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INDUCTION

The sceptical problem about the future, often called the problem of induction, was first published in 1739, in David Hume's *A Treatise of Human Nature*. It doubts that any known facts about past objects or events give any reason for beliefs about future objects or events. A similar problem arises also for inference about unremembered past events, and unobserved present ones, but I shall adopt Hume's own format. Will this bread nourish me? Hume argues that no collection of past observations on alimentation give any reason at all for thinking that the next piece of bread will also prove nourishing. Our expectations are formed by custom and habit, but lack justification.

Closely related is the sceptical problem about generalizations. Can any number of observed instances, short of a complete survey, ever make it reasonable to believe a generalization? The work of Hume has itself lent some credence to the view that particular predictions must be based on sound generalizations. Many philosophers think this problem equivalent to the problem about the future. Whether or not we agree with this supposedly Humeian doctrine, when it is not necessary to distinguish the two problems, we may speak simply of the sceptical problem about induction.

The *sceptical* problem is not to be confused with what may be called the *analytic* problem. Clearly people do distinguish good inductive reasons from bad ones, so we may begin to classify the various degrees of evidential support. This analytical task has been very substantially advanced in the twentieth century by philosophically minded statisticians. Clearly their predecessors broached the same problem long before Hume. Bernoulli did so in the fourth book of *Ars conjectandi*. Leibniz had a vision of inductive logic. Arguably Pascal also wanted to analyse non-deductive inference. We have quoted Hobbes, as early as 1640, 'if the signs hit twenty

times for one missing, a man may lay a wager of twenty to one of the event'. That is at least a preliminary quantitative analysis. All these writers took for granted that, in Hobbes' words, 'they shall *conjecture best*, that have *most experience*, because they have most signs to conjecture by'. Slowly it was recognized that mere quantity of experience is not enough. The careful design of experiments can yield more food for conjecture in a week that the passing show of signs can deliver in a lifetime. But there is no doubt in anyone's mind that some signs do give good reason for beliefs about the future and about the unwitnessed past. Hume's sceptical doubts were unknown before 1739. Why?

The question is particularly pressing for the historian of probability because of what appears to be Hume's own view of the matter. In 1740 he published an anonymous advertisement for the *Treatise*. In this *Abstract* he tells us that,

The celebrated Monsieur Leibniz has observed it to be a defect in the common systems of logic that they are very copious when they explain the operations of the understanding in the forming of demonstrations, but are too concise when they treat of probabilities, and those other measures of evidence on which life and action entirely depend and which are our guide even in most of our philosophical speculations [. . .] the author of *A Treatise of Human Nature* seems to have been sensible of this defect in these philosophers [Arnauld, Malebranche, Locke] and has endeavoured, as much as he can, to supply it [1740, p. 7].

It may be that Hume was merely appealing to the current vogue for probability. The arguments from design, which originated with John Wilkins in the birthtime of probability, had culminated in Joseph Butler's *Analogy of Religion* in 1736, although the defective logical form of these arguments, cast as the character Cleanthes in Hume's *Dialogues Concerning Natural Religion*, was yet to appear. Butler, with a vastly greater audience than Hume, had already told the world it was not his 'design to inquire further into the nature, the foundation and the measure of probability [. . .] This belongs to the subject of logic, and is a part of the subject which has not yet been thoroughly considered' [1736, p. iv]. With less circumspection than Butler the same divines, who debated what proportion of revelation and what proportion of natural argument should be allowed in the foundation of religion, were incessantly quarrelling over how much probability to attach to the testimony of miracles in various epochs. Hume had already written his essay *On Miracles*, but kept this

bombshell secret until [1748], when it prompted more critical study in the next two years than his work on induction was to receive for a century. So perhaps in his *Abstract* Hume was giving vent to justifiable pride that he understood the probability of design and of testimony far better than any contemporary. Or perhaps he was merely pandering to the current penchant for probabilizing. But I think Hume also thought that he could present his problem of induction by grace of his thorough grasp of probability. If so, why should not Hobbes, in 1640, have thought a little harder and propounded the same problem? Are we to suppose that what is commonly acknowledged as one of the great landmarks of epistemology occurred almost at random, and could as well have happened any time in the preceding century?

