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Induction before Hume

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1 INTRODUCTION

The history of what philosophers and scientists have thought about induction has received surprisingly little attention. The reasons for this are not particularly obscure or problematic. Many philosophers in the analytic tradition have professed a conception of the nature of their subject which makes the history of philosophy almost completely irrelevant: the occasional remarks and discussions about historical figures are as perfunctory as those which appear in scientific textbooks, and indeed have an essentially similar function. Other traditions in philosophy may take its history more seriously, but unfortunately they are often either uninterested in or else even contemptuous of the whole subject of inductive inference. Finally, one important and highly influential school within modern philosophy of science has denied not only the rationality but even the existence of inductive reasoning. If such a view is taken seriously then a history of opinions about induction becomes not merely a potentially unprofitable but also an exceedingly problematic undertaking.

If we look back at the history of thinking about induction, two figures appear to stand out from the remainder. Francis Bacon appears, as he would have wished, as the first really systematic thinker about induction; and David Hume appears as perhaps the first and certainly the greatest of all inductive sceptics, as a philosopher who bequeathed to his successors a Problem of Induction, which might be solved, or dissolved, or by-passed, but which could not legitimately or honestly be ignored.

This conception of Hume's achievement, which can be found in the writings of so many twentieth-century philosophers, may seem fairly uncontroversial. It does however carry with it a number of interesting problems. One is that it is in fact by no means obvious that Hume intended to put forward the views which have been so frequently attributed to him

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in this century.[1] Although Hume is now quite routinely interpreted as an inductive sceptic, anyone who reads the relevant sections of the *Treatise* or the first *Enquiry* can hardly fail to notice that the *word* 'induction' is completely absent. In fact it appears only once in the *Treatise*, and once in the *Enquiry concerning the Principles of Morals*. In neither case does Hume's employment of the word conform at all closely to modern usage. In the *Enquiry* Hume is considering what he calls the general foundation of Morals: 'whether they be derived from Reason or from Sentiment; whether we attain the knowledge of them by a chain of argument and induction, or by an immediate feeling of finer internal sense' (Hume [1751], p. 170). The use of the word in the *Treatise* is even stranger. It occurs in the Appendix—a group of miscellaneous additions to Book I tacked on to Book III when it appeared in 1740. Hume has been explaining further his theory of the nature of belief, and adds, 'I conclude, by an induction which seems to me very evident, that an opinion or belief is nothing but an idea that is different from a fiction, nor in its nature, or in the order of its parts, but in the manner of its being conceived.' This use of the word 'induction' may seem strange to modern readers, but, as we shall see, it has a good historical pedigree, and there is no reason to suppose that Hume expected his readers to be either surprised or uncomprehending.

At this point someone may say that we are not or at least should not be primarily interested in this history of the *word* 'induction'. We should be concerned with the history of the *thing*. The word may have other, irrelevant, uses—it obviously does in physics, for example—but we can and should ignore these. Our concern is with what people in the past have thought about induction, whatever the terminology they have seen fit to use.

[1] Another interesting problem which lies outside the scope of this paper, concerns the history of the interpretation of Hume's arguments. It would appear that until relatively recently most philosophers either did not regard Hume as an inductive sceptic, or else did not suppose him to have made any points which needed a serious reply. The nineteenth century, especially the first half, was the great age of inductive theories of scientific method, but throughout all the controversies of that period Hume's name is almost completely absent. Whewell and Mill disagreed deeply about the nature of scientific method, but neither of them appears to have thought that Hume had anything to contribute to the subject. One philosopher who has noticed this, Laudan, has gone so far as to say that, 'it is one of the wilder travesties of our age that we have allowed the myth to develop that 19th century philosophers of science were as preoccupied with Hume as we are. As far as I can determine none of the classic figures of the 19th century methodology—neither Comte, Herschel, Whewell, Bernard, Mill, Jevons, nor Peirce—regarded Hume's arguments about induction as much more than the musings of an historian. (Laudan [1981], p. 240). In fact there were a few nineteenth century thinkers who interpreted Hume in the way that has now become familiar, for example John Venn (Venn [1889], pp. 127–8). Venn appears to have been somewhat isolated however. There is no sign of the modern interpretation in Green and Grose's edition of Hume's works [1874]. It is interesting that Keynes writing *c.* 1910, felt it necessary to remark that though Hume's sceptical criticisms are usually associated with causality, induction was the real object of his attack (Keynes [1921], p. 302). The older interpretation survived as late as Kemp Smith's magisterial *The Philosophy of David Hume* (1941). It would appear that the modern interpretation of Hume as an inductive sceptic arose as a by-product of work done on inductive logic.

Up to a certain point, at least, it is difficult to dissent from this. Plato uses *epagôgê* to mean an incantation (*Republic*, 364C, *Laws*, 933D), but one does not feel oneself to be taking any great risks in neglecting these passages when considering Greek theories of inductive logic. The same is clearly true of modern discussions of electrostatic or electromagnetic induction. Some degree of caution is however necessary. If we regard modern conceptions of induction as being in some way standard or natural, and direct our enquiries towards discovering past employments of the same or closely similar concepts, then we are in danger of producing a severely impoverished kind of history. One of the main purposes of intellectual history is to try to enter as fully as we can into the mentalities of people who thought in ways very different from our own. If philosophers in the past have used words such as 'induction', *inductio* and *epagôgê* in ways that seem odd or surprising to us (for example Boyle's use of *epagôgê* for *reductio ad absurdum* Boyle [1772], vol. IV, p. 468), we should take note of this fact and attempt to pursue its implications, and not filter it out of our consciousness by using a defective method of enquiry.

2 A HISTORICAL SURVEY

How, then, did philosophers think about induction before Hume—before, shall we say, the middle of the eighteenth century? The obvious starting point is with the thought of Aristotle. Locke memorably and caustically remarked that God had not been so sparing to men as to make them barely two-legged creatures, and left it to Aristotle to make them rational; and Aristotle was not the first man, or even the first philosopher, to make use of inductive arguments. (One can find a very early use of the methods of agreement and difference in the Old Testament, in Judges vi. 36–40.) He indeed gave credit for the introduction of inductive arguments into philosophy to Socrates (*Metaphysics*, 1078b28). It was Aristotle nevertheless who was the first philosopher both to use a specific technical term (*epagôgê*) for what we call induction, and to give an account of the nature of inductive reasoning.

Aristotle's theory of science has a place for both deduction and induction. Scientific knowledge is obtained by demonstration from undemonstrable first principles, and knowledge of these first principles is in turn obtained by induction. One might expect therefore that Aristotle would have discussed deduction and induction at something like equal length. In fact his remarks about induction are fairly brief and in many respects very obscure.

There are two main places in which Aristotle discusses the theory of inductive reasoning. The first, in *Prior Analytics* II.23, is not very illuminating. It is concerned purely with induction by complete enumeration, and provides a good example of Aristotle's intermittent but regrettable tendency to use Procrustean methods in forcing other kinds of inference into syllogistic form.

