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The Nature of Scientific Thought*

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ABSTRACT: 'Scientific thought' is regarded here as both a type of goal-directed behaviour (practice) and its product, and the question of its 'nature' posed in terms of that goal and of means appropriate for achieving it, preferably with regard to an existing paradigm (exemplar) such as the 'Galilean-Newtonian'. 'Empiricism', a widely received view of the nature of science, is examined and rejected, as is the general idea that scientific thought has 'philosophical foundations'. The question of the actual or possible scientific status of 'the human sciences' is raised and some methodological guidelines for an answer to it suggested.

1. INTRODUCTION: A SHORT DISCOURSE ON METHOD

1.0. The central theme of this contribution may be formulated as a question: 'What is scientific thought?' or, more nearly: 'What is *scientific* thought?'[1]. That is, it may be said to involve a request for a definition. But the topic of definition itself requires some preliminary clarification.

1.1. According to Aristotle (*Met.* 987b3), Socrates 'was the first to concentrate upon definition [*horismos*]', and, he goes on: 'Plato followed him, holding that the problem concerned not any sensible thing but entities of another kind . . . [that] he called "Ideas" . . . all sensible things being named after them'. Scholastics called the latter universals *ante rem*, that is, ones that are prior to sensible things 'in the order of being'. Aristotle criticised this doctrine, holding that universals exist – once more using the mediaeval terminology – *in re*, that is, in sensible things themselves. However, this divergence only marks out a difference between species of a common genus, called the 'essentialist' approach to definition. This has had numberless later variants, which, however, all tend to cluster around these two 'classical' types.

'Essentialism' has been subjected to many different lines of metaphysical criticism in the history of philosophy. But it is also possible to raise what may be called a purely 'methodological' objection to it that may be put in the form of the following question: 'How can a conflict between contrary essentialist proposals for a definition of a certain subject matter be decided?'.

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For Aristotle (e.g. *Top.*105a13), knowledge of universals is elicted from sensible things by a procedure of 'leading out [*epagōgē*]', undertaken by a special rational faculty (*nous*). In the end it is purely immediate, intuitive (cf. phenomenological *Wesensschau*). But, even supposing that there really is something to be thus intuited, and indeed that one such intuition does capture it in a particular case, what sort of procedure could, at least in principle, resolve a conflict between different intuitions which are, in the nature of case, each a matter of individual certainty? It would seem that contending parties must eventually end by simply – to use an Hegelian expression – 'swapping assurances', and thus that, ironically, a (metaphysical) essentialist approach issues, in effect anyway, in an (epistemological) relativism.

The Socratic-Platonic method is 'dialectical', in the original meaning of the word. That is, a claim about the true character of a universal from which a word derives its meaning is subjected to objections, in response to which the claim is reformulated or entirely replaced, and this in turn criticised, until, in the happiest outcome anyway, agreement is reached. Now, to start with, it might be asked how such a discussion can even begin, far less continue, unless the participants *already* know what they are talking about, so that the whole procedure is either unnecessary or impossible (cf. the 'paradox of analysis' and the 'hermeneutic circle'). But suppose it be rejoined that what enables the discussion to begin, and guides its subsequent course, is a sort of imperfect understanding of the universal, and that the point of the dialectical exercise is to replace this with 'clear and distinct' ideas. However, assume now that someone claims that what is thus first obscurely understood and then better – hopefully fully – understood at the end is not the character of a supersensible Idea but really just the meaning of a certain *word* in a certain language or even sub-language within a specific culture ('form of life'). If it were to be said in reply to this that the Idea just quite simply manifests itself at a certain stage, then we are back with intuitionism and hence with the problems that flow from this, in particular, relativism.

1.2. We can discern here the basic position on language of one of the chief contemporary opponents of the Socratic-Platonic line in ancient Greece, that of the Sophists (see, especially, the confrontation in Plato's *Cratylus*), as resulting from taking seriously the idea that the meanings which definitions seek to capture are really just meanings of words as such, so that a definition is thought of as concerning not a thing but a *word*, ultimately a *flatus vocis*, a 'vocal breath', to use a later term. To put the matter in a nutshell, on this view we do not, as essentialism holds, apply the same general name to various particulars because the latter exemplify the same universal; rather, conversely, we say that they exemplify the same universal because we in fact apply the same general name to them. This is the underlying idea of what became known as 'nominal-

ism', the great traditional counter-current to essentialism, represented, like it, by numberless different varieties.

Again, rather raise than any of the great number of metaphysical criticisms of this doctrine to be found in the history of philosophy, it is possible to pose a purely methodological objection, and indeed one similar in many respects to that brought against essentialism just above. This is that a strict nominalism – and it is pointless to consider watered-down versions – leaves, in principle, no room for critical assessment of alternative linguistic conventions, other than by reference to, for instance, convenience. However, cognitive – in particular scientific – progress often crucially depends on evaluations of competing concept-formations.

1.3. The foregoing discussion suggests that there is something right about both essentialism and nominalism, but that this consists in each's criticism of the other, rather than the positive content of either, which is unacceptable – surprisingly at first sight – for similar reasons. Essentialism points to the arbitrariness of a nominalist approach, its failure to allow for an element of what may be called objectivity in concept-formation. But nominalism points to the inadequacy of the essentialist grounding of this objectivity in 'actually existing' universals. An account more satisfactory than either would accommodate basic thrusts of both: nominalism's that the world does not *prescribe* univocally privileged representations of itself, and essentialism's that, nevertheless, the world significantly *constrains* such representations. To use a rough analogy: the world does not determine that spatial intervals should be measured according to, say, the metric rather than imperial system, but, the choice having been made, it is no longer conventional what the measure of a certain interval in that system is.

1.4. The following sketch of some main elements of a proposal for such an account will be confined, for the sake of brevity, to a certain class of concepts that is nevertheless very inclusive; in particular, it embraces scientific thought. More specifically, the focus will be on what may be called, for want of a better name, 'practical' concepts, that is, ones that designate items that fulfil a certain function or activities directed to the achievement of some goal, as well as the achieved goal. For example, 'chair' and 'carpentry' are practical concepts in this sense, but 'apple' and 'whittling' are not.

