories, that this form of scientific hypotheses is of use only in the elementary stages of a science — if at all. Let us contrast Von Wright's approach to that of the "Carnap school." The latter have also considered oversimplified cases, but always as a first step later to be extended to higher languages (and the reviewer has already indicated elsewhere how such an extension can be carried out). But Von Wright studies these trivial cases as an aim in itself, and the methods used do not appear to be generalizable to more practical cases.

Psychological, logical, and philosophical problems of induction are distinguished. The psychological problem of discovery and the philosophical problem of justification are both outside the scope of the book, which includes only the logical problem of analyzing inductive arguments. Because of the restrictive formulation of the problem of induction, the analysis need only consider sufficient, necessary, and necessary-and-sufficient conditions. Actually it would have sufficed to study one of these, say sufficient conditions. *A* is a necessary condition for *H* just in case *H* is a sufficient condition for *A,* and *A* is a necessary-and-sufficient condition just in case it is a necessary condition and a sufficient condition. The third chapter deals with the logic of such conditions, which is a branch of the calculus of classes, most theorems being generalized tautologies (from the calculus of propositions). Twenty-nine theorems are proved about various relations between conditions, especially when the conditions in question are disjunctions or conjunctions. The theorems do not sound utterly trivial only because the vast terminology disguises them. E.g., the reviewer has always been under the (now apparently mistaken) impression that Modus Ponens is a very simple and intuitive rule. So he was awed to find it described in the form: "From a Universal Implication and its first test-condition we can deduce its third test-condition."

The reviewer would like to argue that terminology should be kept at a minimum, and that only in the presence of overwhelming reasons should an elaborate terminology be admitted as a necessary evil. Such reasons could be the desire to abbreviate lengthy statements, to make fine distinctions, or to explicate intuitive concepts. Therefore, he feels that the introduction of terms which do not explicate anything intuitive, which take the place of familiar concepts, and which considerably lengthen the discussion is indefensible. In some cases the new and peculiar terms even serve to make otherwise utterly implausible argument seem reasonable. A case in point is the "proof" that statistical laws are of the form of a Universal Implication of two properties.

This very surprising claim is based on a perfectly correct analysis of the form of statistical laws. The claim is based, however, on the following errors (any one of which would invalidate the claim): (1) It is ignored that statistical laws assert randomness. (The author cannot treat randomness in the form of a Universal Implication.) (2) The universal implication has as antecedent that a real number is greater than 0 and as consequent that it has the property "of being associated with a point of convergence $m$ of a certain sequence towards a value $p$." This latter property, even according to the author's analysis, has two quantifiers hidden in it (namely '$(Em)$' and '$(n)$'). Only the most peculiar terminology will make it appear that this is a property like being black. (3) Even if we allow this analysis, we run into insurmountable difficulties. E.g., the number of real numbers is not denumerable, as required by the book. (4) If we take the claim literally and accept the method of the book at the same time, we are to picture scientists verifying statistical laws as follows: The scientist picks out a real number, say .1, and tosses a coin an infinite number of times to see whether the ratio of heads is always within .5 ± .1 from some toss on. After completing these infinite number of tosses, he picks another number, say .05, and repeats the process. And so on indefinitely. It seems to the reviewer that this is not a very practical procedure for testing statistical laws.

Let us turn to the discussion of Inductive Truth. This consists of the discussion of methods by which from certain initially designated properties (possible conditions) we eliminate some as not being conditions of one of the three types. Chapter IV is, in effect, a corrected and elaborated version of Mill's canons. As such it is of historical interest, and of possible use as a handbook. The author has done a service to show us what Bacon and Mill could have done had they our modern tools (and the author's great thoroughness). The reviewer cannot help feeling, however, that, after we realize that a universal implication cannot hold if in a single observed case the antecedent is true and the consequent false, the rest is a very simple logical exercise.

