SCIENCE AND PSEUDO-SCIENCE: THE CASE OF CREATIONISM

by R. G. A. Dolby

Abstract. The paper reviews criteria which have been used to distinguish science from nonscience and from pseudo-science, and it examines the extent to which they can usefully be applied to "creation science." These criteria do not force a clear decision, especially as creation science resembles important eighteenth-century forms of orthodox science. Nevertheless, the proponents of creation science may be accused of pious fraud in failing to concede in their political battles that their "science" is tentative and tendentious and will continue to be so while it remains archaic and poorly integrated into the rest of science.

Keywords: creationism; demarcation criteria; science and pseudo-science.

Part of the argument over creationism is whether there can be a creationist science to set alongside conventional science based on the theory of evolution. My concern in this paper will be to look at the criteria which have been put forward to distinguish science from non-science and from pseudo-science, and to see the extent to which they can usefully be applied to the case of creationism. Clearly, creationism is more than just a candidate for science. It is a repertoire of religious viewpoints with a wide popular base and significant political power, especially in parts of the United States of America. Its recent publicity has come from the campaign to get creationism taught as an alternative science in schools, which has been just as vigorously opposed by a lobby defending established science. The battle in the courts and elsewhere raised abstract issues normally confined to the philosophy of science on the demarcation of science from nonscience.[1] The defenders of creation argue that, if evolutionary theory can be regarded as a science,

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[*Zygon*, vol. 22, no. 2 (June 1987).]
 ISSN 0591-2385

then so can creation science;[2] their opponents sought ways of demonstrating the unacceptability of creationism as science.[3] Many times in the past, the supporters or critics of quasi-scientific movements have felt the need to argue about whether a body of belief or a set of research practices should be accepted as science. In such arguments, they have turned to philosophy of science, because it is the modern successor to the attempts by philosophers at the time of the scientific revolution to find a secure base for knowledge in general and scientific knowledge in particular. This paper is a discussion of such attempts to apply the demarcation issue to practical social purposes. My concern will be to abstract the scientific dimension of creationism from the wider social movement, and to explore as objectively as possible the question of how we can decide whether or not creationism is science.

I will not pay special attention in this paper to the question of what science is being demarcated from. Distinctions have been offered in terms of a contrast between knowledge and error, between objective knowledge and mere prejudice, between science and the meaningless, between science and metaphysics, and between science and pseudo-science. All of these distinctions have put science on the side of the epistemologically sound and opposed it to something less sound. I will, however, begin by reviewing some general preconceptions about science which affect the application of demarcation criteria.

Science has been regarded traditionally as a form of organized knowledge, and attempts to identify what is distinctive about science have examined the nature and epistemological basis of that knowledge. In the centuries since the scientific revolution, we have also applied the word *science* to the activity by which that knowledge is constructed. Modern attempts to demarcate science frequently assume that scientific knowledge is fallible and subject to revision; so rather than find an especially authoritative foundation for scientific knowledge, they look for distinctive features of scientific procedure, usually in terms of a conception of scientific method. However, it is not possible to divide demarcation proposals neatly into those directed to science-as-product and those directed to science-as-process, as earlier philosophies of science assumed the ideal method for science to be identical with the procedures by which we establish scientific conclusions as knowledge. The two move apart only in those modern philosophies which distinguish sharply between the creation and justification of scientific knowledge.

The twentieth-century interest is, then, more directed to the practice than the produce of scientific activity. In this new context, it is advisable to make some more distinctions. First, we should note that rather different discussions of the demarcation of science result if the main

attention is directed to the aims of the activity or to the methods which are followed. It is Marxist writers who have paid most attention to the proper aims of quasi-scientific activities, regarding work which tends to advance the interests of one section of society against the interests of the wider society as pseudo-scientific.[4] Later in this discussion I will develop some arguments which question the aims of creationists. However, the mainstream of the English-speaking philosophy of science tradition has assumed that there is no argument that science aims at the advancement of knowledge, and attention has been directed to the proper methods by which that aim may be fulfilled.

A second preliminary distinction of use in considering the demarcation of scientific activity arises because philosophical discussion is conducted in the presence of a well-established and remarkably successful tradition of scientific practice. Philosophical criteria are normally *prescriptive*: to the extent that an actual example of science fails to meet the criteria offered, that implies a criticism of the practice. However, it is sometimes suggested that this is presumptuous of philosophers. The best guide to how science should be conducted is to look at the practice of what is generally agreed to be the best of science. This has led to *descriptive* demarcation criteria.

Prescriptive Demarcation Criteria

In this section a representative sample of prescriptive demarcation proposals is listed and explained. Little philosophical criticism is offered, but the discussion goes on to show some of the difficulties of using such criteria in practical contexts.

