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## OPINION

Probability began about 1660, but the word 'probability' is, in languages drawing on the Latin, a good deal older. The prehistory of probability can usefully begin with a study of earlier meanings of the very word. Its link with numerical ideas of randomness seems first to have occurred in print only in 1662. Some English philosophers, stimulated by an interest in 'ordinary language', and perhaps suspicious of three centuries of success in making probability mathematical, have recently emphasized some pre-1662 aspects of the word. They go so far as to say that even today the primary sense of the word is evaluative. Thus according to W. C. Kneale, 'if we heard a man speak in ordinary life of the equal probability of various alternatives, we should understand him to mean that they are equally approvable as bases for action' [1949, p. 169]. Or again: in the 'common or garden' usage of the word 'probable', 'it is an evaluative term. To say that a proposition is probable is more like saying that it's right to do so and so' [Körner, 1957, p. 19]. Stephen Toulmin [1950], John Lucas [1970] and others have subsequently expressed similar views.

Undoubtedly the Latin word *probabilis* did mean, among other things, something like 'worthy of approbation', but I very much doubt if Kneale's account of the present 'common or garden' usage is right. One way to question it is to note how odd is the sound of older bits of speech in which 'probable' really did mean approvable. The impossibility of the old locutions shows how much the meaning has shifted, and it will also help to lead us back to even earlier senses of the word. A couple of centuries ago one readily spoke of a 'probable doctor', apparently meaning a medical man who could be trusted. We no longer speak that way. For a more striking example, consider a passage in Daniel Defoe's 1724 bawdy novel, *Roxana, or The Fortunate Mistress*. Early in her career the lady in question,

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having got a man with a big house, says of herself, 'This was the first view I had of living comfortably indeed, and it was a very *probable* way, I must confess, seeing we had very good conveniences, six rooms on a floor, and three stories high.'

Since 'probable' had this connotation of approval it may seem reasonable to expect that when, in an antique work, the word is used to qualify some proposition, then the author is saying that the proposition is 'worthy of approval' because it has the marks of truth or is better supported by evidence than any other hypothesis. Such a conjecture requires much caution. Nowadays, according to J. R. Lucas, 'We use the words "probable", "probably", . . . to give a tentative judgement. There is some reason, but not conclusive reason, for what we opine.' Drawing attention to this claim, a reader of *The Times Literary Supplement* wrote to the editor [9 April 1971]:

it seems that for Gibbon in the eighteenth century [the words] had quite a different sense. Summing up a discussion of the conflicting accounts of Hannibal's route across the Alps in the ancient authors, he wrote in his journal for October 24 1763, 'Let us conclude, then, though with some remainder of scepticism, that although Livy's narrative has more of probability, yet that of Polybius has more of truth.' Still more surprisingly to a modern ear, he wrote in a footnote in Chapter xxiv of the *Decline and Fall*, '*Such a fact is probable but undoubtedly false.*'

Such quotations may usefully shake up our preconceptions before we start a serious reading of the documents. Here is a final example. Throughout the first half of the eighteenth century there had been considerable controversy in Britain over the relation between miracles and testimony, and various pieces of probability lore were injected into the argument. In 1748 Hume made a particularly sensational attack on the credibility of miracles, based on his view of probability. This invited a host of serious replies, all of which convey some information about the current understanding of probability concepts. One of these books, by Thomas Church, considers the question of whether a fact can be credible or incredible in itself, 'distinct from the consideration of any testimony'. The author is at pains to insist that credibility is relative to the evidence. Church grants,

that in common discourse it is not unusual to call any thing credible or incredible, antecedent to our consideration of its proof. But if we examine our ideas, this will be found to be a loose unphilosophical way of expressing

ourselves. All that can be meant is, that such a thing is possible or impossible, probable or improbable, or, at farthest, happening very frequently, or very seldom [1750, p. 60].

Here we see an array of all the concepts that have come to cluster around probability: credibility, frequency, possibility and the like. Probability is kept separate from each. Usages like those of Defoe, Gibbon and Church were at the end of the line, and began to die out as mathematical probability became more and more successful. They were the very standard earlier. Clearly we should not expect various formations of the word 'probability' in different languages naturally to translate into our own word. What then was the core of the preceding meanings?

E. F. Byrne has recently published a quite thorough study of probability concepts as they occur in the work of Thomas Aquinas, who is thus both a convenient and natural starting point. The first thing to grasp, as Byrne insists, is the distinction between knowledge and opinion in medieval thought. It is indeed an ancient distinction that we all associate with Plato, but here we are concerned with its manifestation in Aquinas. It contrasts strongly with all modern epistemology. For a good number of years, now, philosophers have been debating whether knowledge is justified true belief. Even when this brisk definition is rejected, it is very widely accepted that if  $p$  is true, then one person may believe  $p$  while another, in a happier epistemological state, may know the very same proposition  $p$ . Knowledge and belief are in the same line of business. If we are to understand the Thomistic doctrine we must adopt a different stance. Knowledge, so far from being justified true belief, does not even have the same objects as opinion.

In medieval epistemology, science – *scientia* – is knowledge. Knowledge is knowledge of universal truths which are true of necessity. The necessity in question is not identical to our concept called 'logical necessity' – a concept that did not properly exist until the seventeenth century. Aside from knowledge of first truths, which are so simple and fundamental that they are beyond disputation, knowledge is arrived at by demonstration. One of the requirements of Thomistic knowledge is that we have 'right' concepts. Take a simple example, the proposition that 'the plague is transmitted by fleas'. It is not possible to know this until at the very least we possess enough epidemiology to distinguish bubonic from pneumonic plague, and enough parasitology to distinguish the relevant kinds of

flea. Some kinds of plague are transmitted by an organism that involves a kind of flea in its life cycle, and some are not. When we have that sort of understanding of pestilence, then we may begin to frame definitions that characterize the concepts in the scientifically relevant way. Of course it is contingent that those are the right concepts – the world might have been different. But once we do have an adequate theory of plague, it will determine the meanings of terms in such a way that it is at least plausible to say that by definition a particular kind of bubonic plague is transmitted by a particular member of the Siphonaptera – it is just that interrelation that is part of the characterization of the two species in question.