Practical concepts are (ideally anyway) univocal with respect to generic, defining ends, but not with respect to specific means sufficient to achieve those ends. For example, 'chair' can be defined in terms of certain functions that chairs have, but an in principle indefinite variety of sorts of things can be properly called 'chairs'; similarly, 'carpentry' can be defined in terms of the achievement of certain aims, but such a definition does not prescribe any specific tools and procedures.

The task of 'defining' an (existing) generic practical concept is by no

means always straightforward. For instance, the *OED* defines a chair as 'a separate seat for one person . . . usu. with a rest for the back and more or less comfortable', whilst the standard Australian *Macquarie Dictionary* defines it as 'a seat with a back and legs or other support, often with arms, usu. for one person'. These specify different conditions for applying 'chair', but following either would lead to our calling a 'chair' what in fact we call a 'car seat'. However, no serious problem need arise here, for we might simply say that it is a case of differences in use in different areas of one language. But such a conflict may arise even within a fairly homogeneous linguistic area. In such a situation we can and probably most often do proceed in the following way. We try to find items about which there is maximum agreement among competent speakers of the language that the word properly applies to them. These may be called 'paradigm' or 'logically primary' or 'basic' cases. This secures a reference, denotation, extension for the term. Then we try to determine its sense, connotation, intension, by endeavouring to find what minimum set of characteristics the members of this extension have in common. In fact, in all or at least a great number of cases there will be no *single* set of 'marks' the applicability of all of which, conjointly, is necessary and sufficient for the correct use of the term. Rather, the term in question will be held to correctly apply so long as just a 'sufficient' number of them conjointly apply in a particular case. Thus, we have to do with a 'cluster' concept, instances of which bear a 'family resemblance' (Wittgenstein) to one another. 'Chair' is an example of this.

At least two further remarks are in order. Firstly, a word defined thus in terms of its paradigm (logically primary, basic) use is often also employed in what might be called a 'non-paradigmatic' ('logically secondary' or 'derived') manner, which is to say that it is used in a certain situation only because the latter has some similarities to ones to which that word 'standardly' applies. For instance, what would normally be called a sort of bar-room stool might, in the context of an avant-garde furniture exhibition, be called a 'chair', or some school of artists might employ what is usually called 'whittling' as an aesthetic procedure and this then be called a form of 'sculpting'. Secondly, the paradigm use itself may actually change over time, probably most often by virtue of a change in status of a non-paradigmatic use. For instance, current standard English restricts 'marriage' to a certain sort of union between a man and a woman, so that to speak of a similar sort of union between people of the same gender as a 'marriage' is to use the word non-paradigmatically; but it is not being too daring to predict that sometime in the future, not too distant either, this will be included among paradigmatic uses.

Now, with reference to the criteria of adequacy indicated at the end of 1.3 above, it may be claimed that on this account the world does not prescribe a certain privileged representation of itself: that, for instance, one colour-classification rather than another reflects the real nature of things. It is thus possible to avoid essentially sterile, scholastic disputes as

to whether something 'really' is X. But the system of representations having been chosen, 'the world' determines, within crucial limits anyway, how it should be applied. There is a perfectly good sense to the idea of assessing concept-formations. In particular, there can be an objective assessment of whether a certain practical concept is indeed specific with respect to a certain generic one, by reference to the actual, tested adequacy of those specific means to the general end.

2. THE 'GALILEAN-NEWTONIAN PARADIGM'

2.0. The methodology just sketched indicates that the problem of delineating the character of 'scientific thought', considered as a 'practical' concept denoting both a process by which a certain result is arrived at and that result itself, should be tackled by identifying (in a purely referential sense) a relevant paradigm, and then explicating the 'sense' of that paradigm in both its generic and specific aspects.

The solution to the first problem is fairly easy: it is surely the core of the 'Scientific Revolution', to all intents and purposes initiated by Galileo and completed by Newton, and hence one that may be appropriately called, eponymously, the 'Galilean-Newtonian paradigm', or 'GNP' for short. The succeeding problems just identified will be tackled, in turn, in the next two sub-sections[2].

2.1. An historical anchor point for a delineation of the GNP may be found in a few lines from the preamble to a draft (*ca* 1703) by Newton of 'A Scheme for Establishing the Royal Society':

> Natural Philosophy consists in discovering the frame and operations of Nature, and reducing them, as far as may be, to general Rules or Laws, – establishing these rules by observation and experiments, and then deducing the causes and effects of things. (ms cit. Westfall 1980, p. 632)

What, somewhat more explicitly, is Newton saying here? Consider first the 'generic' aspect of the GNP, that is, its character as a conception of scientific thought from the point of view of its general goal, aim, *telos*. This is encapsulated in the following words: 'Natural Philosophy consists in discovering the frame and operations of Nature . . . [their] general Rules or Laws . . . and . . . deducing the causes and effects of things'. About this passage we can say the following.

2.11. Natural Philosophy is said to be about 'Nature', clearly meant here in the sense of 'the material world, or its collective objects or phenomena' (*OED*). A corollary of this is that it is concerned with 'discovering', that is, of disclosing, bringing to light what is already there. The first view contrasts with, for instance, the common later idea that the subject-matter of scientific thought is 'experience', and the second with the idea that such

thought basically aims at articulating that 'experience' in one way or another. (For a contemporary example of such a constellation of ideas see, for instance, the doctrine of 'constructivism', chiefly associated in science and mathematics education with the work of Ernst von Glasersfeld, on which see Suchting 1992.)

2.12. Further, part of the task of Natural Philosophy is said to be the discovery of the 'frame of Nature'. In Middle English 'frame' meant 'the universe, the heavens, the earth, or any part of it, regarded as a structure', in Late Middle English to the early eighteenth century it meant 'any structure, device or machine constructed of parts fitted together', and from the late seventeenth to the early nineteenth centures it meant 'definite form . . . order' (*OED*). So Newton may be glossed here as saying here that it is part of the business of Natural Philosophy to discover the *structure*, that is, the way in which the parts of which Nature is composed are ordered.

2.13. The preceding point ties in with the aim of discovering Nature's 'general Rules or Laws'. The significance of this is apt to be lost on contemporary readers, used as they are to the idea that the search for laws of nature is a central – if not the essential – aim of natural science. In fact, this conception is a relatively modern one (Zilsel 1942, Ruby 1986). In particular, the idea of a quite general law of nature is not present in the Aristotelian conception of science: to the extent that something like it is there, Aristotle speaks only of determining how nature is 'always or for the most part' (e.g. *Met.* 1026b28–1027a28). In this regard it is important to note that the Newtonian conception as such does not exclude, for instance, statistical laws, which uniquely determine ensembles, though not the members of the latter. In other words, they are true quite generally and not only 'for the most part'.

