Reviews

The Great Devonian Controversy: The Shaping of Scientific Knowledge among Gentlemanly Specialists. By MARTIN J. S. RUDWICK. Chicago: University of Chicago Press, 1985. 494 pages. $39.95.

This book is destined to become a classic. Quite simply, it is one of the most original and powerful pieces of work in the history of science to have been written since the Second World War. Setting new standards in the field, it is undoubtedly essential reading, not only for anyone who wishes to know how the geological column, that centerpiece of the historical sciences, was put together in the first half of the nineteenth century, but also for anyone who wishes to come to a mature understanding of what, exactly, science is, how, exactly, it works, and why, exactly, it generates authoritative and generally reliable knowledge about the natural world.

Martin J. S. Rudwick is professor of the history of science at Princeton University. Originally a geologist, he turned to the history of science only after having first distinguished himself in the field of paleontology. This is significant, for even after more than two decades away from rocks and fossils, Rudwick clearly retains a sharp sense of the realities of geological research. As Stephen Jay Gould has noted in an earlier review (New York Review of Books, 27 February 1986), the fact that the history of science is often (though not, as Gould suggests, "usually") written by scholars who have never practiced the art of doing science itself can impose "a subtle emphasis on theories and ideas over practice." The history of science is rapidly shedding this particular prejudice; but Rudwick is one of those who has led the way. His work conveys a sense of the state of the art—rocks, fossils, maps, and so forth—that will be appreciated as much by working geologists as by bookish historians.

The Great Devonian Controversy is a 450-page analysis of a single, apparently fairly minor, "episode" (the word is hopelessly inappropriate) in the history of geology. The episode itself occurred in the years 1834 to 1842, when most leading British geologists, as well as other major figures working elsewhere in Europe, became caught up in a controversy concerning the stratigraphical significance of the so-called greywacke rocks of Devonshire. The bulk of Rudwick's book consists of an extremely fine-grained narrative account of this controversy, based upon what appears to have been an exhaustive (and exhausting!) examination of the very detailed documentary evidence that is available in published sources and manuscript archives. Rudwick invites his readers to follow the course of the controversy in great detail, without the benefit of hindsight (only geologists and historians of geology are likely to know in advance exactly how the conflict was eventually resolved), and in terms that would have been readily understandable to those who were actually caught up in the events themselves.

Superficially, at least, this is an unpromising basis for a classic work. In order to stay with Rudwick's central narrative, the reader must be willing to become totally immersed in the geological world of the early nineteenth century—in its

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institutions and social networks, in its internal standards and codes of conduct, in its personalities (great and small), in its theoretical principles and empirical knowledge, and, last but not least, in its practical skills and research methods. This, to all but a narrow group of specialists, is a considerable challenge. For page after page, the general reader must struggle to keep track of a debate which involves ten principal actors, a multitude of personal, social, and technical issues, and a whole series of interacting and subtly varying theoretical positions within the controversy itself. Numerous maps, charts, and figures are an indispensable aid in this tracking effort.

All this detail appears at first sight to count against the wider appeal and significance of the book. Yet I thoroughly recommend that potential readers not be thwarted by the prospect of some hard work, for the rewards to be gained by it are great. Rudwick's purposes are altogether larger than the furthering of our understanding of the history of geology. In documenting a single episode in such detail he aims, as he states it (quoting the anthropologist Clifford Geertz), to make "small facts speak to large issues." In this case, the large issues involve the nature of science itself; and central among these is the question of how the social reality of science as a cultural activity can be squared with that other, and more familiar, intellectual reality of science as a body of (generally reliable and steadily accumulating) knowledge about the natural world. The past few years have seen the publication of an almost bewildering variety of programmatic statements on this and related abstract questions; but if Rudwick has a single central message, it is that these questions can only be sensibly addressed with the help of the most scrupulously careful and microscopic examinations of concrete scientific practice.