Prehistory of the Issue

Modern theories of knowledge in the period of the scientific revolution sought to eliminate the sources of error by which belief was so often corrupted and to find a new more rigorous basis for knowledge in general and for natural philosophy in particular. René Descartes took the view that knowledge is based on clear and distinct ideas held in the mind, and expected all of science to be constructed out of them with the aid of pure reason. The British empiricist tradition held that knowledge is built out of ideas properly based in experience. One aspect of the arguments in the theory of knowledge up to the end of the eighteenth century was conflict between rival conceptions of the proper role of reason and experience. Since the early nineteenth century, natural science has been increasingly accepted as not just the label of a branch of knowledge but also as a successful practice which generates knowledge with a special epistemological status. Philosophy

of science has emerged as a separate branch of philosophy, concerned, in part, with finding what is rationally distinctive about science. The philosophers of science in their search for understanding of the nature of science, accepted its special epistemological status and offered, with justifications, a succession of demarcation criteria. Those which follow have been offered since the beginning of the nineteenth century.[5]

1. Classification of the sciences. This was used by most pre-twentieth-century philosophers of science as a way of rationally representing what does and does not deserve to be included in the total fabric of organized knowledge.[6] The positivist Auguste Comte's famous classification of the sciences, for example, was concerned to legitimate a new science, social physics, by giving it a place. In contrast, the omission of psychology and political economy was appropriate to Comte's view that they did not deserve a place.[7] Creationism would not have done well in a positivist classification of the sciences, but, if the debate over legitimation were still conducted on such lines, the creationists could offer their own alternative classification.

Comte also introduced a three-fold distinction between theological, metaphysical, and positive stages of science. This did not function as a demarcation principle in his work, as he argued that it is only by developing through the stages of first explaining things in terms of gods and then of abstract entities behind appearances that we are able to build up a sufficient repertoire of knowledge to be able to limit ourselves to positive science and concentrate on the laws of succession and resemblance of phenomena. Later positivists did, however, develop demarcation principles based on the exclusion of theological and metaphysical ways of explaining things. By this standard creationism is clearly and unashamedly still in the theological stage of science.

2. Induction. Until this century, the dominant conception of scientific method was of deductive reasoning based on generalizations drawn inductively from a broad range of unbiased observation. This conception was frequently applied to arbitrate on what is acceptable as proper science, particularly in terms of the requirement that an initial basis of observations be collected without bias or selective attention to what supported a pet idea. Supporters of creationist ideas up to the nineteenth century found this view easy to accept. The inductive conception of science was influentially criticized by David Hume in the eighteenth century, so that the problem of induction became a classic issue in the theory of knowledge. Induction was, however, frequently applied as a rational standard for science in the nineteenth century, for example, to Charles Darwin's presentation of the theory of evolution.[8]

Observation in modern science generally presupposes a considerable amount of background theory, for example, about what can be ignored in the observational context and the theory of the instruments used. Since such theories can be challenged, the standard of neutral and unbiased observation has become increasingly difficult to apply in natural science. Another standard for science which has been widely invoked within inductivist and positivist conceptions is the view that science should be value-free.[9] If science is about matters of fact and what can be derived from them, then there should be no room for values. After all, as David Hume argued, you cannot derive "ought" from "is." However, it has repeatedly been argued that value-laden principles are unavoidable at every stage of every science,[10] and that such social sciences as economics and application-oriented disciplines as scientific medicine are strongly value-oriented.

A nineteenth-century variant on the inductive method was the hypothetico-deductive method which was advocated, for example, by William Whewell (1840). Whewell was influenced by the Kantian view that the mind actively constructs its perceptions rather than passively receiving observational impressions. His conception allowed some scope for guiding ideas in each science which went beyond what could be generalized from observation. Modern forms of the hypothetico-deductive view have become increasingly orthodox, particularly because inquiry in physical science is now so much guided by theory that a preliminary phase of neutral observation is largely irrelevant to scientific practice.

3. Conventionalism. The main alternative to induction and hypothetico-deduction at the beginning of this century was conventionalism.[11] This suggested that the principles guiding the conceptual unification of science were created by the agreement of scientists. Conventionalism tends to suggest that the demarcation of science should be left to the judgment of scientists, who draw upon such considerations as simplicity, elegance, and coherence. Such a criterion tends to favor the view of the scientific establishment, which at present has little sympathy for creationism.

Discussions of demarcation since the early twentieth century have often concentrated on rigorous rational reconstructions of science. Alternative approaches have focused upon different levels of analysis: concepts, propositions, theory, and research programs.

4. Operationism. P. W. Bridgman's system of operationism, first proposed in 1927, argued that every legitimate scientific *concept* should be tied to the unique set of operations by which it was to be observed or

measured (Bridgman 1927; 1950; 1959). If a concept, such as the Newtonian idea of absolute space, could not be so defined, then it was meaningless, and the theories based on it were to be rejected from science.

5. Logical positivism. Logical positivists proposed to demarcate scientific knowledge at the level of scientific *propositions*.[12] Under the influence of Ludwig Wittgenstein's view in the *Tractatus* that the meaning of any statement depends on the possibility of its verification, a rational reconstruction of science was proposed in which the truth of all reconstructed propositions could be established simply by showing their logical relationship to the basic (protocol) statements of sensory experience. A single, unified language would be produced, and all other sentences rejected as meaningless. Statements about God, for example, would be rejected as meaningless because they could not be established in such a rational reconstruction. The original standard of demonstration of the truth of all permitted statements proved to be too demanding and was later turned into a requirement in terms of confirmability.