The fifth chapter formulates the problem (recently discussed by Bertrand Russell) of what premises must be added to inductive arguments to make them deductive. The author arrives at two postulates: The Deterministic Postulate states that there are actual conditioning properties of a given property. The Selection Postulate states two things, "first, that the range of initially possible conditioning properties includes the actual conditioning properties, and secondly, that the state of analogy among any given number of things in respect of the initially possible conditioning properties, can be settled on the basis of an enumeration of the data of elimination." It would require careful logical analysis to see whether these postulates can justify even the author's simple inductions.

The sixth chapter discusses the relation between induction and definition (that is, between the formation of theories and of concepts). It is a very good demonstration of the fact that the two processes are inseparable. It is shown how the concepts of Science develop parallel to theories, and how this makes the clear refutation of a given theory an exceptional "ideal" case — perhaps never realized. The ideal is to have theories stated in explicitly defined terms, with all conditions of applicability included in the theory; an ideal which is an unattainable goal to work towards. The discussion is very instructive; the only addition the reviewer would like to make is the assignment of a more central role to the demand for simplicity in this process of theory formation.

The Logic of Inductive Probability is discussed in the last four chapters, which are almost independent of the previous ones. First of all Von Wright presents the calculus of probability (Chapter VII), and this mathematical development is certainly one of the strong points of the entire book. There are, however, two objectionable points. The primitive relation is 'P(A, H, p)' (expressing that the probability of $A$ relative to $H$ is $p$). The use of this rather than

'P(A, H) = p' considerably lengthens statements and proofs, as has been pointed out many times.

The second fault is a material shortcoming. Reichenbach's Rule of Existence is replaced by the Rule of Elimination which is supposed to be no more than a simplification of the former. This rule states: "If a probability-expression which occurs in a formula of the calculus is quantified in the numeral, then it can be omitted from the formula." But it is a trivial theorem of the system that (Ep)P(A,H,p) → (Eq)P(A,H,q). Hence by the rule we can drop the antecedent, and by generalization prove that (A)(H)(Eq)P(A,H,q). This asserts that for any two properties there is a relative probability, a result rejected by everyone including the author. There seems to be no better way of correcting (weakening) the rule than returning to Reichenbach's version.

The formal theorems include generalizations of Bayes's and Bernoulli's theorems. These are the first instances in the book which require the full notation with the variety of superscripts and subscripts and special symbols. The reviewer, on the one hand, concurs in the need for this notation for the very general results; on the other hand, he must confess that he was unable to read these sections.

The seventh chapter concludes with a discussion of various interpretations of the abstract calculus — a discussion most interesting in what it covers, and most disappointing in its omissions. The three interpretations are based on frequencies, number of possibilities, and degrees of belief, respectively. The first one is not Reichenbach's position, because it omits randomness as a criterion; the second is not that of Keynes but the much simpler function of Wittgenstein; the third is not Carnap's, since it omits the requirement of rationality and makes it a purely psychological concept.

In section 3 of the following chapter the missing requirements (of randomness, equal likelihood of alternatives, and rationality of belief) are supplied, and very surprisingly we are shown that they all amount to the requirement that our evidence contain all (relevant) available knowledge. While the discussion is most interesting, it does not do full justice to the additional requirements. We will consider this in just one of the three cases (for the sake of brevity), that of degrees of belief. "Degree of confirmation" is usually thought of as the degree to which a purely rational being would believe in the hypothesis had he the given evidence. Rationality is the difference between the standard set up by (a yet-to-be-accepted) concept of degree of confirmation and the purely subjective degree of belief which varies from person to person. In addition and entirely independently we must also require that the evidence be taken as all the available (and relevant) information. When this is done, we arrive at what Russell calls the "degree of credibility" of the hypothesis; i.e., the degree to which a purely rational being would believe in it if he were in our place. Under this analysis, rationality would eliminate such factors as prejudice, superstition, and stupidity. According to Von Wright's analysis a person (handicapped by not reading philosophical journals) who assigns 0 probability to the sun rising tomorrow after considering all that he knows is rational, but someone assigning a most reasonable probability but ignoring just one relevant fact in his calculation is irrational.

