# THE PARADOX OF INDUCTION AND THE INDUCTIVE WAGER

*A Spanish version of this paper was read to the Asociación Costarricense de Filosofía, San José de Costa Rica, August 11, 1960; the present text is that read to the 1960 International Congress for Logic, Methodology and Philosophy of Science at Stanford, California, August 25,1960.*

Problems of self-reference in philosophy have been at the bottom of many of the now classical paradoxes, and this paper will attempt to show that paradoxical conclusions follow from an analysis of the self-reference of induction. The conditions necessary for the generation of paradoxes always include a negation; for example, in Grelling's paradox of heterological terms, it is only when a term is *not* descriptive of itself that difficulties arise. Similarly, in this case the paradox rests on the assumption that the principle of induction has not been successfully proven. It has often been remarked that induction cannot be relied upon for a proof of itself; but if other proofs had been successful, the continued reliability of inductive inferences would only serve to confirm the principle more fully. If other proofs are not successful, the continued reliability of inductive inferences is, in a sense, an embarrassment. I shall assume that the latter is the case, and shall try first to formulate the source of embarrassment and second to show that, although some authors have gone to extraordinary lengths to avoid a confession of defeat with respect to induction, capitulation is not as dishonorable as it might seem.

Before beginning, however, it is necessary to state which problem of induction is in mind, since one problem has proliferated into many by a process, sometimes referred to as "transformation," in which a closely related but soluble problem is substituted for the original insoluble one. The problems that have been solved include the development of a logical theory of probability, the use of the statistical syllogism, and so on; the original problem, and the one with which I shall be concerned, is that of inferences as to future events drawn from past observations. As used here the term "future events" covers also the future discovery of information about past or distant or concealed events. The difficulty is expressed in the following passage from Hume, whose "statement of the case against induction," as Keynes says, "has never been improved upon":[1]

> 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.[2]

And again:

> 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?[3]

To this challenge Hume found no answer, and this fact is responsible for his reputation as the worst pessimist in the history of induction. Nevertheless, he himself had the greatest confidence in the principle. There is an interesting passage in the *Enquiry* where he actually does apply an inductive procedure to the problem of induction: "This negative argument," he says

> must certainly, in process of time, become altogether convincing, if many penetrating and able philosophers shall turn their enquiries this way and no one be ever able to discover any connecting proposition or intermediate step, which supports the understanding in this conclusion.[4]

But there are obviously two sides to this question, and a little later he appears to have changed to the other:

> 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 on any subject, it may still, perhaps, be rash to conclude positively that the subject must, therefore, pass all human comprehension.[5]

This is exactly the situation in which we find ourselves. "Many penetrating and able philosophers," at least, if not "all the learned," have attempted to discover logical grounds on which a demonstration of the certainty of inductive inferences could rest; many more, since the abandonment of the search for certainty, have tried to do the same for its probability. No proposal for a solution, so far put forward, can claim to have been successful. That the outlines of an inductive logic, based on the theory of probability, now exist, cannot be denied; but this does no more to solve Hume's problem than the fact that there exists a Euclidean geometry helps to make the universe Euclidean. It has puzzled many thinkers that such a gulf should exist between deductive inferences, which everybody agrees to be binding in all circumstances where the premises are true, and inductive inferences, which appear so uncertain; the attempt has therefore been made to locate both kinds of inference on a continuum, so that inductive inferences would be just like deductive ones, only less so. But one circumstance renders all such attempts abortive. If the conclusion of a deductive argument is false, this at the same time renders the premise false, and this may be known immediately; if, for example, it is asserted that all S is P and hence that this S is P, the observation that this S is not P makes it false, according to the usual meaning of "all," to assert that all S is P. No such relation of necessity is available for induction, and if the inference is rephrased to make one appear, it becomes a deductive inference. It seems to me that Hume was right in locating the crux of the argument in necessary connection, and that he was also right in the assumption that the only relation which might provide such a necessary connection would be a causal one. The latter point will be referred to again.

