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PAUL HELM

Is there a Preferred Philosophy of Science for Christians?

After some remarks about the relationship between Christian faith and theoretical and experimental enquiry, particularly scientific enquiry, the author provides a sketch of some different approaches to the philosophy of science. The inevitable connection between the theory of science and scientific success is stressed. But there is a basic difference of approach between those who emphasise the formal conditions of explanation in natural science and those who claim that any explanation must, first and foremost, convey an increased understanding of the phenomena. The article concludes by stressing that, while the Christian has considerable liberty in his approach to the philosophy of science, it would be inconsistent with the Christian faith to adopt any philosophy which denied that there were objective truths of nature.

Key Words: Francis Bacon, explanation, falsification, Paul Feyerabend, Thomas Kuhn, objective truths, paradigms, philosophy of science, Karl Popper, prediction, verification.

In many modern areas of enquiry the thoughtful Christian is faced with a similar problem, that of relating the ever increasing complexity and sophistication of fields of research to the pre-theoretical world of the Bible. The problems are felt to be most acute in several of the special sciences, as well as in philosophy and in ethics. Two responses can be considered to be extreme, and to be avoided. One of these is to suppose that the Bible is more theoretically-orientated than in fact it is; to make it teach some favoured theory of biology, or psychology, or metaphysics. The problem with such a response is that invariably, when the attempt is made to view the data from one standpoint, they do not all point in the same direction. And the approach carries dangerous consequences. For if the Bible is a theoretical book, how can it appeal as the 'guide to heaven' for anyone who reads it?

The other approach is to suppose that the Bible has nothing to contribute to theoretical reflection of any kind. It is a library of ancient books, culturally conditioned, addressed to primitive people. It can have no relevance to a scientific or technological culture.

Without going into all the reasons why the second approach cannot be correct, it is sufficient for our purposes to say that the price exacted for taking such a view is a steep one. For then the Bible can no longer seriously claim to express *truths*, except in the Pickwickian sense that they may be true-for-me, sentiments helping to satisfy certain personal, subjective needs and aspirations, but having no further claim upon the intellect. To limit the Bible's influence to the inculcation of subjective states in its readers must be unacceptable to the Christian.

What then? The true position must lie somewhere in between these two extremes. For if the Bible propounds truths about, say, the human person, then those truths, whatever they may be, will be consistent with anything in any of the special sciences which propound certain propositions about the human person as true (if they turn out to be true). An example may make this clearer.

The Bible propounds a view of the person as a psycho-physical complex, perhaps a psycho-physical unity. And so any science or philosophy which proposed or implied the elimination of either the psychic or the physical might expect to be received by the Christian with suspicion. However, a method of research which treated a person as a purely physical entity, might well be acceptable. So it is necessary to distinguish between the adoption of certain physicalistic methods by science, and the adoption of physicalism as a doctrine. A materialism, which denied consciousness, could not be acceptable, any more than a philosophical monism which denied the reality of matter would be.

Within such broad limits, and because of the previously mentioned theoretical underdetermination of Scripture, it is possible for the Christian who is a serious scientist, or a theoretician of a different kind, to do his work in good conscience. In such work he needs to keep his eye on the Christian theologian, as well as on the corporate testimony of the church, which may have relevant things to say about the exact extent of these broad limits.

The Philosophy of Science this Century

Accepting this general approach, what is there to be said about the Christian's attitude to the philosophy of science? I shall try to answer this question in two phases; first, by sketching some of the changes that have taken place in the philosophy of science in the last 75 years or so; and then, in the light of such changes, and bearing in mind the general approach, I shall try to offer an answer to the question.

Among philosophers of science there is an uneasy tension between, on the one hand, describing and codifying what scientists do, and on the other prescribing certain methodological ideals for scientists. This tension is made more acute than it might otherwise be by a second fact, that the natural sciences have, in the modern era, been amazingly successful. What scientists in practice do, therefore, cannot be all that far out. The thought that a philosopher might come along and tell scientists, in the face of this amazing success, that what they are doing is altogether wrong, could not be taken seriously. And so what the philosophers of science have tended to do is to exploit the difference between what scientists do, and what they *think* that they do. We shall return to this tension between the history of science, and the norms of science, later on.