6. Popper's falsificationism. Karl Popper proposed to demarcate science at the level of scientific *theories*. The principle he used was that all scientific theories should be falsifiable, and any theory which was unfalsifiable was metaphysics rather than science (e.g., Popper 1959; 1974; 1979). If the supporters of a theory tried to protect it from falsification, then their practice was pseudo-scientific.

7. Lakatos's methodology of scientific research programs. Imre Lakatos offered a development of Popper's theory of science which focused upon sequences of scientific theories, which he called *scientific research programs*. Because even good scientists tend to protect their theories from falsification and often seem justified in doing so, Lakatos suggested that we should judge what is properly scientific in terms of which program manages to increase its testable content over the long term.[13] Such a scientific research program is progressive.

8. Feyerabend and "anything goes." Paul Feyerabend has taken an extreme view of rejecting the exclusive application of any one of such criteria, preferring such slogans as that in scientific method; anything goes (e.g., Feyerabend 1975; 1978).

The above list is far from being exhaustive. There are, for example, many demarcation proposals designed to legitimate less well-

established alternatives to science. According to some Marxists the distinguishing feature of scientific procedure is the use of dialectical materialism.[14] Also, as I noted earlier, according to other Marxists we must deny the status of science to inquiry which is contrary to the general interest of society.

The fact that so many prescriptive philosophical criteria have been offered for the demarcation of science suggests that there are problems about establishing an objective standard by which we may make our judgments. If there were only one dominant demarcation criterion in philosophy of science, then it might seem quite powerful; but because there are many, each of which is a good basis for criticizing the others, none can be rationally decisive. A substantial proportion of present-day discussion of rational criteria for demarcation is in terms of Popper's falsifiability criterion, which might have been authoritative if it were the only criterion. However, in the presence of alternatives of such modern versions of induction as Bayesian probability and such critical variants from Popper as Lakatos, its persuasive power is far less.

Perhaps the problem would be solved if philosophers of science were to settle upon a procedure for rationally establishing the relative merits of demarcation proposals, but that is not at present available, and to set about looking for it would only take our interest one step further away from deciding practical cases.

The multiplicity of criteria I have offered might seem to be because I have ranged over the nineteenth and twentieth centuries. Perhaps the older criteria can simply be ignored because they are out of date. However, the history of philosophy of science is not simple progressive enhancement of truth and elimination of error. Philosophy of science is not practiced in a neutral context. The philosophers themselves very often seem to have been concerned to legitimate or to discredit marginal candidates for science, especially from social science. We have noted that Comte was explicitly concerned to legitimate social physics and to deny legitimacy to psychology and political economy. Similarly, in his treatise on induction J. S. Mill was explicitly concerned to set out a sound epistemology and logic of all reasoning that would be adequate as a basis for political and moral theory and action. That is, he wished to find a legitimate basis for social science (Mill [1843] 1975, Book 6). In Popper's autobiographical accounts of the development of his principle of demarcation, he tells us that he wished to provide a principle which would establish the scientific nature of Einstein's general theory of relativity, while denying the same status to Marxism, psychoanalysis, and Adlerian individual psychology (Popper 1974, 34).

This kind of philosophy of science has a clear ideological function. As a result, the development of subsequent discussions has been

shaped by more than purely rational factors. While academic philosophers continued their business of displaying their own cleverness by finding new and more subtle criticisms and failings of earlier ideas, those who wished to make ideological use of the demarcation criteria tended to mold them into intellectual weapons, to sharpen their cutting edges so that they could be more powerfully used in battles over legitimation. In doing so they have often had to gloss over the weaknesses of demarcation proposals exposed by philosophical discussion. Further, because philosophy of science is very often disseminated in this ideologically charged context rather than in the more neutral context of pure philosophy, there has been a tendency for philosophy of science to be distorted by exaggerations of its claims.

There is a sense in which the ideological use of imperfect demarcation criteria from philosophy of science actually causes them to become less useful. To the extent that a particular demarcation criterion becomes common currency, it inevitably loses some of its sharpness because many who seek legitimacy for their systems of belief or practice dress up what they do in terms of that criterion.[15] If we are doubtful about whether a case dressed up as science in terms of a demarcation criterion really deserves to be called science, we are encouraged to ask, "But is it *really* science?" Once it becomes appropriate to ask this question, the criterion has lost its coercive power. We are led to look more carefully at the way people can link a criterion to different presuppositions and so modify its intended application. Descartes's criterion of ideas held clearly and distinctly was used with vigor in his own work. But as a general criterion, it is too readily misused by others who claim that they do indeed hold clear and distinct ideas. The practical application of a criterion may show that although it applies to good science, it is not a sufficient criterion for good science.

I would like to suggest that philosophy of science is not at present capable of providing inescapable arguments on what can or cannot be permitted as science. The fact that it is so often used that way, especially in arguments within social science, has more to do with the great needs of the rival parties to make their views persuasive than it does with the decisiveness of the arguments philosophy of science can offer. I do not wish to draw the conclusion that this means that philosophy of science is useless in discussing the demarcation of science in practical cases. Philosophy of science does attempt to penetrate confusion, to get to distinctive qualities of the intellectual practices and products which are known as science. The kind of critical awareness that it constructs can raise the standard of any discussion of what deserves to be called science. The problem is merely that philosophy of science does not at present have the final answers.