Rudwick has chosen a novel arrangement for his ambitious project. His book is divided into three parts. Part 1 discusses the appropriate methods for putting science under a "historical microscope," and it introduces early-nineteenth-century geology as a world dominated by "gentlemanly specialists." Part 2, the bulk of the book, presents an account of the controversy itself. Rudwick holds nothing back, and only his considerable skills as a narrative historian prevent the major sweep of events from being totally lost in the mass of historical detail. Finally, Part 3 rescues the reader from the particulars of history by first reconstructing the overall shape of the controversy and then drawing from it a series of general conclusions concerning the nature and status of scientific knowledge.

In order to appreciate the major thrust of Rudwick's analysis, it is necessary to summarize very crudely the narrative account contained in Part 2 of the book. This begins by conveying enough about the state of geological knowledge in the early 1830s to enable us to understand why a claim by the English geologist Henry De la Beche in 1834 that he had discovered in the greywacke rocks of Devonshire plant fossils closely similar to those already known from the coal measures of south Wales and elsewhere was seen as worryingly anomalous. (In a nutshell, the claim was anomalous because the greywackes were regarded as being very much older than the coal measures, dating from a time well before terrestrial life had become established.) De la Beche's claim was immediately rejected by the wealthy Scottish gentleman-geologist Roderick Impey Murchison, who, without even having seen the relevant rocks and fossils, declared that his English colleague had made a mistake: being vastly younger, the plant-bearing rocks must lie above (and not within) the greywacke. Moreover, since the two deposits were remote in time from one another, they must be separated by an "unconformity," that is, a temporal gap in deposition.

The issue was far from trivial. The reconstruction of the temporal sequence of earth history lay at the heart of the geological enterprise of the early nineteenth century. Central principles in this enterprise (such as the use of fossils as a criterion in estimating the relative ages of rocks) were at stake, and so too were the reputations of some central representatives of the geological community. Clearly, the scene was set for a major controversy.

Once De la Beche and Murchison had set out their initial positions, others quickly joined in on one side or the other; and over the coming years, as private and public debate got under way, as alliances were made, broken, and then remade, as new field and museum work was undertaken, and as new evidence and new ideas were generated, the positions of the two sides were gradually transformed. In the end, neither of the original positions in the dispute survived: for his part, De la Beche was forced to concede that his plant-bearing rocks did indeed lie above the older "Silurian" deposits of Devonshire, as Murchison had suggested; but for his part, Murchison was forced to concede that De la Beche's material was not separated from the Silurian deposits by any major unconformity. Instead, a consensus emerged around the entirely novel idea that the plant-bearing rocks of Devonshire belonged to a major and previously unrecognized period of earth history: sandwiched between the older Silurian and the younger coal measures, the "Devonian" became a recognized part of the geological column.

Thus summarized, the events detailed in this book appear relatively simple and straightforward. That, of course, is the trouble with summaries; they eliminate all the subtle shades of reality and replace them with crude contrasts. Rudwick shows that almost nothing about the Devonian controversy was simple or straightforward. The paths of all the major participants through the debate were complex, and those of some, for example, Murchison, were positively tortuous. The eventual solution to the controversy (the concept of the Devonian) was unforeseen and probably unforeseeable by any any participant at the outset, for it represented neither of the original starting positions nor some sort of easy compromise between them. In a memorable metaphor, Rudwick suggests that "the original battle lines... having initially faced each other in opposition, filtered silently through each other, as it were, until they faced outward, leaving at their rear a domain defended by them both" (p. 405). This, as Rudwick points out, is a very different image of scientific controversy from that which is to be found in the abstract idealizations of so much philosophy of science; yet it is an image which emerges from the case study only at the level of its finest details. There are implications here, surely, for those who wish to draw philosophical lessons from the history of science.

Rudwick himself sees his work as providing a potential alternative to two equally unsatisfactory extreme views of the nature of scientific knowledge: naive realism (science provides us with absolute and totally objective knowledge of the external world) and naive relativism (science provides us with nothing more than conventional constructs whose contents are totally unconstrained by the external world). Certainly, these extremes are hopelessly naive, and we should all earnestly desire some alternative to them. Rudwick's view appears to be that scientific knowledge is both socially constructed and naturally constrained; that is, scientific knowledge is human and bears the necessary imprint of the social processes by which it was produced, and yet at the same time it is knowledge of a nonsocial nature that plays a significant part in its production.