The power and prestige of natural science (and also one source of alarm about science) lie in its ability to explain phenomena, to predict the outcome of experiments under controlled and repeatable conditions, and (most important of all) to control physical nature by adapting it to human needs. The key to this power lies in the ability of science to explain what is going on in terms of general laws.

But what is an explanation? Augustine said that he knew what time was until someone asked him, and a practising scientist may experience a similar reaction to this question about explanation. It is, of course, a typical philosopher's question. In fairly recent philosophy of science, there have been two overlapping but conflicting criteria of what counts as a scientific explanation.

A Formal Approach

The first criterion claims that explanations must have a certain logical form. If they have that form, and its requirements are properly executed, then this is both necessary and sufficient for a valid scientific explanation. Let us call this the *formal* approach to science. The second criterion relates explanation to understanding. We shall consider this in due course.

Granted that in order to be valid, scientific explanations must have a certain form, what ought that form to be? At this point opinions have differed markedly, and it will be useful to sketch some at least of the main types of answer to this question, and their strengths and weaknesses.

The first, both in terms of its historical influence and its intuitive appeal, is *inductivism*, associated with Francis Bacon (1561–1626), and more recently with J. S. Mill (1773–1836).[1] On this view scientific laws are inductive generalisations, the accumulation of the knowledge of regularities on the basis of extensive sampling. The power of science lies in its ability to predict on the basis on such knowledge. It is the job of the scientist to be assiduous and careful in the collection of such data, by noting negative instances, for example, and then to be circumspect in his predictions.

It is obvious that inductive procedures must play a role in science. But they cannot be the whole of the story. There are two main reasons for this. One is that, as David Hume notoriously pointed out, there is a logical problem about induction, at least if the aim, in conducting inductive generalisations, is to make reasonable predictions on inductive grounds. Suppose that it is said, on the basis of repeated experience, that it is probable that the next piece of bread to be eaten will not poison me. Since any such prediction transcends experience, going beyond the evidence upon which it is based, it is logically invalid to base an indefinite or general prediction upon such experience. For it is characteristic of scientific laws that they are universal or general in character, referring not merely to what has happened in the past, but also to what will happen in the future (and

1 See Mill's 'Methods of Experimental Inquiry' (*System of Logic*, (1843), III, viii).

also to what might have happened in the past). How can it be established, by inductive experience alone, that this sample of copper would have expanded if, ten minutes ago, I had heated it? It is invalid to answer this question by an appeal to a principle such as the Uniformity of Nature, since it is precisely that uniformity which is at issue.

There is another important difficulty with inductivism. To make sense, inductive procedures have to be undertaken in the light of some problem or interest. Mere curiosity is not sufficient to generate scientific laws; the curiosity has to be problem- or issue-specific. Otherwise why collect these data rather than those? There are infinite numbers of features of experience that it is possible to make generalisations about; the mere fact that such generalisations can be or are made is scientifically unilluminating. Science can only thrive on significant hypotheses.

For these and other reasons, but particularly because of the problem of induction, in the earlier years of this century a second kind of formalism became dominant among philosophers of science, that which viewed the form of scientific explanation as *hypothetico-deductive*. In Sir Karl Popper's words

> To give a *causal explanation* of an event means to deduce a statement which describes it, using as premises of the deduction one or more *universal laws*, together with certain singular statements, the *initial conditions*.[2]

So on this view a valid scientific explanation is one which consists of a hypothesis in the form of a general law or laws from which, together with a statement of the physical conditions under which those laws apply, it is possible to deduce that a particular event will occur. Thus 'All copper expands when heated' is a hypothesis. From that hypothesis, and the true sentence 'A is a sample of copper', together with assumptions about the background conditions under which the experiment would take place, it is possible to deduce 'A will expand when heated'.

This approach to the character of explanation has two distinct advantages over the inductivism that was sketched earlier. It rightly emphasises that science does not begin, logically, with observations but with theories. As was noted earlier, mere observation, without the support of a theory, is unilluminating. The other advantage is that it provides a logically stronger basis to prediction than mere inductive generalisation. For what could be stronger than deducibility?