Let us now consider a resident of Cairo in 1837 who conjectured that the plague then infesting his city was transmitted by fleas. We may think more highly of him than his neighbour who attributed the plague to miasma. We have real respect for the Londoners of 1603 who blamed imported cotton for their plague: there is a particular parasite that needs a flea that needs cotton. But let us ask the question, whether the Egyptian of 1837 or the Londoner of 1603 believed something that we now know to be true. It is not unnatural to say that the propositions about the plague now accepted in epidemiology simply did not exist a century or more ago. Bubonic and pneumonic plagues had not, for example, been distinguished, and the very concept of a host parasite was undreamt of. There is a certain family resemblance between present knowledge and old opinions, but arguably no proposition central to modern theories of the plague is identical to any proposition believed a century ago. It is not the case that an old opinion,  $p$ , has become modern knowledge. The old opinion was in a sense incommensurable with modern knowledge. This usage of the term 'incommensurable' was popularized in the 1960s by Paul Feyerabend. Although the theory of scientific theories that accompanies it is rather at odds with recent positivism, it fits in quite well with some tenets of scholastic epistemology. There is, however, one fundamental difference. Aquinas thought that real infallible knowledge is a genuine goal and is sometimes attained. Knowledge is both distinct from and better than opinion. A Feyerabend might say instead that all our beliefs and theorizing are in the domain of opinion, and we should expect that any theory will have to be replaced by another one.

Aquinas' *opinio* refers to beliefs or doctrine not got by demonstration. It may also cover propositions which, not being universal,

cannot (according to Aquinas) be demonstrated. *Opinio* tends to refer to belief which results from some reflection, argument, or disputation. Belief got from sensation is called *aestimatio*. In scholastic doctrine opinion is the bearer of probability. The limit of increasing probability of opinion might be certain belief, but it is not knowledge: not because it lacks some missing ingredient, but because in general the objects of opinion are not the kinds of propositions that can be objects of knowledge.

Even if we have apprehended the notion of opinion we are still far from medieval probability. We may expect that an opinion is probable if there are good reasons for it, or if it is well supported by evidence. This is not the primary sense that Aquinas attaches to probability, and it is instructive to see why. In his mind reason and cause are very closely related. To comprehend the reason for *p* is to understand the cause, to understand why *p*. Causes in turn are to be found in the real definitions that underlie the science. That is, all reasons are demonstrative, because causes are necessary causes. We have come to think that deduction is only one way of giving reasons, and that much evidence falls short of deduction. For the medieval, evidence short of deduction was not really evidence at all. It was no accident that probability was not primarily a matter of evidence or reason. Probability pertains to opinion, where there was no clear concept of evidence. Hence 'probability' had to mean something other than evidential support. It indicated approval or acceptability by intelligent people. Sensible people will approve something only if they have what we call good reason, but lacking an adequate concept of good reason Aquinas could handle only actual approval. Here is a typical statement about opinion and probability:

Since, then, the dialectical syllogism aims at producing opinion, the dialectician seeks only to proceed on the basis of the best opinions, namely what is held by the many or especially by the wise. Let us suppose, then, that one encounters in dialectical reasoning some proposition which could in fact be proven through a middle term but which on account of its probability seems to be self-evident. The dialectician needs no more than this [I. *Post. An.* 1. 38. n.258].

Aquinas continues by saying that 'in demonstration one is not satisfied with the probability of the proposition'. Probability requires probity and approbation but for demonstration we must be able to see and show what is what. The primary sense of the word

*probabilitas* is not evidential support but support from respected people. Byrne has nicely summed up the elements of this concept:

Attribution of probability to opinion has various connotations. In the first place, it refers to the authority of those who accept the given opinion; and from this point of view 'probability' suggests *approbation* with regard to the proposition accepted and *probity* with regard to the authorities who accept it. In the second place, 'probability' refers to the arguments which are presented in favor of the opinion in question; and from this point of view it suggests *provability*, that is, capacity for being proven (though not necessarily demonstrated). In the third place, 'probability' takes on a somewhat perjorative connotation precisely insofar as the proposition in question is *merely* probable; for, from this point of view the proposition is only *probationary* and not strictly demonstrated as are propositions which are properly scientific [Byrne 1968, p. 188].

See how this sense of 'probability' survived into eighteenth century English. We have read Gibbon saying that something probable is false. In other words, an opinion commended by authorities is in fact wrong. He said Livy had more of probability but Polybius had more of truth. This meant that ancient and modern critics tend to weigh in on Livy's side, but in this case they are mistaken. When so understood Gibbon's usage is quite free from paradox. The usage 'probable doctor' and the like is restricted to just those sorts of professions where the layman must largely rely on the judgement of others. We can now get the savour of Defoe's *Fortunate Mistress*. Her agreeable town house 'is very probable indeed' – this means not that she approves it but rather that, in the esteem of her betters, this is a good leg up from her scruffy beginnings.