The most important other place in Aristotle's writings in which the nature of induction is discussed is *Posterior Analytics* II.19. This chapter is notoriously one of the most obscure in all Aristotle's writings, and its interpretation is far from straightforward. A considerable part of its obscurity derives from the fact that Aristotle appears to slide without explanation from an account of how we acquire universal concepts (100a3–b3) to an account of how we acquire knowledge of universal truths (100b3ff). Sir David Ross assumed that Aristotle was concerned both with concept formation and with induction, and passes from the one to the other because of a close analogy between the two (Ross [1949], p. 675). Jonathan Barnes on the other hand supposes that only concept formation is involved, and that Aristotle uses *epagôgê* 'in a weak sense, to refer to any cognitive progress from the less to the more general' (Barnes [1975], p. 256). This problem and others closely related to it have recently been the subject of much discussion among specialists in ancient philosophy (Barnes [1975], Hamlyn [1976], Engberg-Pedersen [1980], Upton [1981], Kahn [1981]). Like most really well established disputes in ancient philosophy, this one is unlikely ever to be finally and definitively resolved. All the less transient interpretations have at least something to be said for them, and we have no final assurance that Aristotle ever formulated a single coherent, or even approximately coherent theory. Further minute analysis of Aristotle's Greek text is unlikely to produce much further enlightenment, indispensable as such analysis certainly is. I would therefore wish to excuse myself from attempting any direct contribution to this debate (except to note a broad agreement with Kahn's approach). Instead it would seem to me useful to look first at the uses to which *epagôgê* was put by Aristotle, and then at the subsequent history of *epagôgê* and of non-deductive inferences generally. By doing this we can hope to gain insight, not so much into what was in Aristotle's mind when he was writing the *Posterior Analytics*, as into the problems and possible solutions characteristic of any broadly Aristotelian system of philosophy.

Aristotle uses the word *epagôgê* and its derivatives with what seems at least to us to be a large variety of senses. Sometimes the meaning seems to be *experience* or *observation* (*Physics*, 185a14; *De Caelo*, 276a14), or *example* (*Physics*, 229b3). More commonly some element of generalisation is involved, but the content of the generalisations is likely to appear strange to someone familiar only with the modern tradition of inductive logic stemming from Bacon. Sometimes we have the kind of argument familiar from the Socratic dialogues: 'If the skilled pilot is the best pilot and the skilled charioteer is the best charioteer, then in general the skilled man is the best in any particular sphere' (*Topics*, 105a15–17). In the majority of cases however what is established by induction has even less claim to be considered as an empirical generalisation. Among the truths which Aristotle describes as being reached by induction we have the principle that non-accidental changes occurs only between contraries, between their intermediaries and between contradictories (*Physics*, 224b30); the principle that whatever is posterior in the order of development is prior in the order of nature (*De Partibus Animalium*, 646a30); the principle that contrariety is the greatest difference (*Metaphysics*, 1055a6); and the principle that excellence is the best position, state or capacity of anything that has some employment or function (*Eudemian Ethics*, 1219a1). What we do *not* find are what we are accustomed to think of as empirical generalisations. Aristotle uses the word *epagôgê* and its derivatives over fifty times in his various writings, and the *only* example of a proposition derived by *epagôgê* which could reasonably be described as an empirical generalisation is the discussion example of all bileless animals being long-lived which appears in *Prior Analytics*, II.23. (On the background to this example, see Guthrie [1981], pp. 194–5.) It is noteworthy that in this case Aristsotle states explicitly that the induction requires a survey of *all* the particular instances.

It appears therefore that although Aristotle's formal position was that first principles of the sciences are obtained by induction, he was not an inductivist after the manner of Bacon, or Herschel, or Mill. Drawing up empirical generalisations from a wide and varied range of particular instances played little part in his scientific practice.

Aristotle's examples of inductive inferences can therefore be divided into two classes. First we have broadly common-sense arguments, usually appearing in rhetorical contexts, whose purpose is to establish some general thesis about human life and conduct. The argument about skilled pilots and charioteers in the *Topics* is an example, and there are other specimens in the *Rhetoric* (*e.g.*, 1398b5–18). These may be termed *rhetorical* inductions. Secondly there are more abstract arguments which are intended to establish some theoretical point within philosophy. These may be called *philosophical* inductions.

If we examine the rather scanty material on induction which has survived from the time of Aristotle's successors down to the end of the ancient world, a broadly similar picture emerges. Rhetorical inductions are used by Cicero, who introduced the word *inductio* as an exact equivalent for *epagôgê* (*Topica*, 42; *De Inventione*, I.51–6), and by Quintilian (*Institutio Oratoria*, V.x.73). This rhetorical tradition continues as late as Boethius, whose *De Topicis Differentiis* was a souce of Aristotelian ideas in the early Middle Ages when all knowledge of the *Posterior Analytics* and the *Topics* had been lost (*De Topicis Differentiis*, 1183D–1184D). The other, philosophical, usage can be found in Plutarch (*Moralia*, 957C) and in Plotinus, who uses the word *epagôgê* twice, once for an argument to show that there is nothing contrary to substance (*Enneads*, I.8.6.30) and once for an argument that whatever is destroyed is composite (*Enneads*, II.4.6.10).

There are other remarks about *epagôgê* elsewhere in the Platonist tradition: very briefly in Albinus (*Didaskalikos*, 158.1) and at considerably more length, in the Middle Platonist source incorporated by Diogenes Laertius in his life of Plato. Here three types of *epagôgê* are distinguished

(III.53–5). One, called *epagôgê kat'enantiosin* is a curious (and in the example given, grossly fallacious) kind of *reductio ad absurdum* (this may be the source of Boyle's curious use of the word *epagôgê*, mentioned earlier). The other, *epagôgê ek tês akolouthias*, has two varieties. One is inference from particulars to other particulars, for example from bloodstains to murder. The other is inference from particulars to universals. Here too the universal proposition given as an example is not one which we would naturally think of as an inductive generalisation, being the principle that opposites come from opposites.

The word *epagôgê* occurs in some of the fragments of Epicurus' *On Nature*, but not, apparently, with the technical Aristotelian meaning (Sedley [1973], p. 66). The Epicureans were however some of the strongest ancient advocates of the use of induction, their preferred name being inference from similarity (*metabasis kath' homoiotêta*). Our best source for these views is the treatise *On Signs*, written in the first century BC by the Epicurean Philodemus and preserved among the Herculaneum papyri.

Philodemus' treatise, so far as we can judge from its surviving parts, was a defence of inductive and analogical inferences against various objections. The source of these objections is not clearly identified in the parts of the work which we possess, but most modern scholars attribute them to the Stoics. The Stoics were indeed quite as hostile to induction as the Epicureans had been well-disposed. One possible explanation for this is that they rejected the whole idea of rational non-deductive inference. Burnyeat ascribes to the Stoics the view that the logic of our reasoning is always deductive (Burnyeat [1982], p. 236, *cf*. p. 231). 'The upshot is that Stoic logic guarantees to Stoic epistemology that the only warrant which one proposition can confer on another is the warrant of conclusive proof' (*ibid*., p. 235). Unfortunately the nature of the surviving evidence makes interpretation difficult and more than usually precarious. It is possible that Chrysippus was as explicit as Popper, but Chrysippus' works have all been lost, and nowhere in the surviving sources is there a clear statement of the position ascribed by Burnyeat. Moreover the Stoics' opposition to induction can be explained without supposing them to have been strict deductivists. The mere fallibility of inductive inferences would, for the Stoics, have been a powerful reason for discarding them altogether. Merely fallible inferences cannot provide us with *knowledge* of anything, for according to Stoic doctrine we only know something when we have an intellectual grasp of it which cannot be weakened by further evidence or argument. Belief or opinion, which can be so weakened, is a very inferior state of mind. Indeed the Stoic ideal, the Sage, is characterised by his refusal to hold any mere opinions; like the ideal sceptic, he lives *adoxastôs*, without beliefs of any kind.