Descriptive Demarcation Criteria

Those who have paid attention to the demarcation issue without being satisfied by the prescriptive criteria of philosophy of science have frequently taken the view that we should try to extract demarcation criteria from the past practice of natural science. For surely natural science has been and still is a peculiarly successful intellectual practice. Perhaps descriptive criteria can be extracted which can settle what deserves to be described as science. The sort of insights which have been produced include those in the list which follows. Traditional rational generalizations about requirements of scientific knowledge and of the procedures which produce it deserve inclusion in our list. These criteria are important in the sense that good science strives to satisfy each in a sensibly balanced way. None of them can be regarded as either necessary or sufficient for the status of science.

1. Scientific knowledge is, or strives to be explicit and open to public scrutiny. In contrast to crafts it is not primarily intuitive and tacit,[16] and in contrast to magical or mystical claims to knowledge it is not secretive or hidden behind deliberately obscure symbolism.[17] (Creationism seems to meet this criterion.)

2. It is coordinated, rather than fragmentary, bringing consistency, coherence, order, and simplicity to its content. (So is creationism.)

3. Although there are also formal sciences, the empirical sciences at least should be based on and seek to explain experience. Conflicts between observation and expectation should be reconciled. (This applies to creationism.)

4. The observations and measurements made should strive to maximize their precision and the range they cover. (Creationism is too immature and too limited in its resources to have made much progress on this criterion.)

5. The science should seek some generality of understanding. (Creationism does.)

6. The concepts used and the relationships constructed among them should be as rigorously formulated as possible, so as to minimize ambiguity and to facilitate checking the inferences drawn. (It is easy to be highly suspicious of the apparent obscurantism of creationism on this criterion.)

7. The science so constructed should have explanatory and predictive power. Some classical conceptions of science have assumed a deterministic framework within which science works towards a complete set of causal laws. The only limit on prediction would then be our own ignorance. Modern science (and quantum theory in particular) has moved away from causal determinism as a scientific ideal. (Creationism

seeks to explain and predict much, but it allows only a limited range of evidence to carry weight in criticism.)

8. Successful sciences should be capable of practical application to other sciences and to the rest of human practice. (The distinctive features of creationism do not seem to have any application, except in a religious context.)

9. There are some kinds of subject matter which successful science should avoid, in particular matters which have proved to be socially divisive, such as the theologically contentious or the politically divisive. Perhaps metaphysical, mystical, and nonnaturalistic approaches should also be excluded. (Creationism clearly fails to meet this criterion, but, as I have noted, none of these requirements can be regarded as necessary for science.)

More recent study of science has looked for further distinctive characteristics in the psychology and the sociology of scientific practice, and in modern professional accounts of the history of its development. Theories of knowledge between the seventeenth and nineteenth centuries very often linked knowledge construction with psychological processes in the human mind. This might suggest that the modern psychological study of science could reveal some of its distinctive qualities. The most sustained interest in the psychology of science has been focused upon the creative process. However, psychological accounts of scientific creativity have not tended to show that it is especially distinctive from other forms of creative activity, and many philosophers of science have argued against any special philosophical significance for the process of discovery. That possibility is not completely dead, however. Modern theories of science have turned away from a psychological conception of individual belief towards a conception of socially shared knowledge carried within a historical tradition and modified by institutionally controlled forms of innovation. In this new context, there has been descriptive interest in R. K. Merton's account of the value system of science and T. S. Kuhn's account of the social nature of normal and revolutionary science. Merton set out four moral norms, or institutionalized imperatives, which he argued were important for maintaining the autonomy and effectiveness of objective science.[18] The norms are that in science truth is universal (universalism), that the findings of science should be shared (communism), that scientists should behave in a disinterested rather than a partisan way (disinterestedness), and that judgment should be based on empirical and logical criteria (organized skepticism). These norms have been widely discussed, widely applied, and widely criticized.[19] They were originally presented in a demarcating context in order to suggest that politically committed quasi-scientific doctrines emerging in Nazi and Stalinist

totalitarian societies were not actually sciences. They are readily applied to creationism, but they do not give clear guidance. The supporters of creationism undoubtedly regard the truths they seek as universal and to be made available to all, but they are perceived by critics to be rigidly committed to reaching conclusions compatible with their religious preconceptions, rather than disinterested. They do practice organized skepticism, but in a highly selective way.

Kuhn's conception of science has been ever more fashionable over the last few years.[20] His account is of communities of normal scientists, in which past achievements are built upon as paradigms until the failure to resolve anomalies induces a sense of crisis. The crisis may be resolved by a revolutionary change to a new paradigm involving a conversion process analogous to the gestalt switch in which perceptions are suddenly organized in a completely new way. The conceptual tools Kuhn developed have been enthusiastically applied in many disciplines by those wishing to present their practice as sciences. In particular, Kuhn's account implied that a mature scientific discipline normally shares a single paradigm rather than several competing approaches. In the end, however, the enthusiasm with which the approach has been applied to art history and even to theology has forced the conclusion that, even if it can be shown to apply to mature science, it is very far from applying only to mature science.[21]

There are problems about extracting prescriptive principles from all these descriptive criteria. What happens to be true of past or present science may, for example, be true of other forms of social life as well and not be distinctive of science. So the discussion continues.

