INDUCTIVE LOGIC AND SCIENCE

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The question of the usefulness of inductive logic for science and even the question of the very possibility of inductive logic are today still debated. The conception here explained differs from that of many other scholars.

First I wish to emphasize that inductive logic does not propose *new* ways of thinking, but merely to explicate *old* ways. It tries to make explicit certain forms of reasoning which implicitly or instinctively have always been applied both in everyday life and in science. This is analogous to the situation at the beginning of deductive logic. Aristotle did not invent deductive reasoning; that had gone on as long ago as there was human language. If somebody had said to Aristotle: "What good is your new theory to us? We have done well enough without it. Why should we change our ways of thinking and accept your new invention?", he might have answered: " I do not propose new ways of thinking, I merely want to help you to do consciously and hence with greater clarity and safety from pitfalls what you have always done. I merely want to replace common sense by exact rules."

It is the same with inductive logic. Inductive reasoning is likewise as old as human language. I mean here by inductive reasoning all forms of reasoning or inference where the conclusion goes beyond the content of the premises, and therefore cannot be stated with certainty.

Thus, for example, if a physicist states a new law or a theory as a system of laws on the basis of the experimental results he has found, he makes an inductive inference. So does a scientist who assumes an unknown single fact on the basis of known facts; for example, the meteorologist who predicts the weather for tomorrow, the physicist who assumes a certain distribution of the velocities of gas molecules which he cannot directly observe, a historian who tries to explain a reported act of Abraham Lincoln by hypothetically assuming a certain

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motivation which is not reported, or a statistician who makes an estimate for the unknown value of a parameter in a population on the basis of an observed sample from the population. Since inductive logic merely intends to explicate common ways of inductive reasoning, the question of its usefulness leads back to the general question: Is it desirable that procedures which are generally applied, though only intuitively or instinctively, are brought into the clear daylight, analyzed and systematized in the form of exact rules? Whoever gives an affirmative answer to this general question will acknowledge the importance of the special problem of explicating inductive reasoning, that is, of constructing a system of inductive logic with rules as exact as those of the older, well-established system of deductive logic. Whether any particular system proposed as a solution for this problem is workable and fruitful is, of course, another question.

As I see it, the fundamental concept of inductive logic is *probability*. All inductive reasoning is probability reasoning. However, the word ‘probability’ is not unambiguous. I refer here to probability in one particular sense, the logical sense, which we might call *inductive probability*. This concept must be clearly distinguished from probability in the statistical sense. The distinction is practically important and theoretically fundamental.