2.14. The business of Natural Philosophy is to discover not only Nature's 'frame' but also its 'operations'. An 'operation', in the relevant sense, is an 'exertion of force or influence; working, activity; an instance of this. . . . the way in which a thing works' (*OED*). This connects with what is affirmed at the end of the passage, namely, the centrality for Natural Philosophy of finding causal relations. This contrasts sharply with at least two crucial aspects of the Aristotelian conception of science (*epistēmē*): first, its basically merely *classificatory* goal, and, second, to the extent that it may be said to have had a dynamical (rather than static-classificatory) goal, the primacy accorded what in Latin translation was called the *causa finalis* as opposed to the *causa efficiens* privileged by the GNP. It also contrasts with those later conceptions that replace the dynamical-causal goal with that of finding, say, simply functional relations.

2.15. Natural Philosophy is said to be concerned with 'deducing' one thing

from another. It is important to note that 'deduce' here should not be taken in its contemporary formal-logical sense, traceable to Aristotle's idea of strict demonstration (*apodeixis*) which is necessary for genuine science (*epistēmē*). Rather,it is a looser sense (that in which Sherlock Holmes uses it), namely, to 'infer or draw as a logical conclusion (*from* something already known or assumed); derive by a process of reasoning' (*OED*).

This elucidation permits a further point about (2.14), namely, that the passage does not say that the 'general Rules or Laws' are necessarily *themselves* causal, but only that they permit the 'deducing' – in the sense of inferring – of causal relations. For instance, the law of conservation of energy is not a causal law, but a principle of invariance; however, it may be used to to infer the existence of certain causal processes involving gain or loss of energy.

2.2. Now, as regards the 'specific' aspect of the GNP, that is, the general conception of the means most appropriate for the attainment of the goals specified in its generic aspect, there are two main themes in the above passage. One may be said to relate to logical features of the process of realising the aims of science, and the other to epistemic features of this process.

2.21. The first theme centres on the idea of 'reducing' of Nature to laws, and then using them in a process of 'deducing'. The second term has just been glossed. As regards 'reduce', the word does not here have the sense of 'identify with' (in whole or part), as, for instance, when a contemporary writer may talk of 'reducing' chemistry to physics. Rather, in Newton's seventeenth century usage it retains its strong etymological connection with the Latin *reducere*, to 'lead back' (as in a *reductio ad absurdum* argument). Thus 'reducing' the structure and dynamics of nature to 'general Rules or Laws' means discursively exhibiting the general patterns of the former.

Now the real force of Newton's linking of 'reducing' and 'deducing' (inferring) is that it adumbrates Newton's 'analytic-synthetic' method, which is essentially the same as what Galileo referred to as the 'resolutive-compositive' method[3]. This may perhaps best be approached in terms of its contrast with what it contested and ultimately replaced, namely, the Aristotelian conception.

According to the latter the object – in the sense of the subject-matter – of science is the world as it is vouchsafed to people through the functioning of normal sense-organs in normal circumstances. '. . . principles . . . require to be judged from their results, and particularly from their final issue . . . And that issue in the knowledge of nature is the phenomenon as it is always authoritatively given by sensation [*to phainomenon aei kuriōs kata tēn aisthēsin*].' (*On the Heavens*, 306a15–17. Cf. *Pr.An.* 46a17–22, etc..) The goal (as already alluded to above) is to discriminate the main

classes of things thus offered to inspection and to determine the relations of inclusion/exclusion between them. Such classes were conceived as being marked out by sets of sensible qualities united by the tendencies of things to reach and/or maintain certain end-determined states (e.g. *Physics*, II,8). Thus we can say, for present purposes at least, that the Aristotelian conception places central emphasis on the ordinary perceptual world and on teleology.

Now this is very much in harmony with the basic perspective of that ordinary life the character of which, considered at this level of generality, is almost invariant throughout human history. (This is one very important part of the explanation for the special 'grip' of Aristotelian thought.) Firstly, everyday life is largely carried on within the framework of the deliverances of the unassisted sense-organs. Secondly, everyday thinking is or tends to be teleological in at least two essential respects: perceptible things tend to be classified within the context of the ends or means of human action (which is, most fundamentally, that of material production and reproduction) and there is an at best only shaky recognition of the distinction between people and the world, explanation tending to be on the model of the familiar relation between intentions and their execution. (These might be called the 'anthropocentric' and 'anthropomorphic' aspects of everyday thinking respectively.)

This approach is, for many reasons, quite unsuited to the task of realising the general aim of the GNP, namely, to discover strict invariances and, by means of them, causal relations. For one thing, the unassisted sense-organs do not have access to more than limited parts of the world, as regards either sorts of things, properties and relations, or the scale of those parts to which they do have access; for another, they are unable to make very sensitive discriminations even within the scale accessible to it; furthermore, their deliverances are variable and therefore often unreliable; moreover, conceptualisations of the world from the perspective of human action simply do not offer means for grasping very general patterns. Directly related to these considerations, a situation as more or less immediately presented is always the nexus and result of many different interactions, and everyday thinking commands only very meagre means for discriminating the relevant elements and their ways of relating, so that commonsense tends to be holistic, syncretic and analogical rather than analytic and genuinely systematic.