But suppose that the predicted event actually occurs. Here the supporters of the hypothetico-deductive approach divide into those that support a *verificationist* approach, and those that are *falsificationist*. For the verificationist, the prediction has value as confirming or verifying the hypothesis,

2 Karl Popper, *The Logic of Scientific Discovery*, Routledge and Kegan Paul, London, (1959), p. 59. See also Carl G. Hempel and Paul Oppenheim, 'Studies in the Logic of Explanation', (*Philosophy of Science*), (1948), **15**, pp. 135–75. Reprinted in Carl G. Hempel, *Aspects of Scientific Explanation*, Free Press, New York, (1965), pp. 245–95.

while for a falsificationist, such as Popper, a theory is confirmed to the extent that strenuous efforts fail to falsify it. (This also provided Popper with what in his eyes is a solution to the problem of induction, for while generalisations cannot be conclusively verified, they can be conclusively falsified.) So in Popper's eyes good science consists in devising maximally stringent tests for theories. Scientific theories suffer a continuous trial by ordeal, and only the fittest survive. But, as the history of science has taught us, even the fittest only survive for a while, being in their turn replaced by theories better equipped to withstand the ever-burning fires of attempted falsification.

So scientific theorising improves by approaching an ideal limit ever more closely, though not ever achieving it. Science improves because the scope of the theories increases in the light of past falsifications of earlier theories.

However, the hypothetico–deductive approach brought difficulties of its own in its train, of which I shall mention only two. The first is that this approach requires that the *explanans* uses sentences which are law-like, universal conditional sentences containing no reference to individual things. For such explanations are of the form

(a) A is a sample of copper
(b) All copper expands when heated
(c) Therefore, A will expand when heated.

The general statement (b) is necessary if the argument is to be valid. But such explanations have been objected to on the grounds that they are too restrictive, since they rule out, for example, theories of planetary motion as scientific theories, since such theories refer to individual things, the planets. Here is another example of the uneasy relationship between scientific practice and the philosophy of science.

The second objection is that the hypothetico-deductive model does not fit statistical explanations in science, since the starting point of such explanations is not some universal law or other, but a knowledge of particular statistical correlations.

Besides these two criticisms, and others which there is not space to detail here, the hypothetico-deductive model is frequently criticised on the grounds that the observation sentences which are deducible from the theory cannot verify it, or falsify it, because any such sentences will invariably be infected by the character of the theory to which they related. In other words, the hypothetico-deductive model requires the drawing of an impossibly rigid distinction between theory and observation.

After all, if there is a conceptual link between theories and observations, then the 'theory' is likely to unfalsifiable not because it survives the fires, but because it is logically impossible to falsify because there is an analytic or conceptual link between theory and observed evidence. It is precisely for this reason that Popper has refused to regard Freudianism and Marxism as embodying scientific theories.

But not only must the distinction between theory and observation be sharply maintained (a point of major importance which will be considered later), the prospect of decisive falsification must also be emphasised. If a theory cannot be decisively falsified, then it cannot rank as a scientific theory, in Popper's eyes. Popper has been criticised on this point on the grounds that such a requirement runs counter to accepted scientific practice. For a scientist, faced with the possible falsification of his theory, is much more likely to seek to save it by providing minor amendments, riders and the like. Here again the relationship between scientific practice and philosophising about science surfaces. To what extent should a philosophy of science, Popper's or anyone else's, take notice of actual scientific practice? To what extent must a philosophy of science be normative?

So far we have swiftly reviewed a number of approaches to scientific explanation. Although there are sharp and important differences between these approaches, they are all sufficiently similar in their general approach for us to regard them as being members of the same family. For they are all endeavouring to uncover one particular form which all and only scientific explanations share. Only scientific explanations have this form because it is held that only such explanations—unlike, say, historical explanations, appeal to universal laws. More than this, each of these philosophies of science presupposes that objective scientific knowledge is attained by one or other process of successive approximation.

Explanation as Understanding

To see how similar the different approaches so far discussed are it is perhaps necessary to contrast them with the second main approach to explanation, which we may call explanation as increased understanding. Here one introduces an altogether different set of considerations from those we have been discussing, as will now be briefly indicated.