One basic Stoic objection to inductive inferences was therefore that they are inherently insecure. We cannot survey all the individual instances, and if we survey only some we risk failing to include the kinds of exceptional case which we already know both can and do exist (*On Signs*, para. 3–4, 28).

The Epicurean reply to these arguments was that inductive inferences are trustworthy provided that we take the appropriate precautions. We should make our inferences 'from what has been tested from every side, and does not exhibit a spark or trace to the contrary' (para. 45). All men who have been beheaded, for example, die at once (para. 18). Moreover we should choose characteristics which belong to all our sample without variation: 'For example, if men are found to differ from one another in all other respects, but in this respect they have been observed to have no difference, why should we not say confidently on the basis of the men we have met with and those of whom we have historical knowledge, that all men are liable to old age and disease?' (para. 35).

The dispute between the Stoics and the Epicureans had a close parallel in the field of medicine, where the Empirical school advocated the use of inductive arguments and the Dogmatic school rejected them. Galen preserves a remarkable soritical argument against reliance on generalisations based on a multiplicity of observations. If n observations are insufficient to establish reliably the truth of a generalisation, where $n = 1$ or some other small number, then $n+1$ observations must also be insufficient. If it were the case that (say) 49 observations were not enough, whereas 50 were, then it would follow that one observation, the 50th, would in itself be sufficient, which is both implausible and contradicts the initial assumptions (Galen, *On Medical Experience*, pp. 96–7 Walzer).

This argument is interesting for many reasons (Barnes [1982]), but not least for the fact that it is not an argument which found favour with modern opponents of induction. This may in part be because *On Medical Experience* has survived only in an Arabic translation, and has therefore been effectively inaccessible prior to the publication of Walzer's translation in 1944. Another reason would be that ancient philosphers took soritical arguments far more seriously than most modern philosophers have thought it necessary to do. To most modern philosophers soritical arguments appear, at their best, to be ingenious, perhaps remarkably different to analyse properly, but at bottom fundamentally *sophistical*. Much modern discussion of scepticism is characterised by what Burnyeat has called 'insulation' (Burnyeat [1984], p. 225): sceptical doubts are not allowed to influence what we do or think outside of philosophy. One of the remarkable things about modern inductive scepticism is that sceptical doubts about induction are much less well insulated from other beliefs than sceptical doubts about time, or memory, or the external world. As a result, arguments which are felt at bottom to be merely sophistical are likely to appear more out of place in discussions of induction than they do elsewhere.

The ancient sceptics appear in fact to have had rather less to say about induction than many of their modern successors. The most substantial discussion of induction surviving from either the Academic or the Pyrrhonist tradition is to be found in ch. 15 of Book II of Sextus Empiricus' *Outlines of Pyrrhonism*:

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

On the surface this passage is fairly straightforward: Sextus appears to be making the familiar point that inductive generalisations derived from an incomplete survey of the relevant particulars are insecure—the word Sextus used, *bebaios*, means safe, steadfast, guaranteed. There is nothing here to suggest that Sextus regarded such inferences as epistemically worthless. Moreover other passages suggest that Sextus had no objection to make against inductive inferences from observables to observables, as from smoke to fire or from a scar to a wound (*PH.* II.102, *Adv. Math.* VIII.154–8). Sextus' polemic was directed against what he called indicative signs: inferences from observables to unobservables, such as Epicurean atoms or Aristotelian elements.

The real problem with understanding this passage is that we can only do so by placing it in its overall intellectual context, and that context itself is highly problematic. It is clear that Sextus assented to the use of at least some inductive inferences, but it is by no means clear what this assent amounts to. Very roughly there are two lines of interpretation: one is that Sextus is concerned merely to doubt the philosophical theories put forward by his dogmatist opponents; the other is that his doubt extended also to the assumptions and inferences made in everyday life. On the former interpretation, Sextus was only mildly sceptical about inductive arguments: they were not wholly reliable and their employment had to be restricted to the everyday world, but subject to these limitations they could legitimately be used. On the latter interpretation Sextus was a sceptic about inductive inferences, not because he found them particularly obnoxious, but because he was sceptical about all inferences without exception.

Neither of these interpretations can easily be dismissed—indeed reading the rapidly growing secondary literature on this topic can tend to induce the kind of suspension of assent which it was the main aim of Sextus' philosophy to produce. The most commonly held view until quite recently was that Sextus was a moderate who was not disposed to reject inferences from observables to observables (Stough [1969], pp. 128–39). More recently opinion has shifted in favour of Sextus as a more thoroughgoing sceptic (Burnyeat [1984], Barnes [1982a]), even though it is generally admitted that the claims that Sextus makes in various places are by no means obviously consistent with one another (for an excellent analysis of the problem, see

Barnes [1983], pp. 154–60; Stough [1984] now advocates a position close to that of Burnyeat, but she still holds (p. 155n) that Sextus is committed to a principle of induction). It does nevertheless seem reasonably clear that Sextus was not particularly hostile towards inductive arguments: on one interpretation they survive; on the other they are destroyed, but only because all arguments are destroyed.

The medieval schoolmen, who had so much to say about deductive inference, contributed relatively little to the theory of induction. Most of the logic textbooks found some space for a discussion of induction, but the remarks made were usually brief and rather perfunctory. Even Ockham's *Summa Logicae*, a treatise planned on a much larger scale than usual, and one of the most remarkable achievements of medieval logic, conforms to the same pattern. Ockham devoted six chapters of this work (III. iii.31–6) to induction, but the treatment is unilluminating; the extra space merely gave Ockham the opportunity (fatal to so many medieval authors) of being prolix and rather pedantic. Aquinas also had little of any value to say: his remarks in his Commentary on the *Posterior Analytics* are very dull. A rather more interesting discussion can be found in the longer of Duns Scotus' two commentaries on the *Sentences* of Peter Lombard, the work now known as the *Opus Oxoniense*. Scotus raised the question which was to be central to so many subsequent discussions: can we be certain about any universal conclusions reached by means of induction? His answer was that we can, provided that we make use of general propositions not dependent on induction—in this case the principle that whatever happens in many cases as a result of a cause which is not free is the natural effect of that cause (*Opus Oxoniense*, I, d.iii, q.4, Wolter [1962], pp. 109–10). Like most if not all such general principles, this one seems both highly dubious and far from obviously capable of doing the job intended for it, but it no doubt seemed satisfactory enough to Scotus, and he was certainly to have many successors in the centuries following.

The most substantial and most influential discussion of induction in the seventeenth century is to be found in Francis Bacon's *Novum Organum*. Bacon was the first philosopher to consider induction as the chief method of inference in the natural sciences, and subsequent estimates of his philosophical stature have reflected very accurately the high or low esteem in which inductive methods have been held. The main elements of Bacon's views are well known, at least in broad outline, and need not be described here, but there are two points which perhaps need to be given some degree of emphasis.