The specific GNP with which we are concerned proposes to overcome the first four of these limitations in at least three ways, all of which can be described as a process of 'reduction' ('analysis', 'resolution'). Firstly, it introduces concepts of things, properties, relations which are not immediately accessible to ordinary perception, are 'unobservable' (from atoms and electromagnetic fields to the neurological bases of generative grammar) and which are not formed in terms of means or ends of human action. Secondly, whereever possible, it replaces unassisted sense-organs with material instrumentation. Thirdly, it introduces the radically new

procedure of constructing 'in thought' situations where abstraction is made from the complexity of the elements and interactions present in real situations – 'pure' or 'ideal' cases – representations of actual situations then being constructed by adding to the model, bit by bit, more elements and their characteristic modes of interaction, a process that may be described as a 'deduction' ('synthesis', 'composition') of (a representation of the) real situation[4]. This threefold movement is summarised with classical brevity by Galileo:

> No firm science can be given of such events of heaviness, speed, and shape,which are variable in infinitely many ways. Hence to deal with such matters scientifically, it is necessary to abstract from them. We must find and demonstrate conclusions abstracted from the impediments, in order to make use of them in practice under those limitations that experience will teach us. (1974, p. 225 – and cf. 1962, pp. 207f)

It should be noted here that this procedure means that the 'general Rules or Laws' are necessarily of *conditional* form, since they concern, in the first place, a range of possible situations, whereas in Aristotelian theory of nature generalities are *categorical.*

At this point it is worth making a comment about a certain view of the relation of the sciences to 'commonsense', widespread today, but already expressed over a century so ago by Thomas Henry Huxley with his usual exemplary brevity and trenchancy: 'Science is nothing but trained and organised common sense' (1893/1968, III, p. 45, elaborated at II, pp. 361ff). (Cf. Popper 1972, p. 22: '. . . Scientific knowledge . . . is *common-sense knowledge writ large* . . .') The preceding has sought to indicate at least some of the main ways in which the standpoint of ordinary life is too *limited* to permit the attainment of the ends constituting the general GNP. A fuller treatment would have to show how it is a positive *obstacle* to scientific thought. For it pervasively involves deeply entrenched conceptions that, though explicable and indeed often useful from the point of view of the needs of ordinary life, are positive *hindrances* to scientific understanding. One of the most obvious examples relates to the rotation of the earth both diurnal and annual. In this regard Galileo again made the point in classical terms:

> I can never sufficiently admire the outstanding acumen of those who have . . . through sheer force of intellect done such violence to their own senses as to prefer what reason told them over that which sensible experience plainly showed them to the contrary . . . 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 mistress of their belief. (1962, p. 328)

There are innumerable other instances, like the idea that all motion has to be sustained by an agent of motion, which stood in the way of the fundamental conception of inertial motion, and the 'substantantialistic' prejudice, according to which all states and events must pertain to some substance, an idea that held up, amongst other things, the development of optics and electromagnetic theory (through, for instance, the idea that

waves must be waves 'in' or 'of' some substance). A science can only be constituted at the point of a 'break' or 'rupture' with such everyday ideas[5].

2.22. As regards its epistemic aspect, Natural Philosophy is said to have the task of 'establishing these rules by observation and experiment'. About this we can say the following.

a) In late seventeenth century usage to 'establish' meant to render something or, especially, someone's position secure. This is a very much more modest cognitive aim than the Aristotelian criterion of modal necessity and epistemic certitude for science (*epistēmē*).

b) The conjunction in the phrase 'observation and experiment' indicates that Newton saw the two as distinct. What is the difference? Roughly, observation is a matter of registering what is simply the case, whilst experiment involves a specially instituted interference in the normal course of things (obviously, usually by means of some material means). For instance, it may be said that Newton's 'establishing' of the composite character of ordinary light involved an experiment (darkened room, creation of pin-hole for entry to light, use of a specially prepared glass prism, and so on) whereas noting the colours of the spectrum and their order was a matter of observation. Obviously the two are related in complex ways with many grey areas. But what has been said suffices to point up the profound conflict here with the Aristotelian conception, according to which 'a sense is what has the power of receiving into itself the sensible forms of things without the matter, in the way in which a piece of wax takes on the impress of a signet ring without the iron or gold' (*On the Soul*, III,12, trans. Smith/Barnes). This means that interference with the object of knowledge obstructs any attempt to gain knowledge of it. So what on the Aristotelian conception is *sufficient* to *prevent* adequate knowledge may according to the GNP be *necessary* to *acquire* such knowledge.

2.23. Finally, bringing together some main features of the 'logical' and the epistemic aspects of the specific GNP, it may be noted that the procedures of introduction of 'unobservables' and of idealisation, belonging to the first, and of experiment belonging to the second, are not just two separate matters but are 'internally' related. For experiment is necessary both to explore the domain of 'unobservables' and to make available to the inquirer in *reality* – at least to the best approximation that the currently available instrumental techniques permit – the 'ideal' situations already envisaged in *theory*, the converse of this being that theory provides a guide, a sort of 'template' for experiment.

2.3. It may be remarked in conclusion that what has so far been distinguished as the 'specific' aspect of the GNP can be and has been further specified in various ways. The historically most significant instance of this is doubtless that based originally upon the model achievement represented

by Newton's celestial mechanics, often referred to as 'mechanism' or the 'mechanistic conception of nature'. A particularly clear formulation of this may be found in Helmholtz's monograph of 1847 'On the Conservation of Force':

> The task of the physical natural sciences is to reduce the phenomena of nature to invariable forces of attraction and repulsion, the intensity of which depends upon distance. The solubility of this problem is at the same time the condition for the complete understanding [*Begreiflichkeit*] of nature. (Sambursky 1975, p. 399 [trans. rev.])

Now the Newtonian approach was, at least in principle, just one among possible alternative ways of realising the specific GNP. But so great did its prestige become – justifiably, in view of its successes – that it was widely identified – as the above passage from Helmholtz shows – with the sole way of understanding nature, with the very character of natural scientific knowledge as such. There are a number of profound dangers inherent in such a conflation. One is that it will stifle the search for different approaches. Another is that the eventual exhibition of the limits of applicability or even the long-run failure of what is really just one possible way of using a paradigm may be interpreted as a limitation or even failure of scientific thought in general. This is,of course, precisely what happened, especially round the turn of the century, when the crisis in certain traditional theories was interpreted as a failure of science as such. (This question is still on the agenda, especially with regard to quantum mechanics.) Nothing could better illustrate the fundamental importance of the notion of a paradigm as it has been sketched earlier and of distinguishing between different levels of generality or specificity in that area.

3. EMPIRICISM

3.0. There has for long been a strong tendency to identify the general standpoint of scientific thought as 'empiricism'. Though this has more recently fallen out of favour in various quarters, its intuitive attraction is very powerful and it may be doubted whether it will not outlast most of its critics, especially given the general fickleness of philosophical fashion. So it will pay to look at 'empiricism' in the light of the conceptions developed above; at the very least this might cast a little more light on the latter.