Rudwick attempts to convey a clearer sense of this view by means of metaphors, especially ones drawn from the world of crafts such as metal-working (he is himself a silversmith). Scientific knowledge, he suggests, is *shaped* or *forged* (in the original sense of that term) in a collective interaction with nature. The resulting objects are products of human creativity, to be sure; but the materials from which they are made are natural, and though malleable they may also be refractory. Stated alternately, we may say that there is no necessity for scientific knowledge to take the particular form that it does (the geological column need not, for example, have been divided up and labelled as it was); but equally, scientific knowledge could not take any old form we may happen to choose (the geological column could not have survived De la Beche's find without some sort of modification or adjustment). Nature is permissive of a range of interpretations, but it is not equally permissive of all possible interpretations. There are some checks (in both senses of the word) upon our scientific knowledge, because this knowledge does indeed refer to a natural world that has qualities which are not determined purely and simply by the way in which people interact with it.

To many readers of *Zygon*, these arguments on behalf of scientific knowledge as something more than simply the product of the social relationships by which it is generated may seem so self-evident as to appear almost platitudinous. However, in recent years a number of rather vocal sociologists of scientific knowledge have been carrying the case for their fashionable discipline perilously close to the position of naive relativism. For this reason *The Great Devonian Controversy* is a particularly timely contribution. There is no doubt that it is a major case study which will be very widely cited not just by historians but also by philosophers and sociologists of science. Given this, it may be just as well to keep in mind Rudwick's own caution: his *is* but a single case study; and we shall need many more—researched with equal care, with equal attention to detail, and with equal rigor—before we are able confidently to pronounce upon the general character of scientific knowledge.

In the meantime, however, I, for one, know which book I shall now choose first whenever I am asked the question, "Why bother to study the history of science?".

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Evolution: A Theory in Crisis. By Michael Denton. Bethesda, Md.: Adler & Adler, 1986. 369 pages. $17.95.

Evolutionary biology is in robust health. The current flurry of debates is an early sign of a new burst of growth. Some observers, apparently deceived by the hyperbole that has accompanied these debates, have mistaken growth pains for terminal illness. Michael Denton is one such observer. *Evolution: A Theory in Crisis* is an anti-evolution treatise. Its theme, exemplified by the title and stated explicitly in the preface, is that there is a crisis in evolutionary biology of fatal

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proportions. Its parting conclusion is the fallacious assertion that the achievements of evolutionary biology amount to nothing more than "the great cosmogenic myth of the twentieth century" and provide no new insight to the origin of living beings on earth.

The book belongs to the "creation science" genre. Denton's presentation differs from the usual creation science works in only one respect: he does not actively espouse the creation science claim for a scientific basis in Genesis. The book, therefore, has the appearance of being strictly a book on biology. Intelligent laypersons reading Denton's book may think that they have encountered a scientific refutation of evolutionary biology. As a serious piece of biology, however, the book could not pass the most sympathetic peer review. In its approach, methods, and style it is straight out of the creation science mold. Abuses typical of creation science literature abound: evolutionary theory is misrepresented and distorted; spurious arguments are advanced as disproof of topics to which the arguments are, at best, tangentially relevant; evolutionary biologists are quoted out of context; large portions of relevant scientific literature are ignored; dubious or inaccurate statements appear as bald assertions accompanied, more often than not, with scorn.

Deciding how to deal with such a book is not a trivial problem. The book purports to be a biological treatise. Its scope ranges from paleontology to molecular biology, with excursions into the history and philosophy of biology. No area escapes misrepresentation and distortion. Point by point rebuttals would require a treatise of comparable proportions, which is certainly beyond the limits of any one review. Besides, detailed exposes of creation science literature already exist, including Philip Kitcher's *Abusing Science: The Case Against Creationism* (Cambridge: MIT Press, 1982) and the collection of essays, *Scientists Confront Creationism*, edited by Laurie Godfrey (New York: Norton, 1983). Many of Denton's misconceptions and distortions are addressed by these two works.