My conclusion is that the academic discussion of the demarcation of science from nonscience tends to find many respects in which science might be thought to be distinctive but none which is demonstrably decisive. Some criteria are too severe and exclude apparently successful science; other criteria are too tolerant and allow examples of nonscience. There is no useful way of combining them into a single scale. However, while dispassionate discussion fails to find much success in the search for a definitive demaraction criterion, the ideological pressures continue to need decisive cutting weapons to eliminate what is perceived as pseudo-science and to legitimate new activities as science.

Creationism

In the light of this general discussion, is creationism to be regarded as science or pseudo-science? Most of the people who have taken up this issue seem to have had strong ideological commitments for or against it. I have tried to avoid such taking of sides. I am quite prepared to

allow creationists to try to produce genuine science. If they succeed, I think it would in principle be quite a healthy development if children were not taught Darwinian science as unassailable dogma but were made aware that some of the fundamental ideas on which much of what they learn is built could conceivably be rejected by revolutionary changes in the further development of science, as has happened in the past. I think teaching science as a system which is open to revision would be a good idea in principle, but I also concede that present-day society might not be ready to take such a view at the level of elementary education. Furthermore, such a view might not be sufficient for the more extreme creationists who wish to create a closed system of science in harmony with their own religion. However, I am expressing these opinions merely to show that I am prepared to look at the scientific status of creationism without needing to legitimate a special interest. Thus the comments which follow are an attempt to contribute to discussion among those who wish to decide *fairly* on the scientific status of creationism.

On brief exposure to the doctrines of creationism,[22] I formed the impression that, in spite of its claims to be scientific, it does not display much substantive science which one can rationally scrutinize; rather it is a range of slightly varying revolutionary frameworks. Each of these is a set of externally imposed beliefs together with a set of ground rules, which constrain any acceptable system of creationist science that might be constructed or reconstructed out of the wreckage it seeks to make of present-day science. Science, each faction of creationism argues, must produce results which are compatible with its own reading of the Bible. Any science which fails by this criterion is attacked.

If creationism is not yet a fully formed body of possibly scientific belief, then demarcation criteria which are applied to systems of claimed knowledge are not appropriate. If it is not a fully worked out set of investigative techniques, then demarcation criteria which are applied to methods of investigation are not appropriate. The appropriate standards are those for a *philosophical* approach which is trying to generate an alternative science.

The principles which scientific creationism develops have the interesting feature that they are very similar to some of the doctrines proclaimed in the name of science in the century after the scientific revolution, in particular in the early eighteenth century.[23] That was a period in which Christianity was far more dominant in European thought, including scientific thought, and the Bible was taken far more literally. It was widely accepted that there is a close relationship between God's Word and God's work and that appropriate worshipful activity for a natural philosopher combines the two. The theories which

found favor were frequently designed to account for biblical events as the outcome of natural processes. Furthermore, it was a period in which scientific knowledge was likely to be limited to what could be directly demonstrated on the basis of immediate experience. Unnatural forms of experimentation, extended chains of mathematical reasoning, and instrumental extensions of observation were frequently looked upon with great suspicion. Creationism has retained the essentials of such views for more than two centuries. On a modern secular view, creationism looks like a theory-based science, for every remark is interpreted on creationist assumptions which noncreationists can challenge. However, in the early eighteenth century, such a religious view was natural and common sense, so that its theory-ladenness was not apparent.

As one who has a professional interest in the history of science, I do not wish to say that those eminent natural philosophers of the seventeenth and eighteenth centuries who produced syntheses like those of modern creationism were doing pseudo-science. Further, if such an activity was describable as science then, there is a case for describing it as science now. So whatever may be wrong with creationism, I do not wich to say that *it* is pseudo-science, either. I am inclined to the view that it is archaic science functioning as a framework for criticism of science.

Even by the early nineteenth century, the kind of fundamentalist science that modern creationists advocate was looking increasingly old-fashioned. For example, consider the following quotation from a reviewer of an early-nineteenth-century book holding a view very much like modern creationism. "From a statement and ratiocination, on the whole flimsy, defective, declamatory, illogical, and ill-founded, we can only lament that the worthy author appears to have been born an age too late. He would have adorned the first meetings of the Royal Society; or at a somewhat later period have been a fit coadjutor of Ray and Derham—able and excellent men indeed, like Mr. Gisborne; but who had a merit which he does not possess, that of having availed themselves of all the lights which their own age afforded" (Anon. 1819, 48-49). This judgment is rather like the one I am making on creationist science one and a half centuries later.