'Probability' chiefly meant the approvability of an opinion. This had a number of important consequences. One was the casuistical doctrine of probabilism which is the butt of the sixth of Pascal's *Provincial Letters* [10 April 1656]. Pascal is called the founder of modern probability theory. He earns this title not only for the familiar correspondence with Fermat on games of chance, but also for his conception of decision theory, and because he was an instrument in the demolition of probabilism, a doctrine which would have precluded rational probability theory. We must briefly discuss probabilism here, but the conception of probability as approval of opinion had a more important consequence. The Renaissance physicists were still dedicated to knowledge and demonstrative science. Hence we shall not find in their work any need for or serious use of probability concepts. The prehistory of

epistemological concepts lies in a less well known area, the purveyors of opinion. In particular, medical science had no hope of being demonstrative; nor even had the 'natural magic' which is the precursor of chemistry. It is in the probable signs of the physicians and the alchemists that we shall find the evolving concepts that make our kind of probability possible.

First a few superficial words on probabilism. It is a principle of casuistry advanced by the Jesuit order in the sixteenth century, and enjoying success, power, but great antagonism and finally defeat in the seventeenth. What is to be done when authorities, especially the Fathers of the Church, are found to disagree? The problem became pressing in the late Renaissance as more and more texts were discovered and more and more interpretations of existing texts were invented. Basically there are two possibilities. To resolve conflict, we can cut down on the authorities whom we will recognize, sticking only to scripture and the natural light of reason. Or we may consider a wide range of authorities, but in deciding among them, consider the social and moral effects of adopting their several doctrines. Roughly speaking the various protesting sects, including the Jansenists (who remained within the Church), took the former course, while the casuists took the latter.

Contrary to what is sometimes reported, probabilism in theology did not say that when authorities conflict, one should follow the most probable opinion. Probabilism says that one may follow some probable opinion or other, even a less probable opinion. The word 'probable' here does not mean well supported by evidence. It means supported by testimony and the writ of authority. When a doctrine is disputed, and you are in doubt as to how to act, you may, according to the probabilists, follow a course of action that is recommended by some authority, even when more or weightier authorities counsel the opposite course of action. But even that is only the half of probabilism. It tells what is permitted, but the Jesuits were not permissive. On the contrary, from the point of view of the Jansenists, the probabilists would first of all decide on a course of action for its social and moral expediency. Then they would find some old text that could be interpreted as approval of that course of action. Then, even if weighty authority tells one to do the very opposite, one may still proceed, for one is using a 'probable' opinion, namely an opinion that is authorized by someone or other.

The Jansenist enclave at Port Royal included among its members Antoine Arnauld (1612–94), Pierre Nicole (1625–95) and Blaise Pascal (1623–62), who loom large in our history of probability. Arnauld, perhaps the most brilliant theologian of his time, was condemned by the Jesuits. The rivalry was old: in 1640 he had printed a scathing little note on probabilism. After much politicking, his enemies had him denounced, although the denunciation was withdrawn in 1669. It had the effect of spurring Pascal to write *Provincial Letters* by way of defence, or rather by way of attack. The sixth letter is a reasoned repudiation of probabilism. A few rude passages in the *Pensées* show how intensely Pascal detested that casuist doctrine. Arnauld was reinstated by a council of 1669; at no time had he lost the respect of those of his contemporaries whom we most remember. He had participated in writing the 1662 Port Royal *Logic*, which both contains an argument against probabilism and is the first occasion on which 'probability' is actually used in what is identifiably our modern sense, susceptible of numerical measurement. Throughout the rest of the century post-Jansenist writers about probability occasionally took pains to say that they did not have in mind the loathsome casuistical concept that bore the name of probability. It is not to be inferred that the rise of probabilism had nothing to do with the emergence of probability. Probabilism is a token of the loss of certainty that characterizes the Renaissance, and of the readiness, indeed eagerness, of various powers to find a substitute for the older canons of knowledge. Indeed the word 'probabilism' has been used as a name for the doctrine that certainty is impossible, so that probabilities must be relied on. I have written above only of probabilism in theology, which is the doctrine that so exercised our founders of probability. Such probabilism is still in that medieval world where probability is an attribute of opinion, and where probable opinion is that which is attested by authority. It is not post-1660 probability at all, and, aside from political and theological overtones, that is why the discoverers of the new probability despised it so much.

I have said that we shall not find students of the physical sciences making much use of anything they call probability, because they are after knowledge, not opinion. Let us take for example Francis Bacon (1561–1626) and Galileo Galilei (1564–1642). The former was taken to be the philosopher of the new physics, and Galileo its greatest practitioner. Now Galileo had, as we shall see, a good sense

of games of chance and was perhaps the first worker really to tackle the problem of how to make the best use of discrepant measurements of the same quantity. Had anyone seen that gaming and the theory of errors would merge with the old notion of 'probability', it should have been Galileo. But although the word *probabilità* occurs frequently enough in, say, the marvellous *Dialogue Concerning the Two Chief World Systems* [1632] it mostly has the old connotations. Indeed at one point Stillman Drake, the editor and translator into English, has to intervene with a footnote, "'Not improbable" here means "not implausible, though incorrect".' Elsewhere Galileo called the opinion of Copernicus 'improbable' because of the plentiful experiences which overtly contradict the annual movement, and because of the strength in debate of the Ptolemaics and Aristotelians. 'There is no limit to my astonishment when I reflect that Aristarchus and Copernicus were able to make reason so conquer sense that, in defiance of the latter, the former became the mistress of their belief.' That is, Copernicus' opinion was improbable and still the one best supported by the deepest arguments. Here we may contrast Leibniz, writing less than a century later, taking this very same situation as one in which, despite all opinion to the contrary, the Copernican hypothesis was, at the time it was promulgated, 'incomparably the most probable'. For Leibniz probability is what is determined by evidence and reason; for Galileo, probability has to do with approval.