The first is that Bacon had an extremely low opinion of induction by simple enumeration. The language he uses makes his scorn absolutely clear. Induction by simple enumeration is 'utterly vicious and incompetent' (*De Dignitate et Augmentis Scientiarum*, V.2), 'gross and stupid' (*ibid.*) and 'childish' (*Novum Organum*, I.105). Bacon's fundamental objection to this kind of induction is that it can lead us only to conjectures, and not to certain knowledge. 'For the induction of which the logicians speak, which proceeds by simple enumeration, is a puerile thing; concludes at hazard; is always liable to be upset by a contradictory instance; takes into account only what is known and ordinary; and leads to no result' (*Magna Instauratio*, Distributio Operis, Bacon [1859], vol. IV, p. 25). Modern introductory books on the philosophy of science sometime commence with a criticism of 'naive inductivism' and carefully warn us of the dangers of concluding that all swans are white from the basis of a limited number of observations. Bacon regarded all such inductions as quite useless.

Secondly, Bacon's own method of exclusion presupposed a considerable body of metaphysics. One instance of this is that Bacon assumed a simple one-to-one relation between the observable natures of bodies and the forms which are their causes. Another is that he assumed a kind of Principle of Limited Variety: that the number of different forms to be found in nature is manageably finite. Both of these assumptions are necessary if Bacon's method of induction is not to be exposed to exactly the same kind of criticisms which he himself had made of the old and despised method of induction by simple enumeration.

Gassendi's remarks about induction are not always easy to interpret. In his early sceptical phase, represented by the *Exercitationes Paradoxicae Adversus Aristoteleos*, he appears to be quite dismissive of the Aristotelian theory of induction. His reasons for this are very similar to those put forward earlier by Sextus; we cannot enumerate all the particular cases since they are actually or potentially infinite in number. Gassendi's one novelty, as compared with Sextus, was to deploy a characteristically voluntarist argument based on God's infinite power. Even if there exist some individuals which are unique, such as the sun, there are nevertheless also infinitely many possible suns which God could create, and any genuinely well-grounded propositions would have to be true of these also (*Exercitationes*, II.v.5, Gassendi [1972], p. 75).

In Gassendi's *magnum opus*, the *Syntagma Philosophicum*, his views on induction appear rather ambivalent. His main discussion, taken by itself, might suggest that the only inductive arguments of any value are enthymemes—arguments which can be turned into regular syllogisms by the addition of a concealed premise or premises. Without the addition of such a premise the inference has no force, *consequutionis vis nulla foret* (*Syntagma*, I.iii.11, Gassendi [1658], vol. I, p. 113). If all the singulars are not included in the enumeration, the proof dissolves (*labefactet*). This is a requirement which Gassendi quite freely admits to be 'very difficult, or impossible' to fulfil.

This passage might suggest that Gassendi saw inductive arguments as being of very restricted value. Elsewhere in the same work however he appears to give induction a substantially more positive role. In Gassendi's opinion the usual descriptions of arguments from the more general to the particular as *a priori*, and from the particular to the general as *a posteriori*, ought really to be reversed. It is particulars which are known first, and all the evidence and certainty which a general proposition can have is dependent on an induction from particulars (*Syntagma*, I.iii.16). This is true even of such highly general propositions as 'every whole is greater than any of its parts'. Gassendi regarded this as an inductive generalisation, based on observations made from childhood onwards that a whole man is bigger than his head, a whole house larger than a single room, and so on.

Other seventeenth-century writers were less ambiguous in their attitude toward inductive inferences. The authors of the Port Royal *Logic* for example regarded all inductions based on a survey of fewer than all the relevant particular as merely sophistical:

> Induction is not at all a certain means of knowing something, except when we are sure that the induction is complete; there being nothing more common than to discover the falsity of what we had believed to be true on the basis of inductions which had seemed so general that one would never imagine that one could find an exception.
>
> Thus until two or three years ago it was believed to be quite indubitable that when water was contained in a vessel with curved sides, one end being narrower than the other, it remained completely level, being no higher in the smaller end than in the larger one. One was assured of this by an infinity of observations.
>
> Nevertheless it has been found recently that this is quite false when one of the ends is extremely narrow, for in such cases the water rises higher in this end than in the other. All this shows us that inductions alone cannot give us a full certainty of any truth—unless we were sure that the inductions were complete, which is impossible.[1]
>
> (Arnauld and Nicole [1662], pp. 316–17)

This inherent fallibility of all inductive arguments was not a source of great worry to Arnauld and Nicole, who believed that scientific knowledge was to be obtained by deduction from self-evident and indubitable axioms. Leibniz's attitude towards induction was rather more complex. In some places he expressed views very close to those of the Port Royal *Logic*. In a letter of 1702 to Queen Sophia Charlotte of Prussia he maintained that induction can never teach us truths which are fully (*tout à fait*) universal (Leibniz [1969], p. 551). Even if we have seen one hundred times that lumps of iron sink when placed in water, we still cannot be sure that this must always happen (*ibid.*). The same is true of other inductive generalisations, such as that heavy bodies will fall, or that we ourselves will eventually die. In the former case, Leibniz commented that we cannot go with complete confidence (*bien seurement*) beyond the experiences we have had, unless we are aided by reason (*ibid.*, p. 550). In the latter, the similarity which exists between men would not by itself justify us in considering the conclusion as certain (*ibid.*, p. 551).

This view that unsupported inductive inferences cannot provide grounds for certainty appears much earlier in Leibniz's writings. In his Preface to

[1] The observations of capillary attraction which Arnauld refers to are probably those of M. Thévenot, undertaken in 1658–61. Cf. *Dictionary of Scientific Biography*, vol. XIII, p. 336.

Nizolius' *De Veris Principiis* [1670] he put forward the view that induction cannot produce any kind of certainty, even moral certainty, unless one also makes use of universal propositions derived purely by reason (*ibid.*, pp. 129–30). With the aid of these universal propositions moral certainty can be attained, even though perfect certainty cannot. Mere inductive generalisations cannot be certain at all.

There are places in the *Nouveaux Essais* where Leibniz appears to have been expressing the same attitude. For example, he remarked that 'however many instances confirm a general truth, they do not suffice to establish its universal necessity' (Leibniz [1765], p. 49), and he illustrates this with one of Popper's favourite examples: that the sun does not rise every 24 hours in the polar regions. There are however other passages which appear to say something rather different. In IV.vi.8, where Leibniz is arguing against Locke's pessimistic analysis of our capacity to know universal truths about substances, he remarks that

> We know almost as certainly that the heaviest of all bodies known on earth is fixed i.e., not decomposed by heating, as that the sun will rise tomorrow. This is because it has been experienced a hundred thousand times. It is a certainty of experience and fact, even though we do not know how fixity is linked with the other qualities that this body has (pp. 404–5).

Here Leibniz's view is that observation of regularities can produce certainty. 'For it seems to me that, in the case of propositions which we have learned from experience alone and not by the analysis and connection of ideas, we rightly attain to *certainty* (moral or physical, that is) but not to necessity' (*ibid.*, p. 406). Apparently similar views can be found elsewhere. In a fragment dated by Couturat to 1693 Leibniz distinguishes three levels of epistemic security: *certitudo logica*, *certitudo physica*, and *probabilitas physica*, and he makes it clear that induction, when properly carried out, produces the second of these (Leibniz [1903], p. 232). This also appears to be the implication of a remark in the *Nouveaux Essais* in which Leibniz explains physical necessity as 'necessity founded on induction from what takes place in nature' (Leibniz [1765], p. 499). It is by no means obvious that all these passages can be reconciled with one another. On the other hand the difference between the various views expressed is perhaps not all that great. Inductive arguments cannot produce the kind of certainty characteristic of mathematical demonstrations, but they can make their conclusions probable, even highly probable. Whether these very high probabilities amount to moral certainty is something about which Leibniz appears to have had no settled opinion.