Is it not entirely clear, however, that the sceptical problem is Hume's. If we are liberal in our interpretations, we can, of course, always find anticipations and precursors. The most likely is the brief discussion in Sextus Empiricus' second century *Outlines of Pyrrhonism*. I quote Book II, chapter xv in full:

It is also easy, I consider, to set aside the method of induction. For, when they propose to establish the universal from the particulars by means of induction, they will effect this by a review of all or some of the particular instances. But if they review some, the induction will be insecure, since some of the particulars omitted in the induction may contravene the universal; while if they are to review all, they will be getting at the impossible, since the particulars are infinite and indefinite. Thus on both grounds, as I think, the consequence is that induction is invalidated.

This might be read out of context as the sceptical problem about generalizations, but in fact it occurs in the course of a long discussion of demonstrative proof. Sextus has (like J. S. Mill's *System of Logic* one and a half millenia later) been accusing the syllogism of committing a *petitio principii*. If we use a premise, 'All *A* are *B*', to prove that this *A* is *B*, we must be begging the question. To ward off the objection that one might obtain 'All *A* are *B*', in some other way, he points out that induction is invalidated. There is no demonstrative proof, but Sextus does not, in this passage, contend that there is no reason, nor that inductive reasons are not reasons. Indeed he is seemingly content with much inductive inference. He strongly opposes those who favour the indicative sign, by which we infer something that is in principle unobservable from what has been observed. He had no truck with theoretical entities. But he is

happy with the associative sign, which we use to infer what is at present unobservable from what is at present observed:

The associative sign is relied on by living experience, since when a man sees smoke fire is signified, and when he beholds a scar he says there has been a wound. Hence, not only do we not fight against living experience, but we even lend it our support by assenting undogmatically to what it relies on, while opposing the private intentions of the dogmatists [*Ibid.*, II, 102].

The 'associative sign' is that which indicates an object or event which at present is hidden from us, but which we can at least in principle discover later. Sextus has, like a good modern positivist, been opposing the indicative sign, which was supposed to lead us to theoretical entities that are in principle unobservable. His scepticism anticipates many of the concerns of a modern logical empiricist, but we do not find him here enunciating any sceptical problem about the future. Nor can we regard his criticism of inductive generalization as a sceptical problem about induction, for he is opposed only to illegitimate use of the syllogism. This interpretation is supported by Stough's [1969] analysis of these texts.

The sceptical problems about induction arise in quite another context. To understand it, we must retrace some of the ground of Chapters 3–5 above. There are two distinct questions: 'How did probability become possible?' and, 'How did the sceptical problem of induction become possible?' The answer to the first question has primarily to do with a transformation in the mediaeval concept of *opinio*. The result was a concept of 'internal evidence', i.e. of evidence other than testimony. In scholastic epistemology opinion was probable when well attested. Then the world began to testify by its signs. So the probable sign is the sign through which the world gives testimony. Moreover signs may be imperfect and only 'very often' right. Frequency and credibility are thus linked. When conventional and natural sign are finally distinguished, it is the latter that furnish 'internal' evidence. With these transformations in hand, the dual concept of probability was possible. The analytic problem of induction was also possible for as soon as there was a concept of internal evidence, men could start to order the different degrees to which hypotheses are supported. But the sceptical problem of induction remained unknown. To understand why we must examine transformations in the concept of *scientia* or knowledge. Although these are not so essential to the formation of the dual concept of probability, they are integral to the sceptical problem of induction.