3.1. To start with, despite the fact that the term is used so frequently and so confidently, it often proves to be surprisingly difficult to pin down exactly what is meant by it in a particular context. It is even more difficult to find a general characterisation that is likely to include all and only those doctrines that have been thus denominated, even the historically and philosophically more significant ones. In the face of this problem the

method of 'paradigm cases' may be called upon once more. More specifically, at least the general lines of the position set out in the relevant writings of David Hume may appropriately be used for this purpose: if Hume is not an 'empiricist' then who is?

At the beginning of Section IV of *An Enquiry Concerning Human Knowledge* (1748), his definitive summary of the epistemological foundations of his system, Hume writes: 'All the objects of human reason or enquiry may naturally be divided into two kinds, to wit, *Relations of Ideas*, and *Matters of Fact*'. The truth of propositions about the first is 'discoverable by the mere operation of thought, without dependence on what is anywhere existent in the universe'. What, correspondingly, is 'the nature of that evidence which assures us of any real existence and matter of fact'? First, there is 'the present testimony of our senses, or the records of our memory'. Second, claims that go beyond this are 'founded on the relation of *Cause* and *Effect*'. But, by a complex argument,which fortunately need not be gone into here, Hume comes to the conclusion that, roughly speaking, beliefs in the existence of causal relations are formed by mechanisms that are rooted in the first type of knowledge. So, ultimately, everything is grounded on the latter.

3.2. This Humean position was subjected to a great deal of criticism in the century of its inception (for instance, by Thomas Reid and Kant), criticism which continued through the nineteenth (for example, by Hegel and T.H. Green), and well into our own, from a very large variety of standpoints (from Dewey and Husserl through the later Russell to Quine and beyond). But it is no less true that strongly influential Humean positions still exist, both in broad intellectual and in professional philosophical circles, sometimes in the form of very sophisticated doctrines like the 'constructive empiricism' of Bas van Fraassen. Much of this criticism is technically very sophisticated and ingenious, but, however probative, it is, more often than not, essentially *criticism of empiricism within an essentially empiricist framework*. Such 'immanent' criticism is of maximum value if it is carried out in the service of a more fundamental critique; but if it is not it tends to lead to just another, allegedly 'improved version' of empiricism. So the basic problem is to find what is really constitutive of this doctrine and to examine that.

Such an inquiry may conveniently start from some of the apparently quite unproblematic, innocuous words cited above. More specifically, Hume writes that our basic 'evidence' is sense-experience, in the form of present 'testimony' or mnemic 'records'. Now as the *OED* says, 'evidence' means, in the relevant sense, 'facts or testimony in support of a conclusion, statement, or belief . . . an oral or written statement', and a 'record' obviously assumes (in its paradigmatic use anyway) some sort of symbol-system. For the sake of brevity and simplicity, attention may be focussed on sense-experience, because, as has been seen, for Hume both this and

memory are species of evidence and this is confirmed by his reference to the 'records' of memory.

The simple but decisive point to be made in the present connection is that sense-experience as such cannot – cannot *in principle* – be 'evidence', for it belongs to a different *category* from the *statements* which can alone stand 'in support of a conclusion, statement, or belief'. Certainly we often say, for instance, that a certain person's fingerprints at the scene of a crime are evidence of his guilt. But this is just a conveniently brief way of speaking about the relation of a certain physical configuration to a certain set of beliefs or hypotheses which are, as such, discursive in character. (A similar remark applies to, say, talk of a geological 'record'.) A passage from the one context to the other is of the general sort that Aristotle called a *metabasis eis allo genos* (*On the Heavens*, 268b1), a 'transition to another kind', in fact an instance of what Gilbert Ryle a long time later dubbed a 'category mistake'. Empiricism thus conflates two fundamentally different contexts. One is the context of *causal* relations. These hold between, firstly, the world and human sense-organs (more generally, 'receptors' of various sorts, including instrumental ones, though ultimately the latter must interact with human sense-organs if knowledge is to be generated), and, secondly, the results of this interaction and various beliefs or statements. The other is the context of the beliefs or statements, and the conceptual system or body of representations that they assume. Only these have *epistemic/cognitive* significance.

Perhaps, in view of the subjectivism rampant in contemporary thought, it should be emphasised that the 'truth-value' of beliefs or statements is ultimately controlled by what the world is like as revealed by its causal input into discourse, that though knowledge must necessarily be expressed in some language, the latter does not somehow generate its own object, it does not determine what linguistic items should, at a certain stage, be rationally counted *as* knowledge[6].

Thus the *key* inadequacy of empiricism has really nothing to do with the centrality it accords to sense-experience; in particular, the controversy over whether the 'basic language' of science should be 'phenomenonalistic' or 'physicalistic' is irrelevant to the main question, a mere internal family dispute, as it were. The central deficiency of empiricism is one that it shares with a wide variety of other positions, namely, all those that see objects themselves, *however they are conceived*, as having epistemic significance *in themselves*, as inherently determining the 'form', as it were, of their own representation, rather than as determining the degree of applicability of representations of a given 'form', and hence, conversely, that the nature of what is represented can be more or less *directly* 'read off' its representation.

For instance, no one who understands the idea of an arithmetical average would think that to say that the average height of people in a certain room is five feet ten inches necessarily means that anyone there is just that height; no-one who understands what a statistical mean signifies would

think that saying that the mean number of children in families in a certain reference-class is two and one half entails the existence of dimidiate offspring; no one conversant with elementary calculus would think that the limit of a certain convergent series that is a relative frequency designates something that can be found somewhere in the world of actual events. But by the time we get to say, quantum mechanics, we find that many people are very puzzled by the question of what corresponds in reality to the difference between unitary and non-unitary operators, when this is just a distinction internal to the representation, and the real question is that of the interpretation and adequacy of the theory as a whole as judged by its ability to explain, predict and give guidance to further research into the micro-world it deals with[7].

Classical empiricism, so hostile to Aristotle, thus finally joins hands with him, both being simply variants of a more deeply shared common position. Indeed, Aristotle, with his conception of universals as emerging *from* the real by *epagōgē*, is, so to speak, the 'open secret' of empiricism.

4. DOES SCIENTIFIC THOUGHT HAVE 'PHILOSOPHICAL FOUNDATIONS'?

4.0. Empiricism is one version of the view that knowledge, in particular scientific knowledge, has 'foundations', which, since they are allegedly common to all sciences, cannot be the province of any one of them; rather, they are conceived of as falling within the subject-matter of philosophy. Hume gives a classical specifically empiricist formulation of this view in the introduction to his first and most comprehensive philosophical work,

A Treatise of Human Nature:

> There is no question of importance, whose decision is not compriz'd in the science of man [*scil.* a *philosophical* 'science' of 'the principles of human nature'] and there is none, which can be decided with any certainty, before we become acquainted with that science . . . the science of man is the only solid foundation for the other sciences . . .