The author repeatedly talks about "minimum" and "maximum" probabilities. If these are something different from 0 and 1 probability, then the reviewer missed some important points in the book. If they are not, then there is no reason why many theorems are burdened by this awkward terminology.

The last three chapters discuss the relation between probabilities and induction. The term "Inductive Probability" is reserved for theories, and this means theories of the very special form treated in the earlier parts. This excludes, on the one hand, most actual theories in science; on the other hand, it refuses to apply the term to predictions. Somehow it is inferred that since predictions cannot be probable in *his* inductive sense, they are therefore not relevant to inductive logic. If the reader will allow an analogy, this is exactly as if some opponent of Von Wright's decided to use the term "induction" to apply only to the formation of "non-trivial" theories (explicitly excluding universal implications), and then argue that this book does not treat induction at all.

The eighth chapter defines a number of interesting terms, which are worth considerable further study. At this point we will only mention Von Wright's Principle of Determinism. This is a principle asserting that every property within a given Universe of Discourse has some necessary-and-sufficient condition of which it is logically independent. He can prove, e.g., that if this principle holds, then there can be no chance in the world (according to his definition of "chance"). This principle is a good explication of what many philosophers have meant by determinism, and the reviewer would like to use this formulation to show that these principles (when clear) are analytically true. Given any property *A,* we will show that there always is a necessary-and-sufficient condition which is logically independent of *A*. Consider all positive instances of *A* (physical objects having this property).

Each such object has at least one property not shared by other objects (e.g., its spatiotemporal location). Imagine a list of these characteristic properties, and let $H$ be the property of having at least one (or exactly one, since they are not overlapping) of these characteristics — i.e., $H$ is the logical sum. Then $H$ is logically independent of $A$, and yet it is a necessary-and-sufficient condition. Thus we see that the Principle of Determinism is analytically true, no matter what kind of a Universe we live in! (It is also a warning that considerations like those of the eighth chapter must always be relativized to a given language if they are to be useful.)

The ninth chapter starts with a very good analysis of possible views concerning inductive probabilities. Three main categories are distinguished — the nihilistic, dualistic, and monistic views. A nihilist is one who rejects the concept of inductive probability either because he thinks it involves contradictions or because he considers it of little value for science. The former alternative is rejected, but the latter (that such probabilities are of little value) is essentially Von Wright's view. The dualist holds that inductive probability is somehow different from "ordinary" probability, either that it does not satisfy the probability axioms or at least that it requires an interpretation of these axioms different from the ordinary variety. The monist believes that one interpretation of the axioms accounts for all uses of "probability."

Then follows one of the most peculiar arguments of the whole book. The dualistic view (like Carnap's) is rejected because a frequency interpretation can be found which is applicable to all cases. After adopting this Reichenbach-like attitude, Von Wright proceeds to show that "it (this interpretation) makes Inductive Probability utterly trivial and void of practical interest." However, we are told that although this is true, it "hardly constitutes an objection." The author shows that while the frequency interpretation of all uses is logically permissible, it gives reasonable results only in the noninductive use. This would seem to be a clear-cut argument for dualism, instead of which Von Wright concludes that the frequency interpretation is the only acceptable one, but that the results bear out his nihilistic view: inductive probabilities are unimportant.

Let us examine the frequency interpretation offered for the inductive probability of theories (of his simple type). If we are to assign a probability to the theory $(x)\ [H(x) \rightarrow A(x)]$, we must have a *property* to which to assign the probability, and an evidence *property*. This leads the author to adopt an "Aristotelian analysis." The property $\phi_A$ to be discussed is that of being a sufficient condition for $A$. The evidence $\phi_o$ is the property of belonging to a given set of possible conditions. Then the inductive probability $p$ is given by $P(\phi_A, \phi_o, p)$, where the evidence is chosen as the property of being a still possible sufficient condition for $A$ (after the given observations). On the frequency interpretation given, $p$ is the proportion of actual sufficient conditions among the remaining possible sufficient conditions.