Science has changed drastically in the centuries since the scientific revolution. Many specific doctrines, such as evolutionary theory, have been added. The achievements of science have been reworked in each scientific revolution so that they have cumulated into a relatively coherent and interconnected system which interlinks scientific ideas acceptable to creationists with others which are not. The whole system is so vast that it can only be managed within a complex and highly segmented institutional structure. Furthermore, the techniques of in-

vestigation and demonstration have become vastly richer and more complex. This too has been coped with by the institutional changes associated with professionalization. Even at the level of rational procedure, the changes are pronounced. For example, testimony based on direct observation has lost its privileged position in the sense that instrumentally aided observation and measurement are the normal empirical base for science; and every part of science, including observation, is now far more theory-laden. In addition to these changes, the dominant conception of science has changed from a limited body of knowledge contained with a religious framework to an open-ended process of knowledge elaboration conducted by professionals who admit no limits on what their procedures may produce as knowledge.

My primary concern is to look at the techniques by which we may decide on the status of creationism as a science today. What I have already said indicates that it might well be a science by the standards of the early eighteenth century. Therefore, we have a choice of strategy. One possibility is to say that, as it is a piece of eighteenth-century science, it is still a science but a peculiarly isolated and archaic science. After all, when philosophy of science tries to establish distinctive rational criteria by which acceptable science may be identified and to which good science should conform, these criteria should not just hold for present-day science but for all science, past, present, and future. In particular, they should hold for that part of earlier intellectual activity which we accept as science. Alternatively, we can say that the standards by which we are to judge what is or is not a science vary over time. Then we should conclude that by modern standards, creationism is not a science. However, if the standards of science can vary with time, then it is possible that they will vary again, and even possible that changes in the political power of fundamentalist religion might force a context in which creationism is to be judged a science. That is, if we take a historical relativist position, the judgment that creationism is not a science would be a judgment of the historical moment and would have less prescriptive force.

Therefore, either the demarcation criteria of philosophy of science should accept creationism as genuine science, or they should include some provision for changes over time in the standards of what is to count as science.[24]

My own preference is, as I have made clear, that creationism is a kind of science. Science is not simply whatever the dominant mode of culture declares to be science. Science must be organized knowledge that meets rational standards, and scientific procedure must be capable of generating new knowledge by those standards. If a culture adapts science to its own image, the result is only science if it is such a growing

repertoire of knowledge. Relativism has its limits. Creationism is a science, then, because it is an archaic framework within which post-eighteenth-century knowledge is to be reconstructed, with the exclusion of all those aspects which are contrary to the prevailing biblical interpretation, on the principle that many of the new knowledge-constructing techniques which have augmented orthodox science since the eighteenth century are to be challenged. Such science, if it became widely practiced, would be a peculiar science but not an impossible science.

I would like to bring out one other kind of problem about creationist science which has to do with the aim of those who practice it. In addition to being archaic science, it is also an especially corrupting form of science. My suspicion is that present publications are to some extent fraudulent, a kind of pious fraud. I have recently been looking at the possibilities of fraud within science. I was especially interested in the possibilities of fraud at the level of theory, the kind of theory which people might take seriously because they want it to be true rather than because the evidence and argument appear to lead to the best judgment being that it is true. The heritability of race or class differences in IQ might be an example. I think that creationism is another. The process of the evaluation of knowledge-claims in science is drawn out, and an increasing degree of commitment to the theory is required if the effort of continuing with the evaluation is to be continued. At each stage, it is possible to exploit the status of the ideas one is negotiating to advance nonscientific interests; and, if the ideas are represented as being more fully established than they really are or than others accept, then that representation can be seen as fraudulent. In the case of creationism, the scientific version of the doctrines has been represented as being a "model" (Morris 1974, chap. 1)—in a nonstandard sense of model—and as carrying less epistemological status than a fully testable theory but a comparable status to the doctrine of evolution. Creationists vigorously criticize key features of evolutionary theory in a manner that looks spuriously authoritative. Defenders of evolutionary theory argue back that their doctrine is far more securely established than the creationists imply, but the point I wish to bring out is that the purpose of the creationists is not simply to propose a theory for impartial consideration but to give their doctrine sufficient status to license it to be taught in schools. Furthermore, their reason for wishing to teach it in schools is not to emphasize its highly unusual and tendentious nature as science but to inculcate belief among those children inclined to take the Bible as literal truth. This, I would suggest, is a fraudulent use of science, and creation science might be labelled as corrupt science because it is designed to facilitate this use.

Therefore, although creationism can be defended as a kind of science, philosophical scrutiny shows it to have a rather peculiar status, for it is a very preliminary form of science which happens to be both archaic and corrupting.

NOTES

1. See, for example, the "Opinion" by Overton [1982] and the news report of the Arkansas trial in *Science* (1982). *Science* reports that Overton devoted thirteen pages of his thirty-eight-page decision to demonstrating that, in his opinion, creation science is not science.

2. So that, for example, Morris (1974) was written to be used as a school textbook presenting the creationist alternative to evolutionary theory.

3. See, for example, Ruse (1982), Kitcher (1983), and Godfrey (1983).

4. Several publications by Hilary and Steven Rose have taken such a line. For example, their conclusion to a critique of neurobiological reductionism says that it is bad science, in part, "*because* it is ideological, that is its research programme and organising paradigm are permeated with those 'ruling ideas' which express class interest, and that the technologies they generate are essentially defensive of that class interest, serving to protect it both physically, by manipulating and pacifying would-be protesters, and ideologically, by providing an apparent biological justification for the social order" (Rose & Rose 1976, 116).