In turn, 'the only solid foundation we can give to this science itself must be laid on experience and observation'. However, it has been argued in the preceding section that these epistemological 'foundations' are simply causal conditions for the existence of knowledge. That knowledge should have such conditions is no more surprising than that architecture, say, or language does (for instance, the existence of more or less rigid bodies or of mechanisms for producing differentiated sounds at will, respectively); indeed, it would be incomprehensible if it did not. And, of course, all such conditions are themselves in principle open to scientific inquiry.

4.1. But it has been traditionally claimed by innumerable thinkers, and continues to be, that scientific knowledge also has cognitively even grander 'foundations' ('assumptions', 'presuppositions', etc.), namely, 'metaphys-

ical' (or 'ontological') ones. For a brief, trenchant formulation of this view we may turn again to Thomas Henry Huxley (1893/1968, I, pp. 60f):

> All physical science starts from certain postulates. One of them is the objective existence of a material world . . . Another . . . is the universality of the law of causation; . . . another is that any of the rules, the so-called 'Laws of Nature' . . . is true for all time. The validity of these postulates is a problem of metaphysics.

These may be called, for the sake of brevity, the 'postulates' of Realism, of Universal Causality (roughly: 'every event has a cause') and of Causal Constancy (roughly: 'every distinct type of event has the same distinct type of cause'). He goes on to write that these 'postulates'

> are neither self-evident nor are they, strictly speaking, demonstrable. The justification of their employment, as axioms of physical science, lies in the circumstance that expectations logically based upon them are verified, or, at any rate, not contradicted, whenever they can be tested by experience.

This second passage may serve to draw attention to the fact that the general doctrine that science *has* metaphysical assumptions must be clearly distinguished from doctrines as to whether we are *justified* in making those assumptions. In other words, the doctrine that there can be scientific knowledge only if we are justified in holding other beliefs (the relevant 'metaphysical assumptions') has to be distinguished from doctrines as to whether we are justified in so doing. If not, then a 'sceptical' position is called for. So, far from scepticism's being entirely antithetical to a positive ('dogmatic' in an older sense of the word) answer to the question of the justifiability of 'metaphysical assumptions', it is simply an *alternative answer* to the *same question* as that addressed by non-sceptical thinkers.

4.2. Turning now to the more specific content of the above passages, the first says that the problem of the 'validity' of the three postulates falls to metaphysics, but the second says that they are neither self-evident nor 'demonstrable' in the strict sense, which may be taken to mean deducible from premises to which we are somehow bound to give assent. It is unclear whether the conjunction of these two views means that the specifically metaphysical problem of the 'validity' of the postulates is insoluble or that it may be soluble within metaphysics but not by exhibiting their self-evidence either direct or indirect. But let us drop this theme for a moment (it will be returned to in 4.4 below) and turn to Huxley's more positive position.

4.3. He says that the justification of the three postulates resides in the fact that 'expectations logically based upon them are verified, or, at any rate, not contradicted, whenever they can be tested by experience'. Now the qualification 'at any rate, not contradicted' can be discounted, since any number of what may be called scientifically idle statements are 'not contradicted' by any tests (for instance, the seventeenth century Occasionalist doctrine that what are apparently direct causal relations between two

states of affairs are really mediated by God). So the claim becomes that the three postulates are justified by the fact that 'expectations logically based upon them are verified . . . whenever they can be tested by experience'.

But, in the first place, if physical science indeed 'starts from' these 'postulates' or 'axioms', that is, if the procedures and hence the results of the former *presuppose* the latter, how can the former be 'verified' by them, in the sense of positively confirmed? Surely this would be arguing in a circle. For instance, what would count as a verification of the postulate of Realism? An initially perhaps plausible answer is the palaeontological record, which shows that human beings and indeed living things in general are fairly recent occurrences on earth, as the relevant time-scales go. But this sort of evidence is only probative if we have already *assumed* a Realist standpoint; otherwise, it may be held (and Bishop Berkeley did in fact hold) that the world existed before the appearance of human beings only by virtue of the operations of God.

Furthermore, how could any of these be 'tested' in the sense of subjected to possible falsification? For instance, referring now to the second postulate, a failure to find a cause for a certain sort of item does not entail that one does not exist. (This is a purely formal-logical point. For the second postulate affirms, roughly: 'For every type of event E there is some cause', and the negation – that is, falsification – of this is: 'No event is a cause of an E-type event', which is equivalent to a *universal* statement which, as such, cannot be exhaustively verified.)

Finally, consider the third and final postulate. Any apparent failure of Constancy of Causation, in particular, time-dependence of a *specific* 'law of nature' will always be found to be based on the assumption that certain *others* are invariable, in particular with regard to time. Again, take what J.S.Mill called the 'Method of Agreement and Difference', that is, briefly, the procedure by which a causal relation between states of affairs of sorts A and B is inferred by establishing that an instance of B is always present when an instance of A is and the latter never occurs without the former. Such an argument presupposes that the situation is characterised by causal constancy in the sense that the results of one experiment hold for all, other things being equal. So, since the results of this sort of procedure *assume* a principle of constancy, they cannot also *test* it[8].

4.4. Turning now to the question of the possibility of a metaphysical (rather than scientific) validation of the three postulates, dropped at the end of 4.2 above, it hardly needs to be said that there have been many attempts in this direction, and a thorough inquiry here would involve a case by case examination of all of them. But a very general overall point may be made which suggests that there is no hope in this direction. This is that attempts at metaphysical validations that go beyond endeavours to show that these postulates are somehow self-evident (and the very formulation here exhibits the incoherence of such an enterprise) in general

try to show that they are the necessary consequences of some other principles. Now either these other principles are claimed to be self-evidently true – which is hardly a justification – or are themselves subject to justification, in which case we have either a vicious regress or an appeal to one of the postulates the justification of which was the original aim.

4.5. The tentative conclusion of the preceding examination is that if scientific thought is indeed based upon postulates of the sort that Huxley and like thinkers have presented, then they cannot be justified and we are left with scepticism. However, there is another path. This consists, to start with, in a rejection of the assumptions that give rise to the problem, that is, a denial of the antecedent of the preceding conditional, or, at least, in a thoroughgoing reinterpretation of its meaning.