Opinion was the staple of low science while knowledge was the goal of high science. Paracelsus was the 'Luther of the physicians', as Copernicus was the Luther of the astronomers. One consequence of their twin revolution was that knowledge and opinion, formerly disparate, entered the same league. Or rather, what happened was that a substantial part of the potential domain of knowledge, including astronomy and the investigation of motion, became part of the domain of opinion. In the writing of Hume, the term 'knowledge' is reserved for pure mathematics. This agrees with the scholastic conception of knowledge as demonstration from first principles. But Aquinas thought one could demonstrate causes and thereby explain why things are as they are. For Hume, demonstration is a matter of the 'comparison of ideas'. This operation can be performed chiefly in the realm of mathematics. Cause, on the other hand, is relegated to the other scholastic category that Hume variably calls 'opinion' or 'probability'. Once the concept of internal evidence was established by 1660, the final transformation needed for the sceptical problem of induction was this transference of causality from knowledge to opinion.

In much modern discussion of Hume it is inadequately noticed how closely, albeit reluctantly, he hews to the established categories of 'knowledge' and 'probability'. A great deal of recent English epistemology has meandered around such questions as 'do I know I am not dreaming now?' or, 'do I know that I have a hand before me?' Philosophers who argue from the meaning of the verb 'know' in ordinary English wish to answer 'yes' to these questions. In the *Treatise* the answer is as a matter of course 'no'. It accepts that what 'knowledge' means is first principles, demonstrations, and comparison of ideas. Hume is certainly attentive to established usage and regrets an inconsistency in it. Probability, from scholastic times, had a pejorative element (as noted e.g. in the quotation from Byrne in Chapter 3). Now that the category of knowledge is relinquishing everything except pure mathematics, the category of 'probability' or of 'opinion' will include items which we cannot complain of as being 'merely probable'. ''Tis however certain, that in common discourse we readily affirm, that many arguments from causation exceed probability, and may be received as a superior kind of evidence.' Modern linguistic philosophers have cited this as the beginning of good commonsense attention to 'common discourse' finally breaking through the clouds of scholasticism. Hume's

remark is indicative of something else. Although he employs the categories of knowledge and opinion, he strips the former of causation. In the common discourse of 1739 many arguments derived from cause and effect were not called (merely) probable, because they had been candidates for knowledge which is (*a*) opposed to probability and (*b*) encompasses causes. The latter feature is ended by the time of Hume, but some reasonings from causes retain the former feature. Hence they are not, now, knowledge. Yet they have never been (mere) probability, so 'they may be received as a superior kind of evidence'. Hume calls these 'proofs' that fall short of demonstration, but when he presents his argument about induction he treats them under the head of probability.

It is clear why the sceptical problem of induction requires a transformation in *opinio*: without that, there is no concept of internal evidence about which to be sceptical. It should also be clear why Hume can begin only when causation is stolen from knowledge. So long as causes were the subject of demonstration from first principles, there would still exist necessary connections between cause and effect, and in particular, necessary connections between a present event (a cause) and a future one (an event). The necessary connections were contingently necessary. That is, it is a contingent matter of fact that a particular theory, founded on a given set of first principles, is in fact the theory of the world. But given that it is the theory, then (to use a modern way of expressing the appropriate notions) the very meanings of the terms in the theory are settled by the theory, and so the propositions of the theory are analytic. That is why I say that in the scholastic view, propositions of cause and effect are contingently necessary. Notoriously Hume spends many pages demolishing the idea of necessary connection. That done, his basic sceptical problem is stated succinctly. An expectation that the future will be like the past must be either knowledge or opinion. But all reasoning concerning the future must be based on cause and effect. Reasoning concerning cause and effect is not knowledge. Therefore it must be opinion, or probability. But all probable reasoning is founded on the supposition that the future will resemble the past, so opinion cannot be justified without circularity. Knowledge and probability are exhaustive alternatives. Hence expectation about the future is unjustified.