5. "Demarcations focussing on *science* have been of prime importance since Kant" says W. W. Bartley III (1984) in a useful discussion of the demarcation problem.

6. For my own discussion of this almost forgotten practice, see Dolby (1979).

7. Comte (1830-42). For the argument rejecting psychology in favor of phrenology, see vol. 3, 761-845, and for the rejection of political economy in favor of social physics, see vol. 4, 264-86. See also H. Martineau's English summary in Comte (1974), 380-98 and 446-50.

8. For discussions of the relationship between Darwin's theory and nineteenth-century methodological writings, see, for example, Ellegård (1957) and Feibleman (1959).

9. The issue is discussed with reference to social science by Nagel (1961), 482-502.

10. Examples of this kind of argument appear in the papers by R. Rudner, R. C. Jeffrey, and I. Levi reprinted under the theme "The Acceptance of Scientific Theories," in Brody (1970), 539-70.

11. Two of the twentieth-century conventionalists who have had lasting influence are H. Poincaré and P. Duhem. See, for example, Poincaré [1902] (1952), [1905] (1958), [1908] (1914) and Duhem [1914] (1954).

12. For a general account of the logical positivist movement, see the introduction to the useful collection of some of its papers by A. J. Ayer (1959).

13. Lakatos's key methodological paper was Lakatos (1970). He also gave an informal presentation of his views in a radio talk. See Lakatos (1981).

14. Dialectical materialism was offered as a theory of scientific method by F. Engels [1927] (1946). For a popular uncritical account see Conze (1935). For an account developed in response to Popper's critique of Marxism, see Cornforth (1977).

15. For example, E. O. Wilson explicitly set up his new science of sociobiology in Popperian terms, using this as a means of distancing it from the more popular works linking animal and human social behavior which had previously been published (Wilson 1975, 27-31). I discuss this point in Dolby (1982).

16. The tacit component cannot, of course, be avoided completely. See, for example, Polanyi (1958) and Ravetz (1971), Part II.

17. Ziman (1968) is a book organized around the idea that science is open to public scrutiny.

18. Originally published in 1942, the key discussion is most readily available in Merton (1968 and 1973).

19. See, for example, Zuckerman (1977). This applies the Mertonian conception to marginal science.

20. The key text is still Kuhn (1970), supplemented by Kuhn (1977).

21. See, for example, Kuhn's comment in Kuhn (1977), 341. Another well-known example of the limitations of Kuhn's ideas as a demarcating principle is P. K. Feyerabend's suggestion that they would apply without difficulty to organized crime (Feyerabend 1970).

22. I have paid special attention to Morris (1974), Gish (1979), and Whitcomb and Morris (1978).

23. A general account of the relevant aspects of science and religion in the period is given in Westfall (1973).

24. The problem of whether and in what respects to regard science as a changing or unchanging target for demarcation criteria is discussed in Amsterdamski (1975).

REFERENCES

Amsterdamski, S. 1975. "Troubles with the Problem of Demarcation," chap. 2 of *Between Experience and Metaphysics: Philosophical Problems of the Evolution of Science*. Dordrecht, Holland: Reidel.

Anonymous. 1819. Essay Review of T. G. Gisborne, *The Testimony of Natural Theology to Christianity*. *Quarterly Review* 21:48-49.

Ayer, A. J., ed. 1959. *Logical Positivism*. Glencoe, Ill.: Free Press.

Bartley, W. W. 1984. "Logical Strength and Demarcation." In *Rationality in Science and Politics*, ed. G. Andersson, 69-93. Dordrecht, Holland: Reidel.

Bridgman, P. W. 1927. *The Logic of Modern Physics*. London: Macmillan.

_____. 1936. *The Nature of Physical Theory*. Princeton, N.J.: Princeton Univ. Press.

_____. 1950. *Reflections of a Physicist*. New York: Philosophical Books.

_____. 1959. *The Way Things Are*. Cambridge, Mass.: Harvard Univ. Press.

Brody, B. A., ed. 1970. *Readings in the Philosophy of Science*. Englewood Cliffs, N.J.: Prentice-Hall.

Comte, A. 1830-42. *Cours de Philosophie Positive*. 6 vols. 2d ed. Paris: Bachelier.

_____. 1974. *The Positive Philosophy of Auguste Comte*. New York: AMS Press.

Conze, E. 1935. *The Scientific Method of Thinking: An Introduction to Dialectical Materialism*. London: Chapman & Hall.

Cornforth, M. 1977. *The Open Philosophy and the Open Society: A Reply to Sir Karl Popper's Refutations of Marxism*. 2d ed. London: Lawrence and Wishart.

Dolby, R. G. A. 1979. "Classification of the Sciences: The Nineteenth Century Tradition." In *Classifications in their Social Context*, ed. R. F. Ellen and D. A. Reason, 167-93. London: Academic Press.

_____. 1982. "On the Autonomy of Pure Science: The Construction and Maintenance of Barriers between Scientific Establishments and Popular Culture." *Scientific Establishments and Hierarchies, Sociology of the Sciences Yearbook* 6:267-92.