Statistical probability is a certain quantitative physical characteristic of physical systems. Like any other physical magnitude it is to be established empirically, by observations. In this case the observations are of a statistical nature. They consist in counting frequencies. Statistical probability is obviously very closely connected with frequency, but it is not just the same as frequency. When we say that for a given die the probability of throwing an ace is 0.158, then this statement refers to a physical characteristic of the die and thus is not fundamentally different from statements about its mass, temperature, electric conductivity, etc. Imagine a fictitious physicist who, like the Laplacean superman, knows the present microstate of the die in terms of the distribution of the particles and of the fields and, in addition, all the relevant laws. This physicist could, by purely mathematical calculations, find not only the present temperature of the die, its conductivity, etc., but also the probability of its yielding an ace if thrown under specified conditions. Since the micro-state is actually not known. the question arises how to test a statement about the probability. how to confirm or disconfirm it. The answer in the ease of probability is not fundamentally different from that in the case. of temperature or other physical magnitudes. The statement is to be tested by making experimental arrangements which lead to observable phenomena connected with the magnitude in question, whose value itself is not directly observable. To test the probability statement, we determine the relative frequency of aces in a sufficiently long series of throws of the die. This frequency is itself not the probability; it is rather a consequence of the probability state of the die, a consequence which is observable and therefore may serve for us as a symptom for the probability state, just as the expansion of the mercury column in the thermometer is not itself the temperature but an observable consequence of the temperature state and therefore a suitable means of testing a statement about the temperature. It is sometimes said that the statistical concept of probability involves a peculiar difficulty, since obviously no finite series of throws is sufficient to determine the probability with absolute precision and certainty. This is indeed true, but the same holds for all physical magnitudes. There is likewise no possible procedure for determining the temperature with absolute precision and certainty. The answer to the question: “How long then shall we make the series of throws with the die in order to determine the probability?” is the same as the answer to the question: “How fine a thermometer should we use to measure the temperature?” In both cases the answer depends, on the one hand, on the time and money available and, on the other hand, on the desired degree of precision. More specifically, it depends on the theoretical or practical advantages to be expected from higher precision. The finer the thermometer and the longer the series of throws, the higher the precision which is achieved. In neither case is there a perfect procedure. The concept of statistical probability may be introduced either by an explicit definition in terms of a limit as done by Mises and Reichenbach, or by an axiom system with rules of application as done by the majority of contemporary statisticians. In either case, the concept is logically legitimate and practically useful for work in statistics and in all branches of science which apply statistical methods. Thus I do not agree with those representatives of the inductive concept of probability, like Keynes and Jeffreys, who reject the statistical concept. On the other hand, I do not agree with Mises, Reichenbach, and the statisticians, who reject the inductive concept. Both concepts of probability are important for scientific work, each in its own field, the one within science itself, the other in inductive logic, which gives rules for certain operations with the statements of the language of science. The statistical concept is today generally recognized. Although certain problems connected with it are still under investigation, a defense of its legitimacy and usefulness is no longer necessary. The status of the inductive concept, however, is still debated. Therefore, today it is still necessary to defend its right of existence and to show its usefulness.

A statement of inductive probability states a relation between a hypothesis and a given body of evidence, e.g., results of actual or possible observations. The asserted probability value means the degree to which the hypothesis is confirmed or supported by the evidence. It is important to notice that a statement on inductive probability or *degree of confirmation is* relative to the evidence. This does not merely mean that the statement is based on or derived from observations. That is the case for *every* scientific statement. For example, the statement "The probability that it will rain tomorrow is 1/5" is incomplete unless we add "with respect to such and such an evidence", e.g., certain meteorological observations. For the validity of the statement it does not matter whether the evidence referred to in the statement is true and whether it is known to the speaker. To be sure, in the practical application of any inference, whether deductive or inductive, the premises are usually known. But that is not necessary. They may be unknown or they may even be known to be false. If this is so, what can be the basis of the validity of the probability statement itself, as distinguished from the validity of the hypothesis or the evidence? It can obviously not be of an empirical nature. All relevant empirical knowledge or assumption is contained in the evidence statement. In our example, we can empirically reexamine the truth of the evidence concerning past meteorological events. If we wait until tomorrow, we can empirically test the truth of the hypothesis that it will rain tomorrow. But in neither way can we test the truth of the probability statement itself. We shall see tomorrow either rain or not-rain, we may observe a rain of short or of long duration, a rain of high or low intensity, but we shall not see a rain of probability 1/5. Some critics of inductive logic have pointed to this fact and drawn from it the conclusion that, since the inductive probability statement is not empirically testable, it must be scientifically meaningless. Their mistake was that they regarded the probability statement as a factual synthetic statement. The statement is, however, of a purely logical nature. Hence there is no need and no possibility of empirical testing. A statement of inductive probability is in one respect similar to a statement in deductive logic: the relation between the hypothesis and the evidence which it asserts is a logical relation, similar to the deductive relations of deducibility or incompatibility, though weaker than those. If the statement asserts a probability value close to 1, then the probability relation hereby expressed is very close to the relation of deducibility : the hypothesis is nearly deducible from the evidence but not quite. On the other hand, if the stated probability value is near to 0, then the probability relation is close to the deductive relation of incompatibility: the hypothesis is nearly incompatible with the evidence but not quite. For any intermediate probability value the probability relation is more remote from the deductive relations which are, so to speak, the extreme cases. Thus inductive probability means in a sense partial deducibility. It is a logical relation inasmuch as it can be established, just as a deductive relation, as soon as the two statements of hypothesis and evidence are given, by merely applying logical analysis, in this case the rules of inductive logic, without the use of observations. Although the statement expresses only a logical relation, it has nevertheless significance. It draws boundaries to reasonable conduct. For example, if the probability of rain tomorrow is 1/5 with respect to the evidence available to an observer, then it would not be reasonable for him to bet on rain tomorrow at odds higher than 1:4.