This second approach insists that the main thing about an explanation is that it should result in an increase in the understanding of the person to whom the explanation is offered. When we reflect upon the numerous every-day, non-scientific explanations which are given and accepted, which result in an increase in understanding, it is at least arguable that the approach to scientific explanation that we have so far considered is altogether too formalistic. For what advantage is it if an 'explanation' follows a correct logical form but does not provide an increased understanding of nature?

To maintain that a scientific explanation is only valid if it results in an increase in understanding is to relativise explanation to a particular population or culture. This is in effect a central feature of Thomas Kuhn's proposal in *The Structure of Scientific Revolutions*,[3] building upon earlier work of Stephen Toulmin and N. R. Hanson.

In brief, Kuhn argues that scientific theorising is not transitional or cumulative but that each scientific era is dominated, from a conceptual point of view, by a particular *paradigm*, a set of shared basic scientific beliefs about the world. Once such paradigms become established, as they do following a successful scientific revolution, then scientists conduct 'normal science' within the framework of understanding provided by that paradigm. They continue to do this until the accumulation of anomalies, together with a spark of scientific creativity, produces a discontinuity, a new paradigm. So, for example, it is possible to speak of the Copernican and Newtonian revolutions in science, for each of these movements heralded a novel framework of understanding (a 'paradigm') for one branch of physical science, within which normal scientific experimentation quickly became established.

So science is marked by discontinuity, not by gradual reform or progress through inductive accumulation but by successive revolution. Because of the centrality of the idea of a revolution in Kuhn's philosophy of science, three other consequences follow. The first is that scientific understanding (Kuhn eschews the idea of a scientific 'theory') is crucially conditioned by the total context in which people do science. Science is not something relatively abstract, detachable from the culture and non-scientific beliefs and expectations of those who do it; science is something that people do, in a particular intellectual and cultural context. Science, and therefore scientific explanation, partakes of something of the character of social activity. There are scientific communities, with dominant beliefs or paradigms of understanding, a scientific establishment giving prominence to the centrality of certain crucial experiments in the field, and there is an accepted way, within that community, of carrying forward scientific work ('normal science').

In later work Kuhn has backed off from talking of paradigms, acknowledging the ambiguity of the word as he used it, and prefers to speak of 'disciplinary matrices' or 'shared group commitments' held in common by a scientific leadership, and in terms of which teachers propound their discipline.

In the second place Kuhn places great store by the fact that the scientific operations which people conduct are 'theory laden'. The scientific mind is not a *tabula rasa* detachable from all the other beliefs, including beliefs about science, which a person, in a community, may have. It is in terms of these ideas that proposed developments in science either succeed or fail; succeed if they provide coherence with the other beliefs that a person already has, ('explanation as increased understanding') or fail if not.

Earlier we noted the importance that certain philosophies of science attach to the distinction between observation and theorising. Kuhn rejects that distinction. The decisive verification or falsification of any theory is

3 Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2nd edition, University of Chicago Press, Chicago, (1970).

not possible. Crucial experiments do not occur in the history of science. Rather what happens is that difficulties with a dominant paradigm are gradually disclosed as normal science advances; these are regarded as anomalies, to be set to one side or dealt with by the introduction of an auxiliary hypothesis. When the weight of such discrepancies becomes insupportable a scientific revolution, which will have the result of incorporating this recalcitrant experience into some new overarching understanding, cannot be far away.

In the third place Kuhn emphasises not only scientific discontinuity, the central importance of scientific revolutions, but he does this to the extent of claiming that one scientific paradigm, and the associated normal science, is *incommensurable* with any other scientific paradigm and its associated normal science. 'Incommensurability' does not refer chiefly to literal unmeasurability, but to a conceptual shift between one scientific era and the next. A scientific revolution occurs when one set of concepts *replaces* the former, dominant set in the way in which a new revolutionary junta might overthrow a former government. It is thus not simply that, say, Einsteinian physics represents a development or an extension of Newtonian physics, but it replaces Newtonian physics by providing an understanding of all that that physics explained, and more, using concepts which do not and could not have been employed within Newtonian physics.