There do remain, however, excellent passages in which Galileo makes plain that approval ought to correspond to the evidence, not the weight of the authorities. For example, Sagredo asserts that the velocity of a body rolling down an inclined plane is a function of only the height of the plane. Salviati replies 'What you say seems very probable, but I wish to go further and by an experiment so to increase the probability of it that it shall amount almost to absolute demonstration.' The *esperienza* in question is based on a pendulum whose fall is arrested at various points. Ernst Mach maintains that it led Galileo to the law of inertia [1895, p. 143]. It does not seem to me that the argument in question does get anywhere close to 'absolute demonstration', but we here have a very clear indication of the notion that experiments – at least thought experiments – can increase probability almost to demonstration. There is no attempt to measure this increase in probability, nor is there any point in measuring it. Galileo longs for absolute demonstration. So did his chief contemporaries.

As well as being the official philosopher of the new physics, Bacon is a good writer to turn to because as he says of himself in the *Novum Organum* [1620], he wants 'to banish all authorities and all sciences' – in particular dogmatic Aristotelianism and alchemical empiricism. Hence 'approval by the wise' is hardly going to be a means of appraisal, and the Latin or English word for probability will not refer to authoritative approval. But it still seems to mean 'worthy of approval', as for example in Sec. 122: 'With regard to the universal censure we have bestowed, it is quite clear to anyone who properly considers the matter, that it is both more probable and more modest than any partial one could have been.' It is no longer the wise who confer probability by their approval; it is those who properly consider the matter. If one does not consider the matter properly, things may only 'seem probable':

The empiric school produces dogmas of a more deformed and monstrous nature than the sophistic or theoretic school; not being founded in the light of common notions (which, however poor and superstitious, are yet in a manner universal, and of a general tendency), but in the confined obscurity of a few experiments. Hence this species of philosophy appears probable, and almost certain to those who are daily practiced in such experiments, and have thus corrupted their imagination, but incredible and futile to others [Sec. 64].

In short, if one does nothing but wretched experiments, opinions will appear probable which can hardly be approved by someone who has a broader stance. Note Bacon's dedication to the scholastic conception of knowledge. Knowledge is derived from common notions and states only universal truths.

Our course and method however (as we have said, and again repeat) are such as not to deduce effects from effects, nor experiments from experiments, (as the empirics do) but in our capacity as legitimate interpreters of nature, to deduce causes and axioms from effects and experiments; and new effects and experiments from those causes and axioms [Sec. 117].

The Baconian doctrine is not unlike what has come to be called the hypothetico-deductive method in science, except that there is that residue of the Middle Ages that later generations found pernicious: we seek true axioms and real notions that will ultimately produce knowledge and not opinion. There is little room in this conceptual scheme for a working concept of probability. Readers of Bacon or Galileo in the latter half of the seventeenth century found them the great originators of the new experimental method combined with a

successful mechanical model of the universe. It has only quite recently been recognized that this interpretation is an artefact of the period after 1650, particularly among members of the Royal Society of London. If we examine the texts of Bacon or Galileo we find a world of first causes. There is no need here for a mathematical concept of probability, nor even a real use for qualitative probabilities. It is not to the 'high sciences' of astronomy, geometry, and mechanics that we must look. Instead it is those lowly empirics who had to dabble with opinion.

Opinion is the companion of probability within the medieval epistemology. There is another concept of equal importance to those empirics who had to work with opinion. This is the *sign*. Inevitably Shakespeare records it: 'The least of all these signs were probable' [Henry VI. 2. 78]. Leibniz, in running over the prehistory of probability, is chiefly attentive to the law – see Chapter 7 below – but recalls how 'the physicians have the various signs and indications which are in use among them'. [P.S. v, p. 447] The history of the concept of a sign is of fundamental importance. In the medical textbooks of the Renaissance there is a characteristic distinction between cause and sign. The causes are chiefly efficient causes, namely what make the person ill, and the signs are not so much what we might call symptoms, as anything by which we may make a prognosis. To take an example almost at random, H. von Braunsweig in 1574 is saying that 'When a man hath a great disease or feebleness and a cold sweat breaketh out only about the nose, that is a very deadly sign.' That sounds familiar enough, but we will also find something else. Here I quote from Fracastoro (1483–1553) on contagion, to whom is often attributed the first germ theory of disease:

Contagions have their own peculiar signs of which some announce beforehand contagions to come, while others indicate that they are already present. The signs that are called premonitory come from the sky or air or from the vicinity of the soil or water, and among these some are almost always, others are often, to be trusted. Therefore one ought not to consider them all as prognostications, but only as *signs of probability* [1546, Bk. I, Ch. xiii].

The signs in question are a heterogenous collection: the planets in conjunction, frequent comets, tempest flares from unctuous foams, and mildew on drying linen when the wind blows from the East.

Swarms of locusts intrigue the author, and once he bursts into verse: 'Often a tiny mouse shall give thee augury of ill. No tie of love can hold it beneath the depths of the earth but it breaks forth from its trenches, forgets its life and its habits and leaves its tender young and pleasant abode . . . .' The swarms of mice that occasionally overran some of the towns of Central Europe, thousands dying frothing in the streets, were indeed a probable sign of plague to come. However, it is of no moment which signs seem sensible to us, and which absurd. Here we have a very clearly stated conception of partial prognostication, which is thereby possessed of probability, rather than certainty, and whose probability arises from frequency, from what happens 'almost always' or else 'often'.