Apart from Bacon, none of Hume's British predecessors has very much to say about induction. Hobbes had little time for the kind of experimental philosophy which Bacon had advocated and which some of his younger contemporaries were trying to pursue. He mentioned induction in only a few places: twice in *The Whole Art of Rhetoric* (ii.21,24), and once in the

Examinatio et Emendatio Mathematicae Hodiernae (Hobbes [1839], vol. IV, p. 179). The *Rhetoric* is a work conceived on very traditional lines, and induction appears to be included primarily because it is one of the traditional types of rhetorical argument. The remark in the *Examinatio* is slightly more informative. Hobbes's target here was John Wallis's *De Arithmetica Infinitorum*, and Hobbes objected strongly to Wallis's use of induction in arguing towards theorems about infinite series and infinite continued fractions. Induction, says Hobbes, is not demonstration '*nisi ubi particularia omnia enumerantur*'. Wallis, it may be noted, himself had exactly the same opinion of the limitations of incomplete induction: it can arrive at a conclusion which '*conjecturalis tantum est, aut probabilis, non omnino certa*' (Wallis [1687], p. 170).

Locke had slightly more to say. His attitude towards induction emerges clearly in what is to the best of my knowledge the only place in all his writings in which the word appears, section xiii of *The Conduct of the Understanding*. Locke remarked that 'those seem to do best, who taking material and useful hints, sometimes from single matters of fact, carry them in their minds, to be judged of, by what they shall find in history, to confirm or reverse these imperfect observations: which may be established into rules fit to be relied on, when they are justified by a sufficient and wary induction of particulars' (Locke [1823], vol. III, p. 214). Locke was not in any way opposed to the use of non-demonstrative arguments, whether inductive or analogical, but he insisted that the conclusions of such arguments cannot be knowledge. 'Possibly inquisitive and observing men may, by strength of *judgement*, penetrate farther, and on probabilities taken from wary observation, and hints well laid together, often guess right what experience has not yet discovered to them. But this is guessing still; it amounts only to opinion, and has not that certainty which is requisite to knowledge' (*Essay*, IV.vi.13).

This sharp distinction between knowledge and opinion or belief, and the insistence that knowledge, in order to be knowledge, must be certain, can be found repeated in many of Locke's other works. When he was forced to defend his position, it was not against anyone who supposed that knowledge could be uncertain, but against theologians like Stillingfleet and Jonas Proast who held that we could be certain about matters of religious faith (Locke [1823], vol. IV, pp. 143–7, 271–99; vol. VI, p. 558).

3 THE VARIETIES OF INDUCTIVE SCEPTICISM

From this rather brief survey one thing at least is clear. In the centuries before Hume, and especially during the 120 years which separated the *Novum Organum* and the *Treatise of Human Nature*, very few philosophers had been entirely unaffected by doubts about the reliability of inductive inferences. Moreover the doubts felt were of very different kinds: a mere division into sceptics and anti-sceptics would be far too crude to be of any real use. What we need is a more elaborate classification of the different levels of unease about inductive reasoning which have felt:

1. There are reservations about inductive reasoning which arise merely because inductive arguments are not deductively valid.

2. There is the view that inductive arguments are inherently and irredeemably fallible: although such arguments may make their conclusions probable, they can never make them certain .

3. There is the view that genuinely universal propositions can never be given a probability greater than zero by any inductive argument.

4. Finally, there is the view that no inductive arguments, whether to particular or to general conclusions, can be given any rational foundation whatever.

These views, which constitute successive and increasingly radical stages of doubt about induction, need to be kept clearly separate if confusion is to be avoided.[1]

Despite a few rather disingenuous claims to the contrary (*e.g.*, Hume [1745], pp. 19, 22), Hume clearly holds position 4, which may be termed radical inductive scepticism (Hume [1739], pp. 267–8; [1748], p. 41).

The third position, that all universal generalisations have a probability of zero given any finite quantity of evidence, has been the subject of much discussion among modern philosophers concerned with inductive logic and probability theory. It would seem however to be a view which would only be likely to appear after the development of a mathematical calculus of probabilities. I am not aware of anyone who held it in the period before Hume.

In the seventeenth century and earlier, the most commonly held position was the second—that inductive arguments are inherently fallible and produce (at best) only probability and not certainty. This was a more radical conclusion than it might seem at first sight to someone familiar primarily with twentieth-century discussions of this topic. For nearly all philosophers in the period before Hume knowledge meant certain knowledge. This was

[1] Popper does not always appear to do this. For example, when he describes Hume as having produced 'a gem of priceless value for the theory of objective knowledge: a simple, straightforward, logical refutation of any claim that induction could be a valid argument, or a justifiable way of reasoning' (Popper [1972], p. 86) there appears to be a slide from position 1 to position 4. In other places Popper sometimes associates Hume with position 1 (Popper [1959], p. 312) and sometimes with position 4 (Popper [1963], p. 200). The adoption of radical inductive scepticism (position 4) has exceedingly disturbing consequences, which Hume saw more clearly than Popper has. If all arguments other than purely deductive ones have no rational foundation, then Hume's and Popper's arguments against induction must be purely deductive. Nevertheless it is quite apparent that the kind of controversy which they have provoked is quite unlike anything which indisputably deductive arguments produce, even when the conclusions of those arguments are profoundly surprising or subversive. The proofs of Gödel's incompleteness theorems are much longer and apparently more complex than Hume's arguments against induction, but they never became a subject of controversy. If both arguments are purely deductive, why should this be?

a view shared by epistemological optimists like Bacon and Descartes, and by sceptics like Foucher and Bayle. Hume himself held that 'knowledge and probability are of such contrary and disagreeing natures, that they cannot well run insensibly into one another' (Hume [1739], p. 181). That the same conception of the nature of knowledge should be shared both by thinkers as confident in the power of the human mind to acquire knowledge as Bacon and Descartes were and by sceptics like Bayle and Hume is in reality not at all strange. In the Ancient world one can find many of the same premises shared by the Stoics and their Academic and Pyrrhonist opponents. Both parties had an identical though opposite interest in insisting that what can count as knowledge must satisfy the most stringent criteria. What is more remarkable is that the same view can be found in more cautious and less confident thinkers who nevertheless had no wish to be included among the sceptics. Locke's views have already been described. John Wilkins in his classification of the kinds of assent which we can give to propositions, placed them all under two main headings 'knowledge or certainty' and 'opinion or probability' (Wilkins [1675], p. 5). Similar views can be found in many of the other philosophers and scientists connected with the early Royal Society (Hooke [1705], p. 330; Glanvill [1676], p. 45).

The idea that there could be knowledge which was uncertain or merely probable is much more difficult to locate. Gassendi once remarked in passing that just as one can speak of certain knowledge and certain opinion, so one can speak of fallible (*imbecillam*) knowledge and fallible opinion (Gassendi [1658], vol. III, p. 206b). The suggestion was not developed, however, and it would be imprudent to try to draw from it views which Gassendi would probably have repudiated. Leibniz held that we can have knowledge of probabilities, but by this he meant that we can know the truth of probability statements (Leibniz [1765], p. 373). There is no suggestion that we can have knowledge when we are uncertain of the truth of the propositions themselves.