So according to this view scientific rationality does not consist in the carrying out of certain procedures according to a certain logical recipe, but rationality must make reference to the social psychology of the scientific community, to their beliefs and overall cultural perspective.

While granting that there are very general standards in any scientific community, standards of 'accuracy, simplicity, fruitfulness and the like'[4] Kuhn places much greater store on the shared beliefs of a particular scientific community, and relativises scientific belief and understanding to that community. There is thus no such thing as scientific progress, if by that one means a linear progression.

The problem with asserting incommensurability in this uncompromising way is plain for all to see. If two scientific paradigms and the normal sciences associated with each are incommensurable, then it is impossible to make comparative judgements about them with respect to truth. It is impossible to say, for example, that the second paradigm, which has replaced the first, is truer than it.

Kuhn places a great deal of emphasis, in his development of a philosophy of science, upon the history of science. Yet is is hard to be convinced that incommensurability faithfully reflects actual scientific thinking and procedures. For one thing, the importance attached to the occurrence of anomalies in the normal science based upon one particular paradigm,

4 *ibid*, p. 199.

coupled with the realisation that the existence of sufficient of such anomalies prompts the search for a new paradigm, strongly suggests that such anomalies provide a conceptual bridge between new and old paradigms. For how can the second paradigm be recognised as eliminating the anomalies unless the existence of such anomalies at least makes sense in terms of the new paradigm? And if anomalies make sense from the point of view of both the new and the old paradigms, then these cannot be conceptually incommensurable.

Other philosophers of science, notably Paul Feyerabend[4], have gone to more extreme lengths than Kuhn, claiming that the history of science does not reveal anything that could remotely resemble what is confidently referred to by many as 'scientific method'. This is to take incommensurability to its logical conclusions. 'What remains are aesthetic judgments, judgments of taste, and our own subjective wishes'[5]. Clearly this will not do. Whatever the difficulty of formulating an adequate philosophy it is quite implausible to suppose that the whole of modern science, and the technology that it has spawned, is a series of subjective wishes or aesthetic judgements made by scientists.

We began this discussion by distinguishing between formalistic and non-formalistic approaches to the philosophy of scientific explanation. It is possible to regard Kuhn's account of the structure of scientific revolutions as carrying the non-formalistic approach to its limit. Apart from the most general phrases, scientific method cannot provide the scientist with any prescriptions or canons of scientific enquiry. All that it can do is to plot, in history, those ways of doing science which have produced coherent sets of beliefs in people at a particular epoch of human culture, and to see how these ways of doing science have been supplanted by others of a similar logical kind. So what, in effect, Kuhn has done is to substitute history, the actual course of scientific development, or at least his understanding of that history, for the development of standards of objective rationality for science, hard though it may be to develop such standards.

An Evaluation

Having briskly surveyed the dominant approaches in the philosophy of science during the last decades, we are now faced with the difficult task of making some kind of evaluation of them from a Christian point of view. The reader should be warned that the evaluation that follows is rather rough and ready; its aim is less to provide a definitive verdict than to draw attention to some of the criteria which, it is believed, should figure in a Christian evaluation of the philosophy of science.

In the material which we have been discussing the interplay between the actual history of modern science, and the development of a philosophy

5 Paul Feyerabend, 'Consolations for the Specialist' in Lakatos and Musgrave eds, *Criticism and the Growth of Knowledge*, Cambridge University Press, Cambridge, (1970).

of science, have been stressed. The fact of such interplay is hardly surprising. For modern science, despite what its detractors may say, is a success-story. Its methods can therefore hardly be completely misguided, and a historical study of those methods, as compared with those which prevailed in the pre-modern era, might be expected to reveal some clues as to a reliable and effective scientific method.

In this respect the situation is different from, for example, ethics. For it is certainly possible for a person to argue that the dominant theories of ethics, and even perhaps all influential ethical theories, are flawed in important respects. Some philosophers, such as Nietzsche, have done precisely this. The reason for this is that ethics, by definition, is the study of what ought to be the case, of what is of ultimate value. It is possible for someone to argue that all prevailing values are the wrong values, and to argue for a novel set. But with natural science it is different. Science is, by definition, concerned with interpreting the natural order, and an integral part of such interpretation is prediction, as we have seen. It would seem to follow from success in prediction, and in the construction of those new materials and structures (for example) that such predictability makes possible, that predictability is a *criterion* of scientific truth.