The following objections occur to the reviewer: (1) "Aristotelian analyses" have been generally discredited. (2) The interpretation is applied to finite and infinite sets of conditions, though in the latter case it is not at all clear what a proportion is. (Hence we will discuss the finite case only.) (3) The author points out that we could equally well have chosen "being a necessary condition for $H$" as the property to assign a probability to. But he does not mention that there is no reason at all why the two different ways of calculating the probability of the same theory on the same evidence should give the same value. (4) Since the choice of "possible conditions" is left entirely open, the interpretation makes the probabilities purely psychological. It is easy to see that no matter what we have observed, the probabilities can have any value at all, depending on the choice of possible conditions. (5) The probability of the theory increases only with the elimination of a possible condition. This has the advantage of simplifying the problem, and of overcoming the "paradox of confirmation." It has one disadvantage: the results bear no resemblance at all to what scientists actually do. The confirmation depends entirely on the variety of the evidence and not at all on sample size. E.g., we may observe a million white swans without adding to the probability that all swans are white. (6) Since we never know what the actual conditions are, we never know the values of the inductive probability. (7) The analysis allows only a trivial role to arguments from simplicity or from analogy.

It is the task of the logical analyst to clarify the procedures of science, and on occasion to suggest improvements. But an analysis which finds that the procedures of practicing scientists are mostly trivial or wrong is certain to be an incorrect analysis.

Chapter X attacks the attempts to found inductive logic on inverse probabilities (e.g., on Bayes's theorem). These attempts have recently been generally discredited, and Von Wright's forceful attack on them is a great service.

Although the reviewer agrees with the author's conclusions, he must challenge the treatment of a crucial example. The example is that we have observed $n$ ravens, all black, and we want to know the probability that the next raven will be black as well. Laplace's rule gives the value

$n+1/n+2$, the main assumption being that the a priori probabilities are equal. The usual analysis considers a number of (imaginary) urns, from which various birds are drawn. For each urn there is a ratio of black birds in the urn $(p_b)$, and an apriori probability $(q_b)$ that a given bird will be drawn from a given urn. Given that $n$ birds were all black, we can calculate the a posteriori probability that the draw was from a given urn, and hence that the next draw (again from the same urn) will be a black bird. Von Wright rightly points out that the calculation depends on our ignorance of which urn we are drawing from, since if we knew, then the probability will be just the $p_b$-value of the urn. But he then argues that we know in this case that we are drawing from the raven-urn, so the calculation makes no sense, since the raven urn has $p_b = 1$. But the urns are identified *not* by what kind of birds they contain, but by their $p_b$-values, and the point is precisely that we do *not* know which $p_b$-urn contains the ravens! The evidence is supposed to make it increasingly probable that we are drawing from an urn with $p_b = 1$, and hence that the next draw will be a black bird. But on the frequency interpretation there is no way of finding the a priori $q_b$-values, and this is the reason why the attempts to base inductive probabilities on Bayes's theorem have been discredited. The reason why the correct analysis of this example is important is that it shows that one of the problems of inductive logic is precisely the problem of supplying the a priori probabilities, which is strong evidence for adopting a dualistic view. Solutions of this problem have been proposed by Carnap and others, some leading to Laplace's rule in this case, and some radically differing from it.

In summary, we find that Von Wright has put a tremendous amount of deep-going research into his book. It has many valuable and illuminating sections, and the reader can profit from the various technical developments and from the historical sections. But the first part, on Inductive Truth, suffers from being restricted to an elaboration of Mill's canons as applied to a very elementary type of theory; while the second part, on Inductive Probability, attempts to make the use of probabilities in induction appear trivial. The reader should carefully examine the arguments before accepting any of the philosophical conclusions of the book.

Errata: p. 50. It is said that 0/0 is the only exception of division of integers. This is presumably meant to be $n/0$.

p. 176. In A6 the last 'H' should be an 'A'.

p. 191. On the last line after '0' insert '→'.

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