It is important that Fracastoro is not a mere empiric of the sort castigated by Bacon. One of the fundamental features of the new science of the seventeenth century was the distinction into primary and secondary qualities. Philosophers know this through Locke, and so miss the point of the distinction. The problem was to make a science – in the Scholastic sense of the term – out of alchemy. The solution, made permanent by Robert Boyle, was to insist that the phenomena of chemistry were to be explained by noumenal things in themselves, little bouncing particles, moving, but not coloured, collectively taking up space but not in themselves having taste. It was for a long time an excellent model. It was hardly new with Bacon or Boyle. As Fracastoro put it, the qualities 'that are called primary generate and alter everything, but those that are called secondary, namely light, smell, taste and sound, do not act on one another but merely serve to arouse the senses' [Ch. vi]. Thus in the domain of causation we will have a set of universal propositions involving primary qualities only. Knowledge of this is knowledge of how the world works; it is science. However, at the level of phenomena there is something else. When the patient comes to Fracastoro, he is blotched, stinks, complains of a foul taste in his mouth and sounds strange when thumped on the back; above all he complains of pain. The causes of all this lie inside the patient and are ultimately to do with atoms. But the signs are all secondary qualities, and in these signs we have to make merely probable prognoses. The real world is described by universal truths, but the Renaissance physician has to prescribe and predict from the phenomena. Our Galileo or Bacon could pursue the real world constantly seeking demonstration, but our Fracastoro must make

prognoses on the basis of what phenomena follow what with greatest frequency.

The connection between sign and probability is Aristotelian. 'Sign', however, had a life of its own in the Renaissance, to our eyes a bizarre and alien life, but a life that we must understand if we are to comprehend the emergence of probability. The old probability, as we have seen, is an attribute of opinion. Opinions are probable when they are approved by authority, when they are testified to, supported by ancient books. But in Fracastoro and other Renaissance authors we read of signs that have probability. These signs are the signs of nature, not of the written word. Yet we shall see, in the next chapter, that this antithesis is wrong. Nature is the written word, the writ of the Author of Nature. Signs have probability because they come from this ultimate authority. It is from this concept of sign that is created the raw material for the mutation that I call the emergence of probability.

## 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 stench 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.

To understand the new kind of evidence delineated in the preceding chapter we must not look at the physicists competing for demonstrative knowledge but at the purveyors of opinion whom I have called the low scientists. The early empirics whom Francis Bacon so denigrated were chiefly alchemists, astrologers, miners and physicians. Every man endowed with lively curiosity pursued every trade, so there is no sharp division into high and low. Cardano, the author of the first book on probability, was famed both for his skill in medicine and his talent at mathematics, but for all the breadth of his interests he can safely be called a student of the low sciences. Copernicus, well versed in medical lore, was a high scientist. However we may quarrel about individuals we can often allot a given piece of work to one category or the other.

Herbert Butterfield has rightly warned that scholars who try to theorize about alchemy 'become tinctured with the kind of lunacy they set out to describe [1957, p. 129]. If we could study the high science of the Renaissance – Copernicus, say – we might stay quite sane. But probability emerges from low science. In recounting the work of the empirics it is of no value sedately to say that they combine science and occultism, and then leave out the 'occult'. We must instead try to absorb an alien conceptual scheme. We must try to comprehend a science,

in which there are two kinds of operation, one produced by nature itself, in which there is a selected man through which nature works and transmits her influence for good or evil, and one in which she works through other things, as in pictures, stones, herbs, words, or when she makes comets, similitudes, halos and other unnatural products of the heavens.

These are the words of Paracelsus [*Werke*, XII, p. 460]. In his own time (1493–1541) he was called 'the Luther of the Physicians'. In the next era John Donne was to describe him in verse as a greater

revolutionary than Copernicus. Yet in the mind of Paracelsus that strange array at the end of my quotation – *bilder, stein, kreuter, wörter* [ . . . ] *cometen, similitudines, halones und ander des gestirns unnatürliche generationes* were all what modern philosophy calls a 'natural kind', namely a collection between which there are manifest family resemblances. The resemblances between words and stones, herbs and comets, are now lost to us, yet it is the conceptual scheme engendering such resemblances that we must try to penetrate. These are not the idle groupings of a man not given to distinctions: 'The physician must know that there are a hundred, indeed more than a thousand, kinds of stomach', says Paracelsus with contempt of those who have a single panacea for all stomach ache [vi, p. 153]. Nor is he an uncritical reciter of tales:

I do not compile my textbooks from excerpts of Hippocrates or Galen. In ceaseless toil I create them anew, founding them upon experience. If I want to prove anything I do so not by quoting authorities but by experiment and reasoning [Sudhoff 1894, I, p. 29].

Paracelsus is a convenient focus for our study of signs. His biographers portray him as a strange figure, a trifle more bizarre than many another of the hermetical wandering physicians who could serve as a model for Faustus. Modern histories of medicine acknowledge him as the man who brought chemistry into medicine, treating patients not only with herbs and seeds but also with distillates and precipitates. It is remembered that he challenged Galen's theories of antipathy, treating diseases by similar substances rather than by opposed ones. His new theory of the elements – mercury, salt, and sulphur – was a great spur to chemistry. But otherwise, in standard histories, his place is incomparably smaller than his fame in, say 1600. He is a figure of the age who was revered as a great man by several succeeding generations and then almost forgotten.