* * *

Let it be conceded, then, at least for the purposes of argument, that all attempts to solve the problem of induction have so far been unsuccessful. Let the *i*th attempt at solution be called $A_1$; then (overlooking the considerable difficulties involved in identifying the A's) we might exhibit a series

$$A_1, A_2, \ldots\ldots A_n,$$

to which the methods of induction could be applied. If we let U stand for the predicate "unsuccessful," the state of affairs may be described by the sentence "All A's so far observed are U." This can clearly serve as the premise of an inductive inference, the conclusion of which will be "All A's are U," or "Probably all A's are U," or "At least 99% (or some other figure, depending on the theory adhered to) of A's are U." The making of such an inference depends, of course, on the reliability of the principle of induction. The assumption of the reliability of the principle leads, therefore, to the conclusion that it is probably indemonstrable. Conversely, if somebody were at last to produce a convincing argument for its validity, that would in effect justify us in saying that what he had done was impossible, or at least highly improbable, since it provided a counterexample of a generalization of a type whose soundness had just been demonstrated.

The longer we go on using the principle of induction, then, the less likely we are to find a justification for it. This is what I have called the "paradox of induction." It is not a rigidly formalized paradox - the introduction of probability prevents that - but whatever variety of inductive theory is employed, conclusions which are, at least to a degree, paradoxical result. For example, if one uses Sir Roy Harrod's ingenious formulation,[6] the hopes for success of a new solution can be dampened by pointing out that one is always likely *not* to be on the verge of a great philosophical discovery.

One objection springs to mind immediately. Nobody considers it paradoxical, for instance, that after years of research a solution should be found to some scientific problem, although it had eluded previous generations; why then should it be so for a philosophical problem? The answer to this is, of course, that the scientific problem yields to new evidence, but that in the philosophical case no new evidence is available. In principle, the fact that different animals require different groups of proteins is just as mysterious as the fact, which intrigued Hume so, that bread is nourishing for men but not for lions and tigers. The causal relation, objectively speaking, is as ineffable as it ever was. It is not inconceivable, I suppose, that new evidence might be forthcoming, and this would be the only way in which a theory of induction could escape the paradox; but it is difficult to imagine what might constitute new evidence in this sense. Williams remarks that:

> The solution of the problem of induction must be at bottom as banal and monolithic as the process of induction itself. Philosophers and logicians have walked around and over our principle for centuries...;[7]

and it is to be supposed that they have seen most of what there is to be seen.

What really convinced Hume of the hopelessness of the situation with regard to induction was the inaccessibility of future data. In his discussion of causality he suggests that there are three elements in a prediction - an observed event, a predicted event, and a causal mechanism, corresponding on the logical side to a premise, a conclusion, and an inductive principle respectively - two of which are needed for the determination of the third, as two sides of a triangle are needed for the determination of the third. If the observed and predicted events are both available they may be taken as defining the causal relation, and this is the way in which the word "cause" is generally used. In the logical case, if premise and conclusion are both known, some probability relation may be established between them, and this may serve as the paradigm of an inductive inference. But where the predicted event has not yet been observed, where the conclusion is not known, the situation is like that of trying to guess where the rest of a triangle lies, if one is given one side. Without further information the task is impossible, and the only way to get further information is to wait. In the absence of any other principle we use, of course, the relation defined by previous sequences of observations; but that the new case will conform to the pattern cannot be known until it has already done so.

Science constructs theories which are designed to fit as closely as possible evidence that is already in, and relies on them, as it bound to do, when it is necessary to speculate as to future states of affairs. The theories present a more or less finished appearance, and as conceptual structures may be explored again and again without revealing any flaws; they may even be rigorously axiomatized and exhibited as logical systems. This in itself does not compel the world to behave as they say it will, and if it behaves differently changes are made in the theories. If physicists had resolved to cling tenaciously to apparently reasonable principles - principles of symmetry or of conservation, for instance - to the extent of demonstrating their logical necessity, there would have been more difficulty than in fact there has been in adjusting to recent developments. The same considerations apply to the principle of induction, which is simply the most general and inclusive theory we possess. I am not suggesting that a disproof of the inductive principle is likely - if it is not verifiable it would not seem to be falsifiable either. Verifiability and falsifiability, as methodological tools, are not as different as they are sometimes thought to be; whenever a crucial test arises, the principle of double negation turns the one into the other. But the principle of induction needs logical foundations as little as the conservation principles needed them; and if they are not needed it hardly seems worth a great deal of effort to supply them.