If this were simply a book written for scientists it could be ignored. However, it is not; it is clearly intended for laypersons, whose interest is most likely motivated by philosophical and theological issues. Such an audience cannot be expected to have the necessary expertise to avoid being deceived by the book's manifold abuses of evolutionary biology. A detailed critique being out of the question, the strategy adopted here is to focus upon two themes that are characteristic of the book's treatment of evolutionary biology: chance and typology.

The first theme, which occurs repeatedly as a leitmotif, is that familiar old war horse, "Mere Chance." It first appears in the preface with the statement that since Charles Darwin's time ". . . chance ruled supreme. God's will was replaced by the capriciousness of a roulette wheel." In a later passage is found the assertion: "The driving force behind the whole of evolution was the purely random process of natural selection" (p. 60). Equating natural selection and the origin of adaptations with "problem solving by trial and error" and "gigantic random searches" is a repeated theme (e.g., pp. 61 and 308).

Pejorative appeal to naive notions of "chance" is typical of creation science literature and is a clear sign that Denton's book is not to be taken as a serious book on biology. Describing natural selection as a purely random process distorts basic population genetic theory. Such statements demonstrate lack of understanding of Darwin's ideas and fail to acknowledge a vast amount of contemporary literature, especially the relevant writings of Ernst Mayr (who is, nevertheless, referenced sixteen times in Denton's index with respect to other topics). Nonbiologists can find a good discussion of the way in which random processes interact with deterministic processes in the theory of organic evolution through natural selection in Mayr's article "Evolution," *The Scientific American* 239 (September 1978):46-55.

Furthermore, the word *chance*, in its every day usage, is filled with ambiguity and imprecision. Kitcher (chap. 4) provides a good discussion of different meanings subsumed by the term, several of which commonly occur in discussions of evolution. The different usages imply very different contexts and carry very different connotations. Because stochastic processes occur in virtually every branch of science, including evolutionary biology, and because laypersons, especially those who are less than comfortable with mathematics, often have difficulties with the concept of random events and with processes governed by probabilistic laws, any writer attempting a serious discussion of phenomena that involve random processes has an obligation to exercise reasonable precision in the way that the role of random events is presented. Uncritical imputation of "mere chance" is not appropriate.

The second theme, which is the major theme of the book, is a typological view of organisms. Six chapters are devoted to a resurrection of this view of biological organization. Under a typological view, different kinds of organisms are regarded as constituting distinct, independext *types* between which any concept of genealogical relatedness is meaningless. Under a typological view, variation is without significance: variations within a type are distractions, inconsequential deviations from the essence of the type; similarities (and differences) between types are mere coincidents, the by-products of each type's being what it is. Subscription to a typological view of organisms was the norm among early nineteenth-century biologists. (It is still a central tenet of current creationism. Denton's focus on typology is right in step with the creationists' agenda.) Abandonment of a typological view of organisms and recognition of the significance that individual variability has had in the history of life on earth is precisely what the Darwinian revolution was all about. In *The Genetic Basis of Evolutionary Change* (New York: Columbia Univ. Press, 1974), Richard Lewontin emphasized this point with a touch of elegance: "He [Darwin] called attention to the *actual* variation among *actual* organisms as the most essential and illuminating fact of nature. Rather than regarding the variation among members of the same species as an annoying distraction, as a shimmering of the air that distorts our view of the essential object, he made that variation the cornerstone of his theory."