The high sciences of the Renaissance have received much scholarship, but only now is low science being studied. There is some debate as to its role in the formation of European thought. Here our concern is not with the general issue but only with the notion of sign. Its structure begins with a truism which my last quotation from Paracelsus will have recalled: in the early Renaissance, books were too much revered. There is undoubtedly more to the veneration of ancient manuscripts than mere respect for a newly discovered classical culture, but that is not our topic. Rather we are concerned with the transformation from the study of books to the study of

nature. Notice in passing how perfectly the constant copying and commenting ties in with the probability of *opinio*. 'Probable' meant 'approved by the wise'. If we follow the exhortation to write down only what is probable or demonstrated, then, in that old sense of 'probable', it is an analytic truth that we should recopy the words of others. He who strives after probable opinion can, of necessity, be only a copyist and a commentator.

Paracelsus and a thousand other voices came to protest the vain repetition of Galen, Avicenna and the like. But they did not say, let us abandon this external evidence and proceed to internal evidence. They did not say, give up copying and look at the facts. Rather they said, stop studying bad books and start studying good ones. 'How can the unlearned man be led out of ignorance to science?' – 'Not through your books, but God's.' – 'Which are they?' – 'Those which he wrote with his own fingers.' – 'Where are they to be found?' – 'Everywhere.' Nicholas of Cusa (1401–64) wrote that in his dialogue *Idiota* [1967, p. 217]. Long before the birth of Paracelsus the radicals were rejecting the commentaries. The greatest rejection was that of Martin Luther. But Luther did not invite us to give up book-learning. He inveighed against vain testimony, and told us to get back to The Book, to the real testimony.

The Renaissance did indeed struggle to liberate itself from the written word and take up the study of nature by experiment. But the revolutionaries saw themselves as returning to the words that really have been written. Here is Paracelsus:

The first and highest book of medicine is called *Sapientia*. Without this book no one will achieve anything fruitful [ . . . ] for this book is God himself [ . . . ] The second book of medicine is the firmament [ . . . ] for it is possible to write down all medicine in the letters of one book [ . . . ] and the firmament is such a book containing all virtues and all propositions [ . . . ] the stars in heaven must be taken together in order that we may read the sentence in the firmament. It is like a letter that has been sent to us from a hundred miles off, and in which the writer's mind speaks to us [xi, 171–6].

Many readers will suppose that Paracelsus speaks metaphorically of books, sentences, letters, alphabet and reading. He does indeed speak of 'reading the urine', it will be protested, but so does last week's brochure giving instructions on how to use pregnancy-testing equipment. The brochure speaks metaphorically. Why not Paracelsus too? The answer is twofold. First, because he himself makes no distinction. Second, and more important, because the

literal sense of his words is essential to the sense of his system. To see that we must go a little deeper into his scheme of things.

It is well known how Galen ran medicine on the principle of the mean. Afflictions must be treated by contraries. Hot diseases deserve cold medicine and moist illnesses want drying agents. Treat excess of  $y$  by something deficient in  $y$  and thereby restore the balance. Paracelsus rebelled; he said that we must treat by similarity and not by difference. To cure a large dose of poison treat with a tiny dose of the same poison. To cure the liver treat with a herb that is shaped like a liver. He liked to quote Hippocrates' claim that experiment is futile. Quite so he said, in Hippocrates' time, 'but now we have a theory!' Since we know what sorts of medicines will be good for what sorts of ailments, we are able to begin to experiment with precisely measured doses.

Any theory that treats disease by similarity will require a theory of similarity. Paracelsus has that. It is the *doctrine of signatures*. Each thing has a signature and the physician must master the signatures. Signatures are ultimately derived from the sentences in the stars, but a bountiful God has made them legible on earth. Everything is written. Nature

indicates the age of a stag by the ends of his antlers and it indicates the influence of the stars by their names. Thus she made liverwort and kidneywort with leaves in the shape of the parts she can cure [...] Do not the leaves of the thistle prick like needles? Thanks to this sign the art of magic discovered that there is no better herb against internal prickling [XIII, pp. 376-7].

In our conceptual scheme the names of the stars are arbitrary and the points on the antler are not. For Paracelsus both are signs and there are true, real, names of things. He often rants against his contemporaries and the ancients who called things by their wrong names, having forgotten, perhaps in Babel or at The Fall, what the names really are. For example, Paracelsus knew that the metal mercury, in the correct dosage, would cure syphilis, and he thereby established medical practice for three centuries. He knew this despite the fact that his colleagues were killing their patients by randomly treating syphilitics, among others, with mercury. Syphilis is signed by the market place where it is caught; the planet Mercury has signed the market place; the metal mercury, which bears the same name, is therefore the cure for syphilis.

The sign was a matter of reading the True Book. Bizarre hermetics like Paracelsus tell us so, but we do not need to consult them exclusively. It is a relief to get back to sober instruction, for example as furnished by Georgius Agricola in *De re metallica* [1556]. His method of reading the signs on the surface of the earth will (we feel) surely help the miner and the entrepreneur for whom the book is written. We cheer his cautious criticism of the alleged merits of divining ore from twitches of hazel sticks. This is a man who understood evidence. He is one of us, it seems, and Paracelsus seems quite alien. Yet when we look again we find that Agricola too is telling us how to read aright, and how to find the Sentences on the earth's surface that say what minerals are down below. We must accept that Agricola (born 1490) and Paracelsus (born 1493) use the same concepts although they have different styles. Nor is this a phenomenon of the 'German renaissance'. In Padua, the intellectual capital of the world, we found Fracastoro (born 1483) telling us that 'the earth itself shall give thee signs', 'as though she knew what is to come, as she quakes and sighs issue from her entrails'. When the world gave a sign of  $p$ , it attested to  $p$ . Hence in the old sense of 'probable',  $p$  was probable. The proposition  $p$  was not probable in our sense of the word, as having much evidence of experience in its favour. It was probable in the old sense of the word, as being *testified* to by sound authority.

