THE EMERGENCE OF PROBABILITY

A PHILOSOPHICAL STUDY OF EARLY IDEAS ABOUT PROBABILITY, INDUCTION AND STATISTICAL INFERENCE

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IAN HACKING

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EVIDENCE

Many modern philosophers claim that probability is a relation between an hypothesis and the evidence for it. This claim, true or false, conceals an explanation as to the late emergence of probability: the relevant concept of evidence did not exist beforehand. The way in which it came into being has much to do with the specific way that the dual concept of probability emerged. One of the preconditions for probability was the formation of this concept of evidence.

What concept of evidence? Crudely, that which some philosophers have called 'inductive evidence'. The label is inaccurate, but at the beginning it can remind us of the philosophers' problem of induction, almost always attributed to David Hume's *Treatise*, published in 1739. Some elements of this problem may have been anticipated in the *Outlines of Pyrrhonism* [II, 204], written by the Greek sceptic, Sextus Empiricus (*c.* A.D. 200). But aside from odd and fragmentary passages almost certainly dedicated to other problems we find no hint of a problem of induction until Hobbes, or, better, Joseph Glanvill's *Vanity of Dogmatizing* of 1661. All modern students of epistemology agree that the problem of induction is of fundamental importance. Most of the other basic problems can be identified throughout the whole Western tradition, and have classic texts in Plato or Aristotle. Why is what C. D. Broad called the scandal of philosophy – the problem of induction – such a newcomer on the scene? There is a simplistic answer. Until the seventeenth century there was no concept of evidence with which to pose the problem of induction!

There are defects in this answer. First, despite such intimations as one may find in Glanvill in 1661, it is significant, and explicable, that the problem of induction had to wait in the wings some eighty years after the birth-decade of probability. As I shall explain in Chapter 19, Glanvill merely raises the flag over a new philosophical continent, discovered at the time of probability, but which cannot be

exploited until other events have occurred. But our simplistic answer is partly right. A concept of evidence is a necessary condition for the stating of a problem of induction. A problem of induction does not occur in the earlier annals of philosophy because there was no concept of evidence available.

'Evidence', however, is far too imprecise a term. Of course some concepts of evidence have been around for a very long time. In this chapter I propose to define one concept of evidence which, I claim, was lacking. In the next chapter I shall describe the terms in which it came into being. My definition of this concept of evidence must, of necessity, be by way of exclusion. I shall describe a number of different kinds of evidence that were not lacking, and label these in various ways. What all of these leave out is something like what our philosophers have come to call 'inductive evidence'.

Concepts of testimony and authority were not lacking: they were all too omnipresent as the basis for the old medieval kind of probability that was an attribute of opinion. Testimony is support by witnesses, and authority is conferred by ancient learning. *People* provide the evidence of testimony and of authority. What was lacking, was the evidence provided by *things*. The evidence of things is not to be confused with the data of sense, which, in much modern epistemology, has been regarded as the foundation of all evidence. On the contrary, we should be concerned with that kind of evidence that J. L. Austin has nicely distinguished from sheer looking:

> The situation in which I would properly be said to have *evidence* for the statement that some animal is a pig is that, for example, in which the beast itself is not actually on view, but I can see plenty of pig-like marks on the ground outside its retreat. If I find a few buckets of pig food, that's a bit more evidence, and the noises and smell may provide better evidence still. But if the animal then emerges and stands there plainly in view, there is no longer any question of collecting evidence; its coming into view doesn't provide me with more *evidence* that it's a pig, I can now just *see* that it is [1962, p. 115].

The evidence that will concern us, then, is not the 'evidence of the senses'. In Austin's examples, it is the evidence of things, such as the pig bucket, and perhaps also the noticeable noises and smells. These olfactory and auditory objects are not private experiences but rackets and stenches as public as pigsties.

The evidence of things is distinct from testimony, the evidence of witnesses and of authorities. Probably Austin did not mention witnesses because they seem parasitic on the evidence of things. We

rely on them when we can not be at the scene ourselves. We use authorities when we are ignorant. People and books, whether they be authorities or chance witnesses, seem to stand in place of ourselves. They report on evidence that they have been able to acquire, and so it seems to us that they are not the basic kind of evidence. The Renaissance had it the other way about. Testimony and authority were primary, and things could count as evidence only insofar as they resembled the witness of observers and the authority of books.

Our form of the distinction between these two kinds of evidence, testimony and the evidence of things, is quite recent. It was clearly stated in 1662, at the end of the Port Royal *Logic*. The authors call the evidence of testimony *external* or extrinsic. The evidence of things is called *internal*. One may find this distinction a few years earlier, for example in Hobbes, but it is, in the hands of these authors, a new distinction. It is our distinction, and characterized in a way that we understand: the primary evidence, the evidence of things, is 'internal', and thereby basic, while testimony is 'external'.