The main way in which the requirement that knowledge must be certain was made less constricting was by the introduction of a variety of different types of certainty. The distinction between absolute, mathematical or metaphysical, certainty and moral certainty appeared early in the seventeenth century[1] and rapidly came into general use. In England a variety of sometimes quite complex systems of classification were drawn up. Glanvill and Wilkins both made a distinction between infallible and indubitable certainty, the former being the higher grade (Glanvill [1676], pp. 47–50; Wilkins [1675], pp. 8–10), and Stillingfleet found it useful to distinguish no

[1] Its origins are surprisingly obscure. Henry van Leeuwen traces it back to Chillingworth and hence (rather tentatively) to Grotius (Van Leeuwen [1970], pp. 21–2). Barbara Shapiro on the other hand ascribes it to the scholastics, though without giving any references to any scholastic author (Shapiro [1983], p. 84). Shapiro's derivation seems slightly more probable: Descartes, who was quite familiar with the idea, seems to have attributed it to the scholastics (letter to Mersenne, 21 April 1641, Descartes [1970], p. 99).

less than five different degrees of certainty: metaphysical, rational, physical, infallible and moral (Stillingfleet [1710], vol. VI, pp. 86–9). Perhaps the clearest system of classification was that used by Boyle: there are three levels of certainty and three types of demonstration: metaphysical demonstrations, which presuppose nothing and give absolute certainty; physical demonstrations, which assume as true various physical principles; and moral demonstrations (Boyle [1772], vol. IV, pp. 42, 182). No-one supposed that the conclusions of inductive inferences could claim any of the more stringent grades of certainty, but if they could aspire even to moral certainty they would thereby become possible objects of knowledge. Unfortunately this is an issue about which much less is said than one might desire, mainly because most of the discussions of the grades of certainty occur in theological contexts in which the reliability of inductive inferences is not really relevant to the points at issue. Among those philosophers who did give serious thought to the use of inductive generalisations in natural philosophy there was no real agreement. The general consensus of the scholastic philosophers had been that no merely probable propositions, however great their probability might be, could be certain (Smiglecki [1638], p. 661). This view was maintained by Locke, by John Wallis (Wallis [1678], pp. 170–1), and by Glanvill (Glanvill [1676], p. 45). Other thinkers held that we could be morally certain about at least some conclusions of inductive inferences. Samuel Parker believed that all general axioms (*e.g.*, that the whole is greater than its parts) 'are only the results and abridgements of a multitude of single Experiments' and are yet 'obvious and apparent Certainties' (Parker [1666], pp. 55–6). Wilkins, more cautiously, held that we could be morally certain that the sun would continue to rise (Wilkins [1675], p. 10). Leibniz, perhaps unconsciously, seems to have vacillated between the two positions. Isaac Barrow appears equally indecisive: on the one hand the confirmation of any proposition by frequent experiments is 'almost sufficient' to enable us to consider it as universally true (Barrow [1734], p. 74), which suggests that such conclusions are merely highly probable; on the other hand when any proposition is found agreeable to constant experience 'it will at least be most safe and prudent to yield a ready assent to it' (*ibid.*, p. 73), and this would appear to imply that such conclusions are at least morally certain.

4 HACKING'S ACCOUNT

We can therefore ask ourselves two questions about inductive scepticism and the origins of the 'problem of induction':

(1) Why does *radical* inductive scepticism seem not to have appeared before Hume?
(2) Why were many earlier philosophers relatively unworried by the implications of inductive fallibilism?

One answer to the first of these questions has been supplied by Ian Hacking in his book *The Emergence of Probability*. Hacking's book is mostly, as its title suggests, about the evolution of various notions of probability, but the final chapter is explicitly about the emergence of the modern problem of induction.

Hacking makes a sharp distinction between what he calls the *analytic* problem of induction—distinguishing good and inductive reasons and classifying the various degrees of evidential support—and the *sceptical* problem (Hacking [1975], p. 176). Discussion of the analytic problem goes back at least to Leibniz and Jakob Bernoulli, but radical sceptical doubts about induction appear only with Hume (*ibid.*, p. 177). Hacking's thesis is that the sceptical problem of induction became a possible problem only as a result of two events. The first was the emergence of a concept of what Hacking calls 'internal evidence'—that is, evidence other than testimony. It was this that enabled the modern concept of probability to emerge, and with it the analytic problem of induction. The appearance of the sceptical problem required one further change. 'Once the concept of internal evidence was established by 1660, the final transformation needed for the sceptical problem of induction was this transference of causality from knowledge to opinion' (*ibid.*, p. 180).

Hacking's claim that there was no concept of internal evidence in Medieval or Renaissance Europe has been damagingly criticised by a number of writers (Blackburn [1976]; Garber and Zabell [1979]; Laudan [1981], pp. 72–85). One particular weakness is that Hacking bases his argument on a very implausible (indeed quite unsustainable) claim about natural and conventional signs. Hacking writes that:

> 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.
>
> (Hacking [1975], pp. 47–8)

According to Hacking, therefore, the distinction between arbitrary and natural signs and the concept of internal evidence emerge together around the middle of the seventeenth century. One fundamental objection to this argument is that the distinction between natural and conventional signs, far from being a new discovery of the seventeenth century, was a commonplace of medieval philosophy. One of the standard features of scholastic treatises on logic is a section on signs and on the difference between natural and conventional signs. Indeed the distinction between these two kinds of signs is very much older still. Book II of St Augustine's *De Doctrina Christiana* is concerned with signs in general, and Augustine prefaces his discussion with a careful explanation (chs. 1–2) of the difference between those signs which signify by nature and those which signify by convention.

A rather less explicit but quite recognisable statement of the same distinction can be found in Aristotle, in chapter 2 of *De Interpretatione*, and Aristotle was almost certainly writing with Plato's *Cratylus* in mind. One of the speakers in that work, Hermogenes, maintains quite clearly the thesis that all names signify solely by convention (383C–D), and it would appear that such views are widely held among the sophists. Hacking makes much of a contrast between Paracelsus and his theory of natural signatures and Gassendi, and adds that with the work of Gassendi and his like-minded contemporaries, 'the discovery that all names are conventional thunders into modern philosophy' (Hacking [1975], p. 4). The difference between Paracelsus and Gassendi is certainly real enough, but the idea that we have here a radical *historical* discontinuity between two quite alien modes of thinking is quite illusory. The idea that a knowledge of the real name of a thing gives one an insight into its essence or nature is of incalculable antiquity, and the idea that Adam was able to give things their natural names, and not merely conventional ones, appears as early as Philo (*De Opifico Mundi*, 148–50). Such views continued to be maintained by Jakob Boehme, John Webster and others well into the seventeenth century (Aarsleff [1982], pp. 60–61). Indeed what characterises that century is not the appearance of the 'modern' view that all linguistic signs are conventional, but the effective disappearance of the opposite view as a serious intellectual option.

Hacking's other precondition for the emergence of a sceptical problem of induction, that causation must cease to be a possible subject of belief or opinion only, is more difficult to evaluate. One problem is that here, as elsewhere in the book, it is not wholly clear precisely what Hacking is trying to say. To say that 'Hume can begin only when causation is stolen from knowledge' (Hacking [1975], p. 181), or that (for Leibniz) 'Truth is ultimately demonstration' (*ibid.*, p. 185) is to indulge in a kind of philosophical impressionism: the general drift may be clear enough, but the particular point being made is not. Hacking's view appears to be that the sceptical problem of induction became possible once the old hope that one could (in principle at least) demonstrate the existence of causal connections between things had been abandoned. Hacking sees this hope fading rapidly in the late seventeenth and early eighteenth century, and finally vanishing with Berkeley, who might well have anticipated Hume had he not possessed such a strong aversion for all forms of philosophical scepticism. For Berkeley and Hume there are no necessary connections in nature. For Berkeley there are no causal connections whatever in the natural world; for Hume there are, but the idea of necessary connection comes from within ourselves, not from outside. The implication of this is that the causes of the appearance of radical inductive scepticism are ontological.