It may be recalled that 'postulate' has its etymological origin in the Latin for 'demand' and that it was the corresponding Greek word that Euclid used for part of that on which it was necessary to gain agreement if the construction of the system of geometry were to begin. This is one approach to the position that, insofar as it may properly be said that scientific thought involves 'postulates', then these should be viewed not as true or false *statements* but as '*rules of the game*' of scientific inquiry. They cannot properly be said to be verified or confirmed by the successes of the latter, nor can they be disconfirmed or falsified by its failures, though the former may encourage people to continue to 'play the game' of science, and the latter discourage them from doing so. More specifically, what Huxley calls the postulate of Realism formulates, roughly speaking, the idea that to engage in the practice of a science is to be engaged in a process not of creation but of discovery of the character of a subject-matter that is 'ontologically' quite irreducible to thought, and, 'epistemically', though inexhaustible by thought, accessible to it without limit. The postulate of Universal Causality formulates a 'program' or 'policy', namely, that of finding causal conditions for states of affairs. The postulate of Causal Constancy formulates, *inter alia*, a condition for a certain procedure for eliminating alternatives in a test situation. It could be that, in fact, there are limits to knowledge, that some items in the world have no causal conditions, or, if they do, no strictly invariable ones. But such possibilities have no relevance for the 'postulates' governing the business of science.

Ultimately, as rationally reconstructed at least, 'being scientific' is a matter of choice, is the exhibition of a set of preferences for such things as a non-dogmatic, anti-fideistic, critical attitude in which strength of belief is attuned to evidence, and for 'open horizons' over closures. In no sense whatsoever does this entail that scientific thought itself is constituted by values: it means only that the adoption of the scientific attitude is so based. To use a rough analogy, someone may or may not decide to play chess, but once the choice has been made that person must 'play the game'.

Finally, this 'framework' character of the sort of 'postulates' in question, their role as helping to *constitute* the distinctive practice of the sciences, accounts for the plausibility of the idea that they are 'foundations' for scientific thought.

4.6. The sciences considered in their own terms, that is, independently of attempts to supplement them philosophically, are constantly engaged in a 'bootstrapping' procedure, by virtue of which *acquired* results at one stage of development, or in one branch, *function* at another stage, or in another branch at the same stage as principles of method (heuristic, interpretative) in the process of *acquiring* further results. Spinoza set out the general idea memorably in his *Treatise on the Emendation of the Intellect* (*ca* 1662):

> just as men, in the beginning, were able to make the easiest things with the tools they were born with (however laboriously and imperfectly), and once these had been made, made other, more difficult things with less labour and more perfectly, and so, proceeding gradually from the simplest works to tools, and from tools to other works and tools, reached the point where they accomplished so many and so difficult things with little labour, in the same way the intellect . . . makes intellectual tools for itself, by which it acquires other powers for other intellectual works, and from these works still other tools, or the power of searching further, and so proceeds by stages . . . [T]he more the mind knows, the better it understands its own powers and the order of Nature. The better the mind understands its own powers, the more easily it can direct itself and propose rules to itself; the better it understands the order of Nature, the more easily it can restrain itself from useless pursuits. In these things . . . the whole of . . . Method consists. (*op. cit.*, §§31,40, E. Curley trans.)

Or as Pascal put in even more briefly in the preface to his *New Experiments concerning the Vacuum* (1647): 'The experiments which give us our knowledge of nature multiply continually . . . and . . . are the only principles of physics'.

5. A PROSPECTIVE QUESTION: CAN 'HUMAN SCIENCES' BE SCIENTIFIC?

5.0. The discussion of the specific theme of this contribution began with an exegesis of some words by Newton about his conception of 'Natural Philosophy', and so far it has maintained, explicitly or implicitly, a primary reference to this area. In fact, discussions in philosophy of science and theory of science education have been dominated by the natural sciences (and, to a somewhat lesser extent, the mathematics so closely connected with them). This has happened, for one thing, because the natural sciences (that is, in the first place, physics, then chemistry and the other sciences of inorganic nature as well as general biology) have been overwhelmingly the most successful and influential since the Scientific Revolution that began in physics. For another, not unconnected with this, philosophical points about scientific thought can very often be made more sharply and

clearly regarding them (and, particularly, of course, physics) rather than other disciplines.

But this makes it all the more important that at least some attention should be paid to the areas of inquiry occupied by those disciplines whose subject-matters are, wholly or mainly, human beings in both their individual and collective aspects, that is, broadly, what J.S.Mill for one used to call the 'moral sciences' (*System of Logic*, Bk VI), or what is now more usually referred to as the 'human sciences' (for example, psychology, sociology, history).

5.1. Arguably, the first question to be raised about them is precisely that of their status *as* sciences, 'within the meaning of the act' presented so far. It is impossible here even to sketch the main issues that arise in this regard. It must suffice to make just a few points of methodological principle about how such an inquiry should proceed, assuming the general character of the approach so far pursued.

To start with, the whole project of examination will get off on the wrong foot if it is assumed that the points at issue are resolvable in a blanket way by some very general philosophical considerations about the relevant subject-matters. An example is the claim that the 'human sciences' are different in principle from the natural sciences because the subject-matter of the first involves 'meanings', and hence a special method of 'understanding', and that of the second does not. Further, it should not be assumed that 'the human sciences' themselves form an undifferentiated block with respect to the question at issue, nor even that existing boundaries demarcating disciplines are permanent. For instance, as Steven Pinker (1994, p. 17) notes, current '"cognitive science" . . . combines tools from psychology, computer science, linguistics, philosophy, and neurobiology to explain the workings of human intelligence'.

This having been said, the considerations in section 1 above suggest that the question whether a particular discipline is to be regarded as 'scientific' should be decided with reference to the paradigm of what is regarded as such. More specifically, the question concerns not the satisfaction of some definite set of necessary and sufficient conditions but of the degree of 'family resemblance' between the candidate discipline and the paradigm, keeping in mind the fact that the nature and extent of relevant resemblances may change with the development of knowledge itself, in particular as a result of developments in the means of investigation available to a discipline, and that even the characteristics of the paradigm may alter.