* * *

It is claimed, however, that logical foundations are needed - that their absence is a "scandal" which is likely to have dire consequences for civilization.[8] This kind of language betrays a concern which is more than philosophical. We do in fact rely on the principle; it has in fact worked, up to this point; we are shocked at our inability to justify our actions logically. We are in the position of people who, as Pascal says, have been acting on an uncertainty without knowing why. "Rem viderunt, causam non viderunt." [9]

Pascal was the first to use a mathematical theory of probability as justification for action on uncertainties, although in a rather unlikely context:

> If we must not act save on a certainty, we ought not to act on religion, for it is not certain. But how many things we do on an uncertainty, sea voyages, battles! ... Now when we work for tomorrow, and do so on an uncertainty, we act reasonably; for we ought to work for an uncertainty according to the doctrine of chance ... St. Augustine has seen that we work for an uncertainty, on sea, in battle, etc. But he has not seen the doctrine of chance which proves that we should do so...[10]

It is quite possible to agree with him that acting on chances is acting "reasonably" in the broad sense of the term (which does not mean "logically") without following to the conclusion of his argument, which is the existence of God - for this passage is taken, of course, from the section of the *Pensées* entitled "The Necessity of the Wager." Similarly, it is quite possible to agree with writers on induction who say, as Williams does,

> It remains none the less reasonable to wager our lives and fortunes where our chances are best,[11]

without following to the conclusion, reached by some in a manner strikingly similar to Pascal's, that a wager can justify induction as a metaphysical principle. This, however, seems to be the logical outcome of some recent proposals.

The two writers in whose treatments of induction the parallel with Pascal is most striking are Reichenbach and J. O. Wisdom. Both authors agree that the problem does not admit of a straightforward solution, either affirmative or negative, just as Pascal admitted that neither of the two propositions "God is, or He is not," could be defended according to reason. And just as Pascal presented two alternative modes of action - to believe or not to believe - so in the case of induction there is a choice - to trust inductively-confirmed statements, or not to trust them. Nature may or may not be such as to vindicate our trust - in Reichenbach's language the world may or may not be "predictable," [12] in Wisdom's the universe may be "favorable" or "unfavorable." [13] We are, in effect, invited to wager on the former possibility, since the odds are heaviest on that side. Although neither of these authors believes himself to have solved the problem exactly as Hume set it - both, in fact, agree with Hume's main criticisms - nevertheless each claims to have removed the problematic elements from it. Reichenbach speaks straightforwardly of "the justification of induction which Hume thought impossible," [14] while Wisdom solves the problem only after "transformation."[15] In both cases their conclusions penetrate beneath immediate strategic necessity to a more fundamental level.

There is a distinction to be made here between recommendations as to strategy - the maximizing of the chances, assuming a regular universe in which we know less than we would like, as practiced in the theory of games - and conclusions as to principle. Many authors stand behind the theory of probability as the best tool we have for guiding our practical decisions, and in this case the wager remains unchallenged - it is what we actually use. But this is not the point at issue. As far as practical affairs were concerned, as was pointed out earlier, Hume too knew and used the principle of induction, and would no doubt have been happy to learn and use also modem methods of probability. He was aware that this fact might be held against him - a kind of *ad hominem* argument based on such a discrepancy between belief and practice appears in nearly all treatments of the subject - and in the following important passage from the *Enquiry* took precautions accordingly:

> 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 scepticism, I want to learn the foundation of this inference.[16]

The foundation has been taken to lie in a metaphysical principle - the Principle of the Uniformity of Nature, of the Principle of Sufficient Reason, or the like. Such principles can be used to justify anything; happily, this kind of metaphysics is increasingly in disrepute. The principle needed is metaphysical, however, in Collingwood's sense,[17] in that it is an absolute presupposition of scientific activity. It appears to me unfortunate to suppose that a wager can be properly used to justify such a principle. If we ask ourselves what is the status of a concept which is made the subject of an intellectual wager - what, for instance, the existence of God meant to Pascal - we have to answer that it is that of something to which there is passionate attachment. Pascal already believed in God; the wager was a rationalization of his belief for the benefit of his wordly friends. Similarly, when Reichenbach says:

> It is better to try even in uncertainty than not to try and be certain of getting nothing,[18]

or Wisdom:

> We must not, however, slur over... the possibility that the universe is favourable,[19]

one is not impressed with a conviction of genuine uncertainty, of genuine doubt as to the nature of things; these devices are merely the best that can be done to provide visible support for a belief which is already stronger than any such devices could possibly make it.