If we recognize that statements on inductive probability have a purely logical character, then we are in a position to clear up a question which has been debated for two hundred years the problem of the so-called principle of insufficient reason or *principle of indifference.* As I see it, the beginning of the development of inductive logic was made in the classical theory of probability by men like Bernoulli, Bayes, and Laplace. Many points of the classical theory, and among them also fundamental points, have been criticized for more than a hundred years and especially in our century. I think that this criticism is correct to a large extent. I agree with the critics that today it is impossible to go back to the classical conception. But I do not agree with those who say that the only way out is the total rejection of the classical conception. The classical principle of indifference states: “If no reasons are known which would favor one of several possible events, then the events are to be taken as equally probable." The usual objection against this principle is that it puts a premium on ignorance; if you do not know anything about the alternatives, then the principle allows you to make a certain statement about them; if you know certain things, then that statement is no longer permissible. To derive a statement from ignorance looks rather absurd. And it would indeed be absurd to apply this procedure to a factual statement. But the statement of equiprobability to which the principle of indifference leads is, like all statements on inductive probability, not a factual but a logical statement. If the. knowledge of the observer does not favor any of the possible events, then with respect to this knowledge as evidence they *are* equally probable. The statement assigning equal probabilities in this case does not assert anything about the facts, but merely something about the logical relations between the given evidence and each of the hypotheses; namely, that these relations are logically alike. These relations are obviously alike if the evidence has a symmetrical structure with respect to the possible events. The statement of equiprobability asserts nothing more than the symmetry.

For these reasons I believe that the basic idea of the old principle of indifference is valid. On the other hand, there can be no doubt that many of the applications of this principle, especially in the earlier period, were invalid and that some of the conclusions drawn were even outright absurd. But I believe that the aim which the classical pioneers envisaged was valid. Our task is not to abandon entirely the classical conception, but to construct an exact concept of degree of confirmation explicating the classical conception of inductive probability on a more cautious and more solid foundation.

Now let us look at the function of inductive logic in the field of science. A scientist makes, on the one hand, observations of natural phenomena or of results of experiments. These constitute his evidence. On the other hand, he entertains hypotheses concerning facts not yet observed or even unobservable. The hypothesis may concern a single fact or it may be a conditional prediction of the form "If we were to do such and such things, then such and such events would happen", or it may have a general form, e.g., a statement about the value of a material constant, or a general law stating the relations between various physical magnitudes in terms of mathematical functions. The law may have a deterministic form or a statistical form, stating e.g., proportions, averages, or other statistical parameters of distributions of certain magnitudes. The purpose of the law is to explain known phenomena and predict new ones. The task of inductive logic is not to *find* a law for the explanation of given phenomena. This task cannot be solved by any mechanical procedure or by fixed rules; it is rather solved through the intuition, the inspiration, and the good luck of the scientist. The function of inductive logic begins *after* a hypothesis is offered for examination. Its task is to measure the support which the given evidence supplies for the tentatively assumed hypothesis. In particular, the task will often be to determine among several competing hypotheses the one which is most strongly confirmed by the given evidence. The competing hypotheses may, for example, concern the possible results of an experiment to be made, the possible causes of an observed event, or possible outcomes of a business investment. Or they may be various laws which are mutually incompatible, each of which might be regarded as an explanation of a given set of observational results concerning new phenomena not explained so far.