For example, quite apart from the implications of quantum mechanics, recent study of non-linear systems within the theoretical framework of classical physics ('chaos theory') has led both to a revision of the ideal of strict predictability in the latter and the development of new means for mastering the complexities of such systems, by way both of novel mathematical methods of a traditional kind and of the working out of thoroughly

innovative techniques, like those of computer modelling/simulation (sometimes called 'wet labs' [see, for instance, Waldrop 1994, especially pp. 267ff]), which, in turn, have opened up new paths in the 'life sciences', economics, and the like. Along the same general path, the accelerating development of new technologies within, say, studies of mental functioning (for instance, Functional Magnetic Resonance Imaging and Magneto-Encephalography) has rendered many of the older – even quite recent – discussions of 'the mind' at best merely quaint and at worst obstacles on the path of development of scientific knowledge.

6. AN INCONCLUSIVE CONCLUSION

If there is a single general conclusion to be drawn from the whole of the preceding discussion it is that there is no final, 'ultimate' answer to the question of the nature of scientific thought, of the sort fantasised by many, especially philosophers. This is not because the question is preternaturally difficult, but because its subject-matter is not sempiternally fixed, since the sciences are always engaged in the process of redefining themselves. This is especially evident both from the calm point of view of what Braudel calls 'la longue durée', the long term, and when there occur those comparatively sudden spasms often called 'scientific revolutions'. Any satisfactory treatment must, in the nature of the case, be contextual, conditional, with an eye to open horizons: 'closed' answers must, for that reason alone, be suspect, indeed rejected.

NOTES

1. Space-constraints entail that not even all the main aspects of this question can be so much as mentioned, and that those which are will have to be treated very summarily indeed, precluding otherwise desirable elaboration and argument. A similarly necessarily restricted number of references may ameliorate this situation to some extent; moreover, further relevant literature (a good deal of which is mentioned in Suchting 1994) can be followed up from indications contained in these. The following includes some of the work that has been specially important to me in thinking about the relevant issues, though it will not be referred to again in the following notes. Above all there is the work of Louis Althusser, e.g. amongst many other things, his 1969 and 1990 and his contribution to Althusser and Balibar 1970. (This was the main proximate influence on e.g. Suchting 1986.) Further, there are the writings of Gaston Bachelard, a major influence on Althusser. Unfortunately, very little of his prolific work on philosophy of science is available in English – though his 1986 (1934) is to be recommended – but this is partly offset by the availability of two excellent expository works, namely, Lecourt 1975 and Tiles 1984. Further, there is Dewey's great 1938 (the current neglect of which is a scandal). Beyond these I have learned a great deal from, amongst others, Hacking 1983, Lakatos 1978, and Miller 1987. A good elementary survey of some trends in recent philosophy of science will be found in Chalmers 1982, and there is a comprehensive selection of what might be called 'main line' recent and current philosophy of science in Boyd *et al.* 1991.

2. See on the general character of the 'Scientific Revolution' Lewin 1935, and Mittelstrass 1972.
3. Note here and in the following that 'analytic' is etymologically related to the Greek *analysis* which Aristotle used to denote the 'resolution' of a problem by the discovery of its conditions – *Nic.Eth.* 1112b23 – as opposed to *synthesis* or 'composition', *Nic.Eth.* 1174a23, whereby the linguistic lineage of Galileo's expression becomes evident.
4. It is very important to distinguish the idea of a 'pure'/'ideal' case in question here from Max Weber's notion of 'ideal type' (see, e.g., Weber 1949, pp. 90ff). Firstly, he himself insists that the idea is not applicable in the natural sciences (where, he thought, the relevant concepts are purely empirical generic ones which 'merely summarize the common features of certain empirical phenomena', *op.cit.*, p. 100), an 'ideal type' being constructed from the point of view of certain cultural *values*. Secondly, even in the domain proper to them, they are of *purely* heuristic significance, so that, in particular, there is no question of their being part of a 'resolutive-compositive' methodology. See further on this point Nowak 1980, Ch.3.
5. Bachelard's splendid work on science's relation, or rather lack of it, to 'common sense', *La Formation de l'esprit scientifique* (1938, 8th ed. 1972), is unfortunately not available in English, but some information about it can be gained from the literature cited in note 1 above and from Balibar 1978. Quite recently there has been some published recognition of the fact that there is a 'rupture' between a science and the commonsense world, though without reference to Bachelard (Cromer 1993, Wolpert 1992).
6. The fundamental criticism of empiricism in the text is, at the very least, convergent with one of Wittgenstein's key later doctrines – see his 1953 as well as many other writings – namely, that 'meanings' arise, proximately, within linguistic practices and not by some more or less direct connection with actual states of affairs (in particular sensations). Rorty 1979 clearly distinguishes between the causal and cognitive contexts. There may well be a significant connection between the present criticism of empiricism and what Feyerabend – e.g. 1981, I, pp. 50ff – calls the 'pragmatic theory of observation', and also Popper's account of the 'empirical basis' in his 1972 [1934], Ch.V. But the issues are somewhat clouded here by Popper's later embrace of a 'correspondence' theory of truth (even if on the basis of a misunderstanding of Tarski's account of 'truth') and the inclination – at least – if the first to what has been called the idea of the 'theory-ladenness' of observation. (The latter is typically a thoroughly confused position. Sometimes it appears to be a doctrine about how peoples's perceptions are influenced by their theoretical beliefs, which is relevant to psychology rather than epistemology – insofar as it concerns the latter it is just a variant of empiricism. But sometimes it does seem to contain an inkling of the view presented in the main text to this note, namely, that the causal-experiential order on the one hand, and the epistemic/cognitive on the other are categorially distinct. One way of putting the point is that the term 'observation' is an index of the problem here: sometimes it seems to refer to a certain psychological process, sometimes to a certain sort of *statement* that may be produced on the causal basis of the latter.)
7. There is an excellent discussion of the quantum-mechanical example in Cartwright 1983, Essay 9. See especially pp. 199f. (Indeed, the whole book is to be recommended, and might well be added to note 1 above.)
8. So, all in all, the problem seems to be similar to that which Descartes brings up in his dedicatory letter to his *Meditations* addressed to the Faculty of Theology at the Sorbonne, where he says that 'we must believe in the existence of God because it is a doctrine of Holy Scripture, and conversely, . . . we must believe Holy Scripture because it comes from God', though 'this argument cannot be put to unbelievers because they would judge it circular'.

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