That this is Hacking's view appears most clearly in a comment made in a subsequent paper on this same topic. 'There is a sceptical problem of induction, not because (as with Glanvill) we may be in doubt that we have located the necessary connections that will guide our predictions about the future, but because we now think that there are no necessary connections, not even unknown ones' (Hacking [1981], p. 116).

This is in many ways an attractive suggestion. Glanvill's mild scepticism about his contemporaries' claims to have any kind of scientific knowledge does contrast strongly with Hume's vivid portrayal of his own complete cognitive disorientation in the last few pages of Book I of the *Treatise.* 'Where am I, or what? From what causes do I derive my existence, and to what condition shall I return? Whose favour shall I court, and whose anger must I dread? What beings surround me? and on whom have I any influence, or who have influence on me?' (Hume [1739], p. 269). There is nothing like this in any of Glanvill's writings. Glanvill indeed was much closer to Locke: confident of living in an ordered and intelligible world, but pessimistic about our capacity to discover very much of that order. Hume by contrast lacked this kind of ultimate assurance. The only feelings of confidence which he could have were those which arose from surrendering to natural propensities to believe which were themselves incapable of any kind of rational justification.

There are however some problems with this account. It could for example be objected that Hume does not deny the existence of necessary connections. If he had wanted to do so he would have denied that we have any such idea; in fact, his concern was to discover the impression from which that idea was derived. In reply to this it could be said that what Hume denied was the existence of necessary connections *in nature*: the impression from which the idea is derived is the customary transition which occurs in our thought from the idea of the cause to the idea of the effect.

That Hume held that there were no necessary connections in nature is uncontroversial. What is less clear is whether Glanvill (or indeed many of Hume's other predecessors) thought that there were. If they did not, then the radical difference between Glanvill and Hume which Hacking rightly remarks on will need to be explained on other grounds.

In considering this issue it is essential to distinguish carefully between *necessary* connections and *causal* connections. The view that there are no causal connections in nature appeared long before Berkeley or Hume. It can be found in certain Islamic philosophers such as al-Ashari (d. 935), whose views became known in the West through attempted refutations by Averroes and Maimonides (Wolfson [1976], ch. 8). Similar ideas reappeared in the seventeenth century, most notably in Malebranche. Hume was aware of at least the later stages in the history of this theory, and regarded it with little favour (Hume [1748], p. 73n). He was himself quite ready to ascribe causal powers to bodies. Indeed he had no real alternative: all the philosophers who had denied the existence of causes in nature had made God the immediate cause of all phenomena, and this was hardly an approach likely to appeal to Hume.

It would appear therefore that a denial of the existence of causal connections in nature has no obvious tendency to generate inductive scepticism. Malebranche and Berkeley were very little interested in the problems that were to concern Hume so much, and there is no sign that either of them would have had any leaning towards or any sympathy for Hume's approach.

In the case of the existence of necessary connections the situation is more complex. After all, what precisely *are* the necessary connections which Hume denied but which presumably some at least of his predecessors had supposed to exist? The fundamental maxim on which Hume grounded his argument against the existence of necessary connections is that every real thing can be supposed, without absurdity, to be capable of existing separately from every other real thing (Hume [1739], pp. 79–80, 173, 233, 247, 249, 466). It is this that prevents us from deducing any causal relations *a priori*, and compels us to reply solely on experience.

On this account there is a necessary connection between two entities a and b if either a cannot exist in the absence of b, or b cannot exist in the absence of a. Hume denied the existence of such necessary connections, but so also did a good number of his predecessors. Indeed the view that there are no necessary connections in nature resembles the view that there are no causal connections in having a past which extends back to the Middle Ages. Ockham introduced into philosophy the notion of what he called a *res absoluta*, and maintained that every *res absoluta* can exist independently of every other (*Quodlibetae*, VI, q.1; Ockham [1980], p. 605). For Ockham all substances and all real qualities are *res absolutae*.

The idea that lay at the heart of Ockham's views—that God, being omnipotent, could bring into being any possible state of affairs—was very widely held in the seventeenth century. Clearly it was an idea with the most far-reaching consequences, potentially at least. That its actual consequences were less radical can be explained partly by the general human tendency not to follow lines of argument as far as they can go, and partly for two other reasons.

The first is that almost all seventeenth-century philosophers either were positively attached to the metaphysics of substance and attribute, or at least (like Locke) found it impossible to discard it completely. Such a metaphysics clearly had a tendency to limit the scope of voluntarist arguments from divine omnipotence (as one may see if one tries to imagine how Descartes would have answered the question of whether God could create a thought without a thinking substance).

Secondly, nearly all seventeenth-century thought about the natural world had in the background the idea that the world is divinely governed. It might be admitted that God had the absolute power (to use the convenient scholastic term) to do anything that might be described without contradiction, but most of the range of possibilities thus disclosed were of little relevance to natural philosophers who believed themselves to be investigating the workings of a world created and providentially governed by a rational and benevolent deity.

Neither of these beliefs was shared by Hume, who discarded completely the traditional metaphysics of substances and attributes, and who disbelieved in the existence of God. It might be supposed that Hume's atheism would have led him to reject the voluntarist arguments against necessary connections which had relied for their force on the doctrine of divine omnipotence. In fact the conclusions survived, and the arguments were adapted with God being replaced by the human imagination. 'Whatever is clearly conceiv'd may exist after the same manner' (Hume [1739], p. 233). Hume was therefore able to reason along the same lines as his late medieval and early modern predecessors, and, unencumbered by many of their presuppositions, to arrive at conclusions which even he found to be deeply disturbing.

5 AN ALTERNATIVE EXPLANATION

Hacking's explanation of the emergence of inductive scepticism would seem therefore to be fundamentally unsatisfactory. Of the two parts of his explanation, one (the claim about signs) is certainly false, and the other is seriously inadequate. It would appear that what distinguished Hume from his predecessors was not the adoption of any radically novel metaphysical axioms. The most fundamental premises of his philosophy were far from new. What distinguished Hume and enabled him to formulate a kind of scepticism without clear historical precedent was a greater readiness and ability to pursue certain lines of argument to their ultimate conclusion, a temperament sympathetic to the construction of a systematic kind of philosophy (unlike Bayle), and a notable freedom from many of the philosophical and theological constraints which guided most of his predecessors.

In the remainder of this article I would like to set out an alternative hypothesis to Hacking's. Superficially it might appear to diverge from the account of Hume's position given in the last few pages, but on deeper examination the two analyses will be found to be entirely compatible with one another.

The problem of induction is at bottom a problem about inference from particular to universal propositions. It would seem therefore reasonable to suppose that there may be some usefully close connection between this problem and the metaphysical problems about the nature and existence of universals which have become known as the problem of universals.