To understand the preconditions for this argument we need to investigate knowledge and causation. A proper scrutiny demands a

full re-examination of seventeenth century 'high science'. I shall be content with the very end, when the scholastic goals of high science have been severely eroded. Robert Boyle, in making the low science of alchemy into the high science of chemistry had much to do with that erosion. The alchemists, although dreaming of causes, had to be content with signs. They believed that the world worked according to its primary qualities, but they could only experiment on the secondary qualities. There was still the belief that there were true necessary connections among the primary qualities that made everything go. If I may be forgiven the crudeness in such a brief sketch, Boyle, for the first time succeeding in getting behind the phenomena, found no scholastic causes. He speculated about primary qualities, but necessary connections were nowhere in sight. Hence the whole conceptual scheme of a demonstrative knowledge of primary qualities was disintegrating. The final stage in this disintegration gives us an immediate key to a sceptical problem of induction. This is the theory of gravitation. It is only a final stage, and not even an essential one, but it is a good reminder of the state of the old 'knowledge'. In the preceding chapter I have cited numerous Royal Society theologians contending that the non-mechanical law of gravity serves only to describe constant regularities in the universe. Newton, the chief glory of physics, has not come up with the goods. Where we had longed for causes and rational demonstration, we found only constant conjunction and lawlike regularity.

Berkeley's reaction is instructive. In a late work, *Siris*, he attacked the corpuscular philosophy of Boyle and Locke on the ground that it never finds efficient causes. It is restricted to seeking 'the general rules and methods of motion and conformity' [1744, p. 111]. Earlier, in *De motu*, written about 1720, he had stated clearly that,

It is not, however, in fact the business of physics or mechanics to establish efficient causes, but only the rules of impulsions or attractions, and, in a word, the laws of motions, and from the established laws to assign the solution, not the efficient cause, of particular phenomena [sec. 35].

This idea of Berkeley's is not fully derived from physics – he is in truth reporting a widespread view that exactly coincides with his philosophy. In Sec. 31 of the *Principles*, published in 1710, he mentions the 'sort of foresight' provided by what are called laws of nature: food nourishes, to sow seed in seedtime is the way to reap the harvest, and so forth. He asserts that all these things we know

'not by discovering any *necessary connection* between our ideas, but only by the observation of the *settled laws of nature*'. Moreover, in *Towards a New Theory of Vision*, he indicates that this rejection of necessary connection has far deeper roots than a problem about gravity:

Upon the whole, I think we may fairly conclude that the proper objects of vision constitute an universal language of nature ['an universal language of the Author of nature' in the 3rd edition] whereby we are instructed how to regulate our actions [. . .] It is by their information that we are principally guided in all the transactions and concerns of life. And the manner wherein they signify and mark out unto us the objects which are at a distance is the same with that of languages and signs of human appointment, which do not suggest the things signified by any likeness of identity of nature but only by an habitual connection that experience has made us to observe between them [sec. 147].

'*The connection of ideas does not imply the relation of cause and effect but only of mark or sign with the thing signified. The fire which I see is not the cause of the pain I suffer upon approaching it, but the mark that forewarns me of it.*' With this passage in sec. 65 of the *Principles*, truly, as Michel Foucault says, 'Hume has become possible.'

The knowledge that divined, *at random*, signs that were absolute and older than itself has been replaced by a network of signs built up step by step in accordance with a knowledge of what is probable. Hume has become possible [Foucault 1970, p. 60].

Cause and effect – the paragon of the old knowledge that was demonstration – and signs, the purveyors of opinion, have become one. The sceptical problem of induction is possible. Or rather, in stating the sceptical problem of induction, Hume completed that historical transformation by which the signs of the low sciences became identical with the causes of the high. Berkeley had said that the things we commonly take for causes – such as the fire – are not really causes. They are signs uttered by the Author of the universal language, and that Author is himself the efficient cause. Physics investigates not efficient causes but mere regularities which we know about not by 'any likeness of identity of nature' but by 'an habitual connection'. The causes lie with God. Hume enunciates the final twist. The fire is, after all, the efficient cause, but like all efficient causes it is only a sign!