When stripped of its cloak of respectable terminology, Denton's case for a typological view of organisms is seen to be nothing more than the old arguments of "missing links" and "gaps in the fossil record"—arguments that long ago ceased to have biological support. Current debates among biologists on the topic of gradualism versus punctuationalism might appear to involve new evidence, but these debates are, in fact, a red herring for advocates of a typological view of organisms. The key issue for typological thinkers is an absence of genealogical relations between types. The questioning of gradualism by contemporary biologists is a debate, first, about the tempo of morphological change and, second, about processes responsible for large-scale patterns of variation among organisms. Nowhere in the debates is the issue of genealogical relatedness brought into question.

Denton attempts to build a broad case for his typological perspective. I shall confine attention to his treatment of molecular data, which his editors specifically tout in the blurbs on the dust jacket. (Readers interested in problems with Denton's treatment of other areas should see the chapters by Joel Cracraft, Laurie Godfrey, and C. Loring Brace in *Scientists Confront Creationism.*) Advances in molecular biology during the past thirty years opened a new window for viewing genealogical relations among organisms. The results are close to spectacular. Embedded in the structures of common proteins are telltale clues of genealogical relationships that provide overwhelming, independent, corroboration of the principle of biological evolution. Typological thinking in biology died long ago; molecular data have sealed the coffin. Denton, however, contends that molecular biology provides new evidence for a typological view of organisms. Inspection of Denton's arguments in Chapter 12—"A Biochemical Echo of Typology"—reveal that his conclusions are based upon an artifact produced by faulty interpretation of the data. Since Denton's professional training is said to be in molecular biology, a detailed look at the situation is in order.

Biochemists have elucidated detailed structures of a variety of proteins obtained from a diverse array of organisms. (Anyone unfamiliar with rudimentary molecular genetics can read, with confidence, Denton's Chapter 11.) Some of the proteins studied are found only in certain kinds of organisms; others occur in virtually all organisms. In the latter case, the molecular structure of a specific protein—cytochrome C is a classic example and the one used by Denton—can be determined in each of many different organisms. It turns out that the structures of the same protein in two different organisms are rarely identical and in some cases quite dissimilar. The amount of difference can be quantified.

Denton provides representative data in Table 12.1. The data are extracted from the leading biochemical reference on the subject and are good; Denton's analysis and conclusions are not. Denton builds his arguments upon a phenomenon that he calls "molecular equidistance." He uses this phrase to refer to empirical results such as the observation that cytochrome C in bacteria, for example, differs by approximately the same amount (roughly 65-70 percent) from the cytochrome C's found in each one of the other organisms listed in the table (vertebrates, insects, plants, and yeasts). Denton uses such observations to infer (erroneously) distinct typological classes. Discussing the data, he makes statements such as: "The bacterial kingdom has no neighbour in any of the fantastically diverse eucaryotic types. The 'missing links' are well and truely missing" (p. 281); and "There is not a trace at a molecular level of the traditional evolutionary series: cyclostome → fish → amphibian → reptile → mammal. Incredibly, man is as close to lamprey as are fish!" (p. 284).

These conclusions are erroneous: in his interpretation of "molecular equidistance," Denton has confused ancestor-descendant relationships with cousin relationships. The telltale clues of molecular data are not, directly, concerned with parents and offspring, intermediate forms, and "missing links." They are, instead, reflections of relative relatedness between contemporary cousins. Twentieth-century bacteria are not ancestors of twentieth-century turtles and dogs: they are very distant cousins, and, as the data in Denton's presentation show, the bacteria are roughly equally distant cousins of both turtles and dogs (and all the other organisms that Denton included in Table 12.1).

Cousin relationships between contemporary individuals are governed by the number of generations since there last was an ancestor in common to the individuals. Different members of a group of close relatives always have the same relationship to a more distantly related individual who stands outside the group. Two sisters are equally related to a mutual first cousin. Members of a group of siblings and first cousins are all equally related to a mutual fifth cousin. Lampreys are equally distant cousins of both fish and humans because the last ancestor that lampreys had in common with humans was the same ancestor lampreys had in common with fish. The "molecular equidistance" argument that Denton invokes is invalid, resulting from making comparisons between a single distantly related organism and various members of a more closely related group.