When, however, are signs to be trusted? For although a reading of the book of the universe, if complete, would always be trustworthy, we have not yet managed to read the one great sentence that is writ upon the firmament, and must rely on the microcosm around us. Not all signs are equally trustworthy. As Fracastoro put it, 'Some signs are almost always, others are often to be trusted', and these are 'signs with probability'. It is here that we find the old notion of probability as testimony conjoined with that of frequency. It is here that stable and law-like regularities become both observable and worthy of observation. They are a part of the technique of reading the true world.

In Chapter 2 I emphasized the duality of the probability that emerged around 1660. On the one hand it is epistemological, having to do with support by evidence. On the other hand it is statistical, having to do with stable frequencies. Any theory on the emergence of probability must try to explain why the concept that emerged was dual in just this way. The old medieval probability was

a matter of opinion. An opinion was probable if it was approved by ancient authority, or at least was well testified to. This medieval concept of probability is indeed related to our own, but in a surprising way. A new kind of testimony was accepted: the testimony of nature which, like any authority, was to be read. Nature now could confer evidence, not, it seemed, in some new way but in the old way of reading and authority. A proposition was now probable, as we should say, if there was evidence for it, but in those days it was probable because it was testified to by the best authority. Thus: to call something probable was still to invite the recitation of authority. But: since the authority was founded on natural signs, it was usually of a sort that was only 'often to be trusted'. Probability was communicated by what we should now call law-like regularities and frequencies. Thus the connection of probability, namely testimony, with stable law-like frequencies is a result of the way in which the new concept of internal evidence came into being.

The concept of sign as evidence, with its attendant implications of testimony, reading, and probability became the standard in all walks of life. Perhaps it is possible to see this as part of the intellectual back-sliding and obscurantism that is sometimes attributed to Renaissance thought. Some historians tell us that the high middle ages were full of 'good science' that gradually ran downhill in the fifteenth and sixteenth centuries. I think that the truth in their assertions is that *scientia* ultimately modelled on Aristotelian canons is collapsing and *opinio* is still formulating its own kind of evidence. Be that as it may, I do not claim that the concept of sign-as-evidence is a 'progressive' notion. We note only that it occurs in more and more of the sentences, written down in those days, and which have been preserved.

The sign-as-evidence indicates with probability but I do not claim that the authors who employed it were an 'influence' on the founding fathers of probability. Some historians of ideas are much concerned with the way in which work of *A* can influence his successor *B*. Two kinds of influence are considered. *B* may deliberately choose to employ central concepts or techniques of *A*, or else *B* may unwittingly pursue a programme initiated by *A*. Such talk of 'influence' is part of the historians' language of precursors and anticipations. It would be amazing if a Paracelsus were an 'influence' on a Pascal or a Leibniz. The mathematicians despised what they knew of the occult. Yet their contempt for those earlier

hermetical figures does not preclude the possibility that whenever these geometers thought about opinion, they thought in a conceptual space that was the legacy of the very empirics whom they scorned. The intellectual objects about which, and *in* which, the new mathematicians thought had been formed in the crucibles of the alchemists and the vials of the physicians.

To prove this we must ourselves stop speculating about preconditions and briefly examine a few actual precursors. We must illustrate how the generation or so preceding 1660 wrote about non-demonstrative evidence. We shall show that the arcane events I have described, taking Paracelsus as model, have become encoded as the commonsense of the time. Sign-as-evidence has become a fixed point on the intellectual scene, not a matter for debate or reflection. I argue this not by 'interpreting' the words of early seventeenth century thought, but by quotation of the actual sentences that had become current.

First, the metaphor of the 'Author of the Universe' became endemic. Even Galileo could find it convenient to talk that way, and the lesser lights of the time did so everywhere. Here is Galileo, deliberately popularizing and using the commonsense of the time to argue for a more mathematical approach to physics:

Philosophy is written in this grand book, the universe, which stands continually open to our gaze. But the book cannot be understood unless one first learns to comprehend the language and read the letters in which it is composed. It is written in the language of mathematics [1957, p. 237].

That sort of talk was everywhere. Mention of signs and probability was not quite so universal, and so we can observe, in particular cases, the groping for a conceptualization which was achieved only around 1660. Here let us take as examples only the three great philosophers of the time, Descartes, Gassendi and Hobbes. The first has no truck with the nascent concept of probability, but the other two seek it out.

Probability had no place in the schematism of Descartes. Although he had grave qualms about *scientia*, he still sought to demonstrate not only the laws of motion of the planets and the laws of refraction of light, but also that the blood must be red. Descartes did employ what many a modern philosopher calls induction: he argued from observed effects to hypothetical causes. But he insisted that even though no scholastic would call that 'demonstration', it

still was, in common speech, called demonstration. He did think that the hypotheses he demonstrated were mere fictions. Historians usually say this is because he was afraid of the sort of persecution that fell on Galileo, but there are deeper reasons. That was the only way in which he could fit the new hypotheses into the old theory of demonstration. In the waning distinction between high and low science, Descartes firmly opted for the high, and thereby determined the course of his philosophy. It had no room for probability.

As a fulcrum between Gassendi and Descartes we may usefully consider Herbert of Cherbury. His book *De veritate* was published in France in 1624. Descartes liked it and said so to his friends. Gassendi wrote a tract against it. After a theory of knowledge Herbert presents, in successive chapters, a sliding scale: truth, revelation, verisimilitude, possibility and falsehood. The 'verisimilitude' is entirely based on testimony. Herbert had only the old theory of *opinio* derived from witnesses or authority. That was fine for Descartes who thought that demonstrative science was possible. It was anathema to Gassendi, who could contend, *Quod nulla sit Scientia, et maxime Aristotelea*. There is no demonstrative science! This is the heading of a celebrated section in his book attacking the Aristotelians [1658, *Lib. II, Ex. vi*].