Hume, then, completes the Berkeleyan syllogism. Causes are signs, but the signs suggest the things signified 'only by an habitual connection'. Reasoning by cause and effect is thereby 'habit and

custom only'. We can indeed find such notions verbally prefigured in the coarse philosophizing of those Royal Society theologians who wrote about constant regularity and the new theory of gravitation. But they are not the source of Hume's thinking. They merely express what is happening to the concepts of the time. They conveniently mark the end of the old 'knowledge' because the whole republic of letters begins chanting that the greatest known law of nature is a 'mere constancy' learned by experience which leaves us ignorant of the efficient cause. Yet even in terms of superficial historical 'influence' speculation about gravity did not much move Berkeley. It was an afterthought used for example in *De motu*. Indeed if one examines the main 'influences' on Berkeley's thought one is directed back to the more profound symptoms of the breakdown in knowledge. One of the chief precursors of Berkeley's doctrine, namely Malebranche's theory of occasionalism, was intended to solve the problem of interaction of mind and matter by conceiving of 'feelings' and sensations as signs that God constantly presents to the mind. Although we think of this as philosophical psychology, Leibniz reminds us that it began as physics. After distinguishing minds from matter Descartes had supposed that a mind interacts with material substance at a geometric point, perhaps in the pineal gland. Leibniz insisted that this is bad physics. Descartes had inadequate conservation laws. He knew force is conserved, but did not know that conservation is vectorial. Thus force at a point could, so far as physics was concerned, be directed any way. So the forces which the human body brings into play are determined by the laws of physics, but the direction of their application is extra-physical, that is to say, mental. Only when Leibniz discovered the true conservation laws was he bound to invoke pre-established harmony to replace interaction between mind and matter. Martial Guéroult has amply shown how that doctrine arose chiefly in order to solve problems in dynamics. Long before Hume, and actively rejecting any law of gravity, Leibniz had the idea of 'constant conjunction'. Minds and bodies 'express' each other, and one body, in being, as we say, 'affected' by another, is better described as 'expressing' the other. Arnauld not unnaturally asked Leibniz what this meant. Leibniz replied: 'one thing expresses another, in my use of the term, when there is a *constant and regulated relation* between what is true of the one and what is true of the other'. [*P.S.* II, 112].

Leibniz's philosophy is one of the last desperate defences of the old category of knowledge. He had to believe that there is no interaction between the real things in the universe: there is only 'constant and regular relation'. Moreover, material objects can only be 'well founded phenomena'. He could even write, 'If a thing is not actually sensed, then there is no thing.' Many of the Humeian ideas are present in Leibniz, but one is lacking. For Leibniz, the category of knowledge is still sacrosanct. Truth is ultimately demonstration. Efficient causes may be constant conjunction but final causes will constitute the reason for things. There is a sufficient reason for any truth and it can be proven *a priori*. Where cowards were surrendering the outworks of knowledge to a concept of opinion increasingly fortified by a concept of evidence, Leibniz counter-attacked with one last marvellous innovation. Knowledge had always been demonstration from first principles. Leibniz produced the first 'modern' analysis of proof as formal relationship between sentences. A demonstration of a logically necessary proposition p will be a finite sequence of sentences terminating at p. A proof of a contingent proposition q will be an infinite sequence asymptotically converging to q. Thus all truth is swept into the category of knowledge by refurbishing the concept of demonstration.

Leibniz has been our constant witness to events in probability from 1665 until 1713. He was the first philosopher of probability and anticipated, often in great detail, many of our modern probabilistic conceptions. His lack of anticipation of a sceptical problem about induction – at the very time that he was inventing inductive logic – is as significant a testimony as any. It reminds us that there could be no problem about induction until *scientia* was abandoned. Probability emerged from the Renaissance transformation in *opinio*. That sufficed for an analytic problem about induction. The sceptical problem could arise only when causation had moved from knowledge to opinion. Thus although the emergence of probability is a transformation in opinion, the emergence of 'probability-and-induction' is a more complete event depending on parallel transformations in high science and low science.