Sometimes an objection is raised against the idea of a system of inductive logic with exact rules for the determination of the degree of confirmation because of the fact that a scientist who chooses one among a number of considered hypotheses is influenced in this choice also by many non-rational factors and that he would not be willing to hand over the task of this choice to a machine or to have himself, so-to-speak, transformed into a machine which merely applies fixed rules. Now it is true that many non-rational factors affect the scientist's choice, and I believe that this will always be the case. The influence of some of these factors may be undesirable, for instance a bias in favor of a hypothesis previously maintained publicly or, in the case of a hypothesis in social science, a bias caused by moral or political preferences. But there are also non-rational factors whose effect is important and fruitful; for example, the influence o£ the "scientific instinct or hunch". Inductive logic does not intend to eliminate factors of this kind. Its function is merely to give to the scientist a clearer picture of the situation by demonstrating to what degree the various hypotheses considered are confirmed by the evidence. This logical picture supplied by inductive logic will (or should) influence the scientist, but it does not uniquely determine his decision of the choice of a hypothesis. He will be helped in this decision in the same way a tourist is helped by a good map. If he uses inductive logic, the decision still remains his; it will, however, be an enlightened decision rather than a more or less blind one.

In addition to judging the status of hypotheses, inductive logic has also the task of supplying rules of *estimation.* There is much discussion and controversy among statisticians concerning the validity of particular methods of estimation and the choice of a suitable method of estimation in a given problem situation. I believe that, if the basis of inductive logic is constructed by laying down rules for calculating the degree of confirmation, then it is possible to define a general estimate function in terms of degree of confirmation, applicable to all kinds of magnitudes expressible in the language in question. The definition which I propose takes as the estimate of the magnitude on the basis of a given body of evidence the weighted mean of the possible values of the magnitude, the weight of each value being its degree of confirmation with respect to the given evidence. This is the same as the expectation value of the magnitude (if we understand-the term 'expectation value' in the inductive sense based on inductive probability, in contrast to its statistical sense based on statistical probability). To obtain a general method of estimation would be of great importance not only from a *theoretical* point of view but also for the problem of determining *practical* decisions in a rational way. Suppose a man has to make a decision in a given economic situation, e.g., concerning investments. This means that he has to choose one among a number of alternative actions possible to him in the situation. For each possible action he considers the various possible outcomes in terms of money gained. If he is able to determine the degree of confirmation for each possible outcome in the case of the considered action, he may calculate the sum of these gains, each multiplied with its degree of confirmation. This will be his estimate of the gain in the case of the action considered. In the same way, he may calculate the estimate of the gain for each of the possible actions. Then, if he is a rational man, he will choose that one among the possible actions for which the estimated gain has its greatest value. (A more exact procedure would consider, not the gain in terms of money, but the utility of this gain, i.e., the measure of satisfaction derived by the person from the gain.) Thus inductive logic serves as an instrument for the determination of rational decisions.

As mentioned earlier, the development of inductive logic began with the classical theory of probability. However, its systematization as a branch of modern logic is of recent origin, beginning with John Maynard Keynes thirty years ago. I have constructed a set of rules of inductive logic for a simple language system, which is restricted to qualitative descriptions of things without the use of measurable magnitudes (like temperature, electric current, etc.), but including statements of frequencies. These rules make possible the calculation of the degree of confirmation for any hypothesis and any body of evidence expressible in that language system and the calculation of the estimate of a frequency on the basis of any given evidence. The further development of inductive logic for more comprehensive language systems and finally for the language of science as a whole remains a task for the future.

A theory of inductive logic is systematically developed in my book *Logical Foundations of Probability,* Chicago, 1950. The underlying conception of inductive probability is explained in *The Nature and Application* of *Inductive Logic,* Chicago, 1951, which is a reprint of six non-technical sections from the book mentioned.