The problem of universals as it existed in the Middle Ages and subsequently was essentially the problem of whether there are any *things* which are universals, or whether the only things that exist are particulars. Towards the end of the Middle Ages the two contending parties became known respectively as realists and nominalists, a pair of rather inappropriate names which have survived, but which are potentially very misleading. The view which has become known as nominalism is, despite the unfortunate connotations of the word itself, not primarily about names at all. Nominalism is best understood as the thesis that everything which exists is an individual or a particular, and realism as the denial of this. When Berkeley, in the person of Philonous, remarked to Hylas that 'it is an universally received maxim, *that everything which exists, is particular*' (Berkeley [1713], p. 192) he was both indicating his own adherence to the nominalist side and at the same time presuming that those whom he was trying to persuade would be of the same point of view. If we assume that Hylas was intended to be a plain man who had derived his philosophical opinions largely from Locke's *Essay*, then Berkeley was clearly justified in his approach. Hobbes, Locke, Berkeley and Hume were all quite unambiguously nominalists, not in the sense that they used this name for themselves, but in that they were in full agreement with Ockham and his followers on the fundamental principle that only individuals exist. (On the importance of nominalism in Locke's thought, and the misconceptions which arise when the character and historical continuity of the nominalist tradition is inadequately understood, see Milton [1981].)

The connection between the problem of universals and the problem of induction appears if we consider the truth-conditions of universal propositions. If we assume a correspondence theory of truth, then if we suppose that universal *things* of some kind exist, universal propositions can be true or false according to whether they correctly state how things are with the corresponding universal entities. This is perhaps clearest if we consider the early, as yet uncomplicated, theory of Ideas put forward by Plato in dialogues like the *Phaedo* and the *Republic*. Universal propositions about lines and circles are true not because of their relation to particular, sensible lines and circles, but because of their relation to the Ideas. Parallel accounts can be given for Neoplatonic, Aristotelian and Neo-Aristotelian theories, though the relations are more complex and more difficult to describe.

If on the other hand we accept the nominalist position, all this talk of universal entities is wholly and utterly mistaken, and so therefore are all theories which presuppose their existence. A universal proposition has to be considered as an infinite number of singular propositions. The universal proposition is true only if all the singular particular propositions are true, and it can be *known* to be true only either if it is deduced from higher-level universal propositions which are themselves known to be true, or else if we know that *all* the singular particular propositions are true. Since there cannot be an infinite regress in the first case (cf. *Posterior Analytics*, 72b5–25), it appears that in order to know the truth of any universal proposition we must first establish the truth of at least some universal propositions by investigating the truth or falsity of all their particular instances, and this may well be difficult or impossible.

There are a variety of possible responses to this predicament. One is, in effect, to admit defeat. In the words of the sixteenth-century Portuguese sceptic, Francisco Sanches (addressing an imagined scholastic opponent):

> You admit that there is no science of individuals, because they are infinite. But species are nothing or at least only a certain kind of imagination: only individuals exist, only they are perceived, from them only is science to be had, from them it must be taken; if not, show me these your universals in nature. You will put them in the particulars themselves. I however see nothing universal in them: everything is particular.
>
> (Sanches [1581], p. 126).

Here the connection between nominalism and scepicism is very clear. Another approach is no longer to require certainty, but to be prepared to be satisfied with probabilities. This retreat to probability is the way taken by nearly all modern writers on inductive logic. Yet another possibility is to suppose that in certain circumstances one need investigate only a limited number of particular cases in order to establish firmly the truth of a universal generalisation. One can find a very clear description of this kind of approach in the writings of such sixteenth-century Paduan Aristotelians as Ludovico Buccaferrea (Risse [1964], Vol. I, p. 217) and Jacopo Zabarella. Zabarella held that there is a particular kind of induction, demonstrative induction, which enables us to infer universal conclusions with certainty from an incomplete survey of the particulars. When using this kind of induction 'we do not take all the particulars: for when we begin to enumerate some few of them, it is at once apparent that the predicate is essential to them: therefore leaving aside the enumeration of the remainder, we infer the universal . . .' (*De Methodis* III.14, Zabarella [1608], col. 225F).

In another work, *De Regressu*, Zabarella describes demonstrative induction in a similar way:

> 'Demonstrative induction can be carried on in a necessary subject matter and in things which have an essential connection with one another. It does not therefore consider all the particulars, since after certain of them have been examined, our mind immediately discerns the essential connection, and then, disregarding the remaining particulars, at once infers the universal; for it knows that it is necessary how things must be with the remainder.'
>
> (Zabarella [1608], col. 485D).

This kind of demonstrative induction presupposes the existence of necessary connections between things which are capable of being intuitively grasped by our minds. As Zabarella says '*Inductio . . . demonstrativa fit in materia necessaria*', and if there are in reality no necessary connections to be grasped then the kind of demonstrative induction which Zabarella describes is impossible.

With this statement of Zabarella's theory in mind, we may return to examine its ultimate source, the account of induction in *Posterior Analytics*

II.19. As was remarked earlier, Aristotle appears to slide from an account of how we acquire general concepts to an account of how we get to know general truths. The reason he does this is certainly not philosophical incompetence, or mere carelessness. Rather it is that the two processes are extremely closely connected within the general framework of Aristotle's philosophy. In *Posterior Analytics* II.19 Aristotle was not describing the formation of something like a Lockean abstract general idea. For Aristotle what comes into the soul is a real universal *thing*, a form no longer individuated by matter. This is often concealed for the English reader by translators who put words into their translation which have no counterpart in the original, presumably with the aim of making Aristotle's thought more intelligible to modern readers. Tredennick, for example, in the Loeb edition, gives the following translation of 100a14ff:

> Let us re-state what we said just now with insufficient precision. As soon as one individual percept has 'come to a halt' in the soul, this is the first beginning of the presence there of a universal (because although it is the particular that we perceive, the act of perception involves the universal, e.g., 'man' not 'a man, Callias'). Then other 'halts' occur among these proximate universals, until the indivisible genera or ultimate universals are established. E.g. a particular species of animal leads to the genus 'animal', and so on. Clearly then it must be by induction that we acquire knowledge of the primary premises, because this is also the way in which general concepts are conveyed to us by sense-perception.

The word 'concept' in the last sentence has however no equivalent in the Greek text, which refers merely to universals (cf. 100a6). These universals are not mere 'general ideas', of the kind which appear in seventeenth- and eighteenth-century theories of knowledge. They are on the contrary real universal things existing in the intellect (*De Anima*, 417b23), and it is their presence there which makes possible the kind of demonstrative induction described by Zabarella. Once their existence is denied then two things begin to happen which determine much of the distinctive character of modern philosophy. If there are no real universals, then the only things to which universality can intelligibly be ascribed are signs which may be words or mental concepts or ideas, however conceived. Propositions about such signs are still possible objects of human knowledge, but inevitably they appear more and more limited in scope. Plato and Hume are superficially in agreement in regarding the relations of ideas as the true objects of human knowledge. The intellectual distance between them is a consequence of the utterly different meanings which they attached to the word 'idea'. Propositions of this kind are, according to Hume, 'discoverable by the mere operation of thought, without dependence on what is anywhere existent in the universe' (Hume [1748], p. 25). They can therefore tell us nothing whatever about the existence or nature of anything in that universe. Propositions about matters of fact and existence cannot be shown to be true or false by any kind of intellectual intuition, including the kind of intuitive induction described by Zabarella. The only way in which such propositions can even in principle be shown to be true is by ordinary non-intuitive induction, and this appears capable at best of furnishing us with probabilities, and at worst of giving us no reason whatever for accepting the truth of any universal generalisation. The problem of induction emerges as one of the central problems of modern philosophy.

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