We use the expressions 'to have an opinion' and 'to know' interchangeably, as the practice of everyday speech shows, and if you look at the matter carefully, knowledge and opinion can be considered synonyms [1658, *II, vi, 6*].

Here is a nice example of the impotence of linguistic philosophy. Descartes said we commonly use 'demonstration' for inference of hypothesis from effect, so *scientia* stands inviolate. Gassendi said science and opinion are synonyms, and thereby denounced the old interpretation of demonstration. Descartes and Gassendi were both apostles of the new science, but they were pulling it in opposite ways.

Gassendi is first in the great line of empiricist philosophers that gradually came to dominate European thought. Unlike Francis Bacon (to whom this accolade is usually given) he did not try to revise the theory of *scientia* but to demolish it. He was sufficiently a scientist that he did not risk scorn by trying to work out the methodology of the empirics, but rather sought for ancient models. Much of his laborious scholarly production is directed at just this end. He did not find what he wanted until late in the 1630s. He

wrote several chapters of the *Syntagma* about 1636, in which *signa* play no serious theoretical role. After he had devoted serious study to Sextus Empiricus, 'signs' are everywhere. [Bloch 1971, p. 145]. He had learned of the stoic conception of *signa*, which are either indicative or what Gassendi translates from the Greek *hypomnestika* as 'probable'. Modern translators prefer to call the latter 'associative' or 'suggestive'. Smoke is an associative sign of fire because smoke and fire have often been observed together, so that the presence of smoke calls fire to mind. O. R. Bloch summarizes the consequences of Gassendi's use of this stoic doctrine:

It is in terms of signs that Gassendi developed his account of all kinds of scientific reasoning, accumulating astronomical and geometrical examples in order to show that it is through the use of signs that the astronomer and the mathematician become able to establish hidden truths [*Ibid.*, p. 146].

According to Gassendi, even syllogistic proof is a matter of signs, for the middle term in a syllogism is a sign. There was many a Pyrrhonist of the day who could echo Gassendi and say that there is no science. But Gassendi did not take the next step to total scepticism. On the contrary, the old demonstrations are preserved by his theory of indicative signs, and less certain knowledge is analysed by the theory of probable signs.

A great deal has, however, happened to the concept of sign when it passed from the language of the physician to the sign which is the deliberate, conscious, and understood expression of internal evidence. It is necessary, for example, to make the distinction between natural signs, and conventional ones. Paracelsus, remember, classed words with comets, halos, and statues. He thought that the (true) names of the stars are signs in exactly the way in which the points on a stag's antlers signify the animal's age. Of course it had always been realized that we can choose names at will, but wilful names were not true signs at all. The physician, chemist and astronomer must aim first and foremost at discovering the correct names of things. There is no element of convention in that. The discovery that all names are conventional thunders us into modern philosophy.

Arbitrary and conventional signs are carefully distinguished in the Port Royal *Logic*, the same book from which I took my terminology of internal and external evidence. Hobbes also very sharply distinguishes 'arbitrary' and 'natural' signs. Once natural signs have been distinguished from any sign of language, the

concept of internal evidence is also distinguished. Hobbes was also able to record, almost casually, the connection between natural signs and the frequency of their correctness. By 1640 he wrote:

This taking of signs by *experience*, is that wherein men do ordinarily think, the difference stands between man and man in *wisdom*, by which they commonly understand a man's whole ability or *power cognitive*; but this is an *error*; for the signs are but *conjectural*; and according as they have often or seldom failed, so their *assurance* is more or less; but *never full and evident*: for though a man have always seen the day and night to follow one another hitherto, yet can he not hence conclude they shall do so, or that they have done so eternally: *experience concludeth nothing universally*. If the signs hit twenty times for one missing, a man may lay a wager of twenty to one of the event; but may not conclude it for a truth [*Human Nature*, IV. 10].

Here, in a text published in 1650, probability has emerged in all but name. The new internal evidence and the witting cognizance of frequency go hand in hand in a glove that bears one word: 'sign'. The space, in which the concept of probability was to emerge, is complete.

## THE FIRST CALCULATIONS

The first faltering steps towards a European arithmetic of games of chance have been well chronicled by Øystein Ore [1953, 1960] and others. The only sixteenth-century book on the subject was not published until 1663, but throughout the sixteenth century various exercises on random phenomena occur in the commercial tracts that we now recognize as the start of European algebra. There are two fairly distinct aspects of these anticipations of probability theory: combinatorial problems, and problems about repeated gaming. The latter concern the division of spoils in an incompleting game. They form part of a large corpus of 'division problems' that arise in trade, and most of which have no aleatory basis. No one was able to solve those that were related to chance. It would be a brave interpreter of these early exercises who could assert, with confidence, that the authors had any idea that they were dealing with a new kind of subject matter. They were attacking one of a host of problems of 'fairness' that beset the new merchant class, and probability, in general, had nothing to do with these.

Combinatorial problems have a different kind of history. They become hooked up with the division problem in a clear and recognizable way only in the time of Pascal. Roughly speaking, combinatorial problems remain thoroughly in league with the alchemical magic of signs until the sign itself is liberated from that background in the seventeenth century. The great alchemist Raymond Lulle (1234–1315) is usually cited as the founder of the theory of combinations. He hoped to represent all the elements of the world by their true signs and then, by generating all possible combinations of signs, to produce true signs for all possible compounds in the universe. One would then know how to make any possible thing. The great combinatorial work follows this tradition. In a recent article Eberhard Knobloch [1971] shows how this