*　　*　　*

Today we are not, most of us, moved by Pascal's argument. If the arguments of Reichenbach and Wisdom appear more compelling, that is because of our historical perspective. The conflict between religion and the world is more or less quiescent; science, together with the philosophy of science, occupies an area of active concern. Induction has an importance to us now that the existence of God has not; we are therefore more sympathetic to proposals for providing it with a logical foundation. But the truth or falsity of the principle of induction is not affected by our efforts, any more than the truth or falsity of the existence of God is. Electing one side of the other, *as a result of logical calculation,* is in any case futile. A regular world, viewed *sub specie aeternitatis,* is a fantastic improbability; an irregular world, viewed from our temporal standpoint, is equally a fantastic improbability. "It is incomprehensible," says Pascal, "that God should exist, and it is incomprehensible that He should not exist." [20] In our niche of space and time it seems foolish not to trust the principle of induction; in Pascal's, it seemed foolish to question God's existence. I do not doubt that in his circumstances he was right, and I do not doubt that in our circumstances we are right, but that gives us no reason for claiming the philosophical immutability of the principles to which we subscribe. Wagers are appropriate to limited objectives, not to ultimate metaphysical commitments. Neither Reichenbach nor Wisdom, perhaps, intends to give the impression that an ultimate metaphysical commitment is in mind, but by bringing in the notion of the world in which series converge to limits coincident with "best posits," [21] the universe in which regularly unfalsified hypotheses remain unfalsified,[22] they have moved into metaphysical territory, where gambling is out of place.

The principle of induction is left, therefore, unverified, unfalsified, and apparently empty and useless. Some critics might be tempted to say that this end could have been reached much more quickly by the employment of a meaning criterion, or something of that sort, which would have shown from the beginning that the principle could say nothing. But that would have been appealing to one more unnecessary assumption. I have preferred to show the impossibility of its logical proof in another way, by locating it among the paradoxes, and to show that some attempts at such a proof, in fact, appeal to something quite apart from reason. This is far from saying, of course, that the principle is uninteresting or unimportant. While it need not always do so, the discovery of a paradox may indicate a profound truth. It was this, perhaps, that Unamuno had in mind when he defined a paradox as "a proposition which is at least as evident as the syllogism, only not as boring."

PETER CAWS. UNIVERSITY OF KANSAS.

---

[1] J. M. Keynes, *A Treatise on Probability,* London, Macmillan, 1921, p. 272.
[2] David Hume, *An Enquiry Concerning Human Understanding,* Section IV, Part II.
[3] *Ibid.*
[4] *Ibid.*
[5] *Ibid.*
[6] R. F. Harrod, *Foundations of Inductive Logic,* London, Macmillan, 1956, *passim.*
[7] Donald Williams, *The Ground of Induction,* Cambridge, Massachusetts, Harvard University Press, 1947, p. 21.
[8] *Op. cit.,* ch. 1, *passim.*
[9] Blaise Pascal, tr. W. F. Trotter, *Pensées,* New York, Random House (Modern Library), 1941, p. 84, no. 235.
[10] *Loc. cit.,* no. 234.
[11] Williams, *op. cit.*, p. 62
[12] Hans Reichenbach, *Experience and Prediction,* Chicago, University of Chicago Press, 1938, p. 350.
[13] J. O. Wisdom, *Foundations of Inference in Natural Science,* London, Methuen, 1952, p. 226.
[14] Reichenbach, *op. cit.,* p. 348.
[15] Wisdom, *op,. cit,,* ch. XXIV.
[16] Hume, *loc. cit.*
[17] R. G. Collingwood, *An Essay on Metaphysics,* Oxford, at the Clarendon Press, 1940, *passim.*
[18] Reichenbach, *op. cit.,* p. 363.
[19] Wisdom, *op. cit.,* p. 229.
[20] Pascal, *op. cit.*, p. 79, no. 230.
[21] Reichenbach, tr. Maria Reichenbach, "The Logical Foundations of the Concept of Probability," in Feigl and Brodbeck, *Readings in the Philosophy of Science,* New York, Appleton-Century-Crofts, 1953, p. 466.
[22] Wisdom, *op. cit.,* p. 226.