I claim not only that the distinction is new, but also that the very concept of internal evidence was new. Internal evidence must not be confused with verisimilitude. We say that a proposition has verisimilitude when it is a proposition of the sort that is true. For example, when in 1440 Lorenzo Valla (*c.* 1406–57) exposed the fraudulent Donation of Constantine, he did so in a way that modern textual critics find very strange. Indeed, as one of these has remarked to me, 'he did not use any *evidence* at all!' Lorenzo instead considered whether the Donation is the sort of thing that could have happened. Constantine, according to documents, donated the Roman Empire to the Church after his miraculous cure from leprosy. Lorenzo imagines a long conversation between Constantine, giving the Empire to Pope Sylvester, and Sylvester declining. No Emperor would ever give away his dominion, nor any Vicar of Christ accept it. And look at the very prose, continues Lorenzo: it is not the sort of thing to occur in an historical document.

Modern textual critics take solecisms and historical anachronisms as evidence that a text is faulty or fraudulent. That is a case of one thing (these particular words) serving as evidence against the claim that the whole text is sound. Just like Austin's pig-food, they are instances of one thing being evidence for another. We can recognize

the production of some evidence in Lorenzo's polemic, but Lorenzo himself is not arguing that way. He is saying that this document is not like a true document: it lacks verisimilitude. Evidence, in my usage, is a matter of inferring one thing from another thing, while verisimilitude is a matter of one thing being, or not being, what it seems or pretends to be.

The kind of evidence that I have in mind consists in one thing pointing beyond itself. This must be further clarified. It is non-deductive pointing. A single observation that is inconsistent with some generalization points to the falsehood of the generalization, and thereby 'points beyond itself'. But this pointing is by way of *reductio ad absurdum*, a demonstrative form of argument. Such form of argument was well known to the *scientia* of medieval times and the early Renaissance. Here is a typical example, by the Archbishop of Canterbury, John Pecham (*c.* 1230–92).

> Proposition 28: *Sight occurs through lines of radiation perpendicularly incident on the eye.* This is obvious, for unless the species of the visible object were to make a distinct impression on the eye, the eye could not apprehend the parts of the object distinctly. [Lindberg 1970, p. 109].

This is from a manuscript which, under the name *Perspectiva Communis*, circulated widely in the fourteenth century. Whether or not the argument be persuasive, the form of the argument seems plain enough. Sight occurs through lines of sight perpendicular to the eye, or it does not. We have a known fact inconsistent with the latter, so the former must be true.

Demonstration, testimony and verisimilitude were quite well understood at the beginning of the Renaissance. Only internal evidence was lacking. Now to say that there was no concept of internal evidence is not to say that people did not use what we call evidence. Doubtless men have long inferred that there was a pig in the thicket from the sound, smell, and broken branches. But dogs and boars can tell there is a pig, and do not thereby have a concept of evidence. We do not deny that men in the Renaissance were able to take advantage of what we call the evidence. I deny that their description of this practice was at all like our description, or even fits into any present category.

Naturally I here make no claim about Sanskrit or Greek concepts of evidence. I am concerned with a specific lack at a particular time, and am interested in what stood in place of evidence. This, as we

shall see, was the 'sign'. What happened to signs, in becoming evidence, is largely responsible for our concept of probability. We cannot even speculate about how another concept of probability might have emerged elsewhere at another time, from the transformations in another culture.

It will be my claim, in the next chapter, that the concept of internal evidence of things is primarily a legacy of what I shall call the low sciences, alchemy, geology, astrology, and in particular medicine. By default these could deal only in *opinio*. They could achieve no demonstrations and so had to resort to some other mode of proof. The high sciences, such as optics, astronomy, and mechanics, still lusted after demonstration and could, in many cases, seem to achieve it. They could scorn *opinio* and any new mode of argument. New modes of argument arose, perforce, among the students of opinion. I shall be using some of the more bizarre examples taken from the hermetics because they so forcefully illustrate what seems to me to be important, but we can find exactly the same emergence of the 'sign' and the new kind of evidence in the sane and cautious words of the geologist Agricola (1490–1555) who remained in the established cloisters, as we shall find in the drunken speculations of the itinerant physician Paracelsus (1493–1541).

Before proceeding to the study of signs, I should make a distinction between evidence and experiment. There is an ongoing debate among historians of science as to the roots of the 'experimental method'. Some historians attribute the method to the growing self awareness of the new mechanics. Their chief hero is Galileo, a man who, even if he did not experiment as much as was once thought, admired and imagined many experiments. Other historians emphasize the role of the low sciences, emphasizing the bizarre laboratories of the new physicians and alchemists. Yet a third school of history claims that there are different experimental traditions that converge in the seventeenth century. Since I shall be discussing the origin of the concept of something like 'inductive evidence' it may seem as if I can contribute to this debate about origins, but that impression is largely illusory.