This does not mean that what works is true; but rather that if a theory is true, or to the extent that it is true, one would expect the predictions which follow from it to hold good. For when all is said and done the theory is meant to be a theory about the natural world.

However, it is one thing to say that the history of modern science points to the fact that its theories, and hence its methods of theory devising and testing, are not misconceived. It is quite another thing to say that what scientists have in the past done is the sole guide to what they ought to have done, and to what they ought to do in the future. The distinction between 'is' and 'ought' is crucially important here. The history of science deals with what is or was the case, with the facts of the development of modern science, an amazing set of changes. But such matters of fact are not of themselves logically sufficient to indicate what ought to be the case in science: the methods that ought to be encouraged and to be made to prevail.

It is for this reason that a question-mark must be raised over Kuhn's contribution. However accurate his account may be of scientific revolutions in the past (and we have suggested some reason to think that it cannot be wholly accurate), this accuracy could not, in and of itself, be prescriptive. While our beliefs may be theory-laden, or theory-infected, and while the distinction between theory and observation may be a hard distinction to maintain, nevertheless it is a distinction that ought to be maintained. For once it is surrendered one also surrenders the possibility of verifying or falsifying theories, and each theory become conceptually linked with its support, and conceptually severed from anything that would provide a *prima facie* refutation.

For illustration, consider the matter of bias in the law. The existence of such bias is notorious. But how are we to react to its presence? One reaction would be to surrender to it, and to argue that the dispensing of justice is inevitably biased. An opposite reaction would be to attempt to devise procedures and safeguards which would, as far as possible, preserve the administration of justice from bias.

And similarly with science. Because observations are frequently impure, or perhaps always impure, tainted with bias and misbelief, it does not follow that this is a state of affairs in which the scientific community ought to indulge. And indeed the development of scientific procedures, the experimental method, the language in which experiments are conducted and reported, represent so many efforts at minimising such bias. The success of modern science shows the relative success in eliminating such bias and bears testimony to the existence of an objective state of affairs (an objective creation, the Christian would say) which is partly and imperfectly codified in the stock of accepted scientific laws.

Such laws cannot be simply an expression of our culture and nothing more, a matter of collective taste. Anyone who flies in an airplane, for example, entrusts himself to a machine which is the product of modern science, the performance of which (and its occasional catastrophic departures from such performance) are objectively measurable. It would be laughably unrealistic to suppose either that the invention of the airplane was a matter of collective decision, or that it was a matter of practical common sense divorced from scientific theory.

Thus the relativism that Kuhn espouses in an effort to stress the distinctiveness of different scientific epochs defeats any attempt at providing one account of scientific rationality, and leads inexorably, and with perfect consistency, to the extreme conventionalism of Feyerabend.

One feature of any philosophy of science which ought to prove attractive to the Christian is thus that it does justice to the basic objectivity of science, and does not allow that objectivity to be dissolved by treating the philosophy of science as history, and as social history at that.

Another feature that, it is suggested, ought to weigh with the Christian is the ability of any philosophy of science to do justice to the *incrementalism* of modern science. Science is not a series of discrete developments, but it is perfectly natural, given its history, to talk in terms of the *development* of modern science. For the Christian such a development is natural and something to be expected, as men and women come to understand more and more about the one, integrated creation.

But it is doubtful whether, in attempting to answer the question at the heading of this article, it is possible to go further than that in arbitrating between the competing philosophies. The reason for this is simple, namely that while Scripture endorses the fact that the physical universe is objective, and thus that the methods of investigation should endeavour, as far as possible, to match that objectivity, it gives no guidance beyond that as to which of the philosophies of science which respect such objectivity is the preferred one. In the search for an answer to this further question one must be guided by the strengths and weaknesses of the competing philosophies themselves.

Paul Helm is Reader in Philosophy at the University of Liverpool, where he has taught since 1964. He has recently published *Eternal God* (Oxford: Clarendon Press, 1988).