Duhem, P. 1954. *The Aim and Structure of Physical Theory*. Princeton, N.J.: Princeton Univ. Press.

Ellegård, A. 1957. "The Darwinian Revolution and Nineteenth Century Philosophy of Science." *Journal of the History of Ideas* 18:362-93.

Engels, F. 1946. *The Dialectics of Nature*. London: Lawrence and Wishart.

Feibleman, F. K. 1959. "Darwin and Scientific Method." *Tulane Studies in Philosophy* 8:5-14.

Feyerabend, P. K. 1970. "Consolations for the Specialist." In *Criticism and the Growth of Knowledge*, ed. I. Lakatos and A. Musgrave, 197-230. Cambridge: Cambridge Univ. Press.

_____. 1975. *Against Method: Outline of an Anarchistic Theory of Knowledge*. London: New Left Books.

_____. 1978. *Science in a Free Society*. Manchester: New Left Books.

Gish, D. T. 1979. *Evolution? The Fossils say NO!* San Diego, Calif.: Creation-Life Publishers.

Godfrey, L. R., ed. 1983. *Scientists Confront Creationism*. New York: Norton.

Kitcher, Philip. 1982. *Abusing Science: The Case Against Creationism*. Cambridge, Mass.: MIT Press.

Kuhn, T. S. 1970. *The Structure of Scientific Revolutions*. 2d ed. Chicago: Univ. of Chicago Press.
_____. 1977. *The Essential Tension: Selected Studies in Scientific Tradition and Change*. Chicago: Univ. of Chicago Press.
La Follette, M. C., ed. 1983. *Creationism, Science and the Law: The Arkansas Case*. Cambridge, Mass.: MIT Press.
Lakatos, I. 1970. "The Methodology of Scientific Research Programmes." In *Criticism and the Growth of Knowledge*, ed. I. Lakatos and A. Musgrave, 91-195. Cambridge: Cambridge Univ. Press.
_____. 1981. "Science and Pseudo-Science." In *Conceptions of Inquiry: A Reader*, ed. S. Brown, J. Fauvel, and R. Finnegan, 114-21. London: Methuen.
Merton, R. K. 1968. "Science and the Democratic Social Structure," in his *Social Structure and Social Theory*. 3d ed., 604-15. New York: Free Press.
_____. 1973. "The Normative Structure of Science," chap. 13 of his *The Sociology of Science*, 267-78. Chicago: Univ. of Chicago Press.
Mill, J. S. 1875. *A System of Logic, Ratiocinative and Inductive*. 2 vols. 9th ed. London: Longman.
Morris, H. M., ed. 1974. *Scientific Creationism*. General ed. San Diego, Calif.: Creationism-Life Publishers.
Nagel, E. 1961. *The Structure of Science: Problems in the Logic of Scientific Explanation*. New York: Harcourt, Brace & World.
Overton, Judge William R. [1982]. "McLean v. Arkansas. Opinion." In *Creationism, Science and the Law: The Arkansas Case*, ed. M. C. La Follette, 45-73. Cambridge, Mass.: MIT Press.
Poincaré, H. 1914. *Science and Method*. London: Nelson.
_____. 1952. *Science and Hypothesis*. New York: Dover.
_____. 1958. *The Value of Science*. New York: Dover.
Polanyi, M. 1958. *Personal Knowledge*. London: Routledge & Kegan Paul.
Popper, K. R. 1959. *The Logic of Scientific Discovery*. London: Hutchinson.
_____. 1974. *Conjectures and Refutations*. 5th ed. London: Routledge & Kegan Paul.
_____. 1979. *Objective Knowledge*. Rev. ed. Oxford: Clarendon Press.
Ravetz, J. R. 1971. *Scientific Knowledge and its Social Problems*. Oxford: Clarendon Press.
Rose, Hilary and Stephen Rose. 1976. "The Politics of Neurobiology: Biologism in the Service of the State." In *The Political Economy of Science*, ed. H. and S. Rose, 96-111. London: Macmillan.
Ruse, Michael. 1982. *Darwinism Defended: A Guide to the Evolution Controversies*. Reading, Mass.: Addison Wesley.
Science. 1982. "Judge's Ruling Hits Hard at Creationism." *Science* 215:381-84.
Westfall, R. S. 1973. *Science and Religion in Seventeenth-Century England*. Ann Arbor: Univ. of Michigan Press.
Whewell, W. 1840. *Philosophy of the Inductive Sciences*. 2 vols. London: Parker.
Whitcomb, J. and H. Morris. 1978. *The Genesis Flood*. Grand Rapids, Mich.: Baker.
Wilson, E. O. 1975. *Sociobiology: The New Synthesis*. Cambridge, Mass.: Harvard Univ. Press.
Ziman, J. 1968. *Public Knowledge: An Essay Concerning the Social Dimension of Science*. Cambridge: Cambridge Univ. Press.
Zuckerman, H. 1977. "Deviant Behavior and Social Control in Science." In *Deviance and Social Change*, ed. E. Sagarin, 87-137. Beverly Hills, Calif.: Sage.