To begin with, we may distinguish, abstractly, numerous kinds of experiment. I shall call them, for ease of reference, the test, the adventure, the diagnosis, and the dissection. The *dissection* is a matter of taking something apart to see what is inside. It has a primarily visual motivation. The early dissections of Vesalius and

his peers have been much studied in the history of science, although undoubtedly the more recent positivist thesis, that seeing is believing, has distorted our understanding of what was once done in the dissecting room. The *test* is entirely different, and operates by that inner seeing which is deduction. One tests an hypothesis H when H implies that if event E occurs, then result R will follow. One endeavours to make E occur. If R fails to follow, then H is confuted. If R does follow, H is thereby corroborated. We have come to think of a positive result of a test as somehow conveying inductive evidence for H, but that was not the original intention, for there was no concept of inductive evidence. Passing the test was often called a proof of H. Here proof bears that old sense we still find in expressions like 'printers' proofs' or, 'the proof of the pudding is in the eating'.

The test is conducted in circumstances where, if one believes the theory, one has firm expectations about the outcome. An *adventure*, in contrast, is guided by no good theory and we may only guess what will happen. Much early alchemy seems to have been adventure. You heated and mixed and burnt and pounded to see what would happen. An adventure might suggest an hypothesis that can subsequently be tested, but adventure is prior to theory.

An adventure is an end in itself. Indeed, the ultimate aim may be to make gold or to find out more about the universe, but the adventure is done for its own sake. To this we contrast the *diagnosis*. In a diagnosis, for example, you add substances to the urine of a sick man, collect the precipitate and pound it. Perhaps you can only guess the outcome, but this is not a pure adventure. Rather, from the character of the precipitate you infer what is wrong with the patient. The surgeon cuts up live people and the anatomist dissects the dead, but the physician must be content with reading the signs in his laboratory.

Tests, adventures, dissections and diagnoses all provide 'evidence'. The evidence that they provide is of differing kinds. The test demonstratively refutes an hypothesis, or else corroborates it. The adventure suggests a theory. The dissection exhibits the inner working of man and beast. My preceding discussion has excluded all these kinds of evidence. The Middle Ages possessed a concept of each kind of evidence provided by such experiments. Only the diagnosis gains, in the Renaissance, a new conceptualization. It uses a thing, the precipitate, as evidence for another thing, the state of

man's insides. It is not a matter of simply looking, nor a matter of testing, nor a matter of guessing a new law in the light of an adventure. It is the evidence of one thing that points beyond itself.

The 'experimental method' is truly of many kinds and has many origins. The internal evidence of things need not be conceptualized before there is experimental method. The diagnosis has not that much to do with the origin of the experimental method. It may, however, have something to do with the interpretation in the seventeenth century, when 'the experimental method of reasoning' became exalted above all else. It became fashionable to regard all experiment as what I have been calling diagnosis. In the old Aristotelian tradition *scientia* was to proceed by the demonstration of effects from first causes. In the new science, one was to infer the causes from experiment. The old causes got at the essence of things. The new causes were efficient causes, explaining how things were made to work. You inferred the efficient causes from experiment. You inferred something small, inner, atomic, and precise from something, large, outer, gross and inaccurate. Just as the physician read the state of his patient from the signs in the urine, so the scientist was supposed to read the state of the atomic world from his crude diagnostic tools. In this way the test, for example, was transformed. The tests of the old *scientia* were demonstrative, and the result of passing a test was just that: passing a test. But in the new philosophy of the inductive sciences the result of passing a test was to get new inductive evidence for the hypothesis. One was, as it were, diagnosing the good health of the hypothesis. Karl Popper's methodology of science, brilliantly expounded in his *Logic of Scientific Discovery,* is an attempt to cast out from science the alchemists, the physicians, and their diagnostic experiments, returning science to a plain demonstrative model.

We can here better understand a certain ambiguity in the philosophers' term of art, 'inductive evidence'. It has come to mean two things. On the one hand is evidence for a generalization or even for a law of nature, gained from particular observation and experiment. On the other is the induction from particular to particular. Hume, in fact, chiefly considers the latter, as when he wonders whether *this* piece of bread before me is nourishing. J. S. Mill went so far as to claim that all inference is from particulars to particulars, generalizations being merely the schema of particular inference. In the Renaissance the evidence of particular things for particular

things emerged first. The 'proof' of generalizations earlier used deductive modes of inference, as in my quotation from Pecham. When all experiments began to be conceived of as diagnosis, one was no longer diagnosing the state of the hidden liver, but rather the hidden laws of the universe, and so inductive inference for generalizations, and induction from particulars to particulars, become conceived of as in the same line of business.

Thus I do admit that my thesis on the origin of the concept of evidence may connect with current debates on the experimental method. This is not because our low scientists were peculiarly experimental, but because one kind of experiment in which they engaged had much to do with the subsequent interpretation of all post-Aristotelian science. Doubtless the technology devised by the proto-chemists affected what men did, but the true effect, of lasting importance to the new civilization, may lie in how men thought about what they did. Probability and the new understanding of experiment both had as their preconditions a transformation of an old concept of sign into a new concept of evidence. That we must now describe.