There is an irony in Denton's presentation to anyone familiar with the data of molecular evolution. Reflections of genealogical relationships are so strong in molecular data that Denton, in spite of his arguments to the contrary, is unable to hide them. The missing "trace" of which he speaks is not a trace; it is a shout. Simple inspection of the data in Table 12.1 will reveal that cytochrome C found in horses, for example, is quite similar in its molecular structure to that found in turtles, slightly less similar to that in fish, still less similar to that in insects, and very much less similar to that in bacteria. The traditional evolutionary series is very much in evidence.

Denton provides a series of diagrams (pp. 282-87) in which nested elipses, arranged on the basis of molecular data, are used to illustrate his spurious "molecular equidistance" thesis. In these delightful figures organisms are seen to cluster fully in accord with the genealogical relationships that evolutionary biologists deduced from comparative anatomy and paleontological evidence long before molecular data were available. In the final figure, humans and chimps are seen side by side as each other's closest cousin. Anyone who wants to argue that these nested groups of organisms constitute separate, distinct, and unbridgable groups has to contend with obvious hierarchical patterns of relatedness among the various groups. Notions of relatedness are, of course, antithetical to a typological view of organisms.

Denton claims that a crisis exists within evolutionary biology. His claim is off base: to the extent that evolutionary biology is at all involved with a crisis, the crisis lies outside of biology. For creationists, with a strictly literal interpretation of the Bible, the biological facts of human history create a theological crisis. Their assault upon sound science has elevated the American penchant for anti-intellectualism to a crisis stage with which everyone, including biologists, should be concerned. British evangelicals wrote in the 1830s that "If sound science appears to contradict the Bible, we may be sure that it is our interpretation of the Bible that is at fault" (*Christian Observer* [1832], p. 437; quoted by Stephen Neill, *Anglicanism* [Baltimore: Penguin Books, 1960], p. 240). Nevertheless, not only creationists but also many contemporary evangelical Christians are genuinely uncomfortable with evolutionary biology and what they perceive as a threat to the scriptural basis of their faith.

In other theological circles, evolutionary biology created little, if any, crisis. In 1930 William Temple, the Archbishop of York, wrote: "When my Father [Frederick Temple, Archbishop of Canterbury] announced and defended his acceptance of evolution in his Brough Lectures in 1884 it provoked no serious amount of criticism. . . . The particular battle over evolution was already won by 1884" (F. A. Iremonger, *William Temple, Archbishop of Canterbury, His Life and Letters* [London: Oxford Univ. Press, 1948], p. 491). To a large extent it would seem that evolution has been tacitly accepted and essentially ignored within such circles, although there has been a significant number of serious attempts to integrate evolutionary understanding into theology. Pierre Teilhard de Chardin provides a famous example, as does biologist Theodosius Dobzhansky. For more than twenty years the pages of *Zygon*, to cite an obvious example, have carried notable contributions from scientists and theologians. I suggest, however, that such efforts have been predominantly academic and philosophical. For the typical cleric and the average person in a pew on Sunday mornings, evolutionary biology, if not considered outright hostile to religious convictions, tends to be kept in a separate mental compartment.

Biology and theology each have important things to say about the human condition. Sound science without theology leaves us stranded with subjective values, no basis for morality, and no conception of purpose. Sound theology, if it ignores biology, can give at most incomplete—and at times faulty—understanding of human nature. Creationists use an objectionable piece of theology to justify inexcusably bad science. Books like *Evolution: A Theory in Crisis* are, at the very least, hindrances. We need good science and good theology. The two have operated too long in isolation. The time is ripe for a grand synthesis that will bring into register the complementary insights into human nature provided by modern biology and biblical theology.

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Images of Science: Essays on Realism and Empiricism, with a Reply from Bas C. van Fraassen. Edited by PAUL M. CHURCHLAND and CLIFFORD A. HOOKER. Chicago: University of Chicago Press, 1985. 309 pages. $18.95 (paper).

This collection of critical essays is directed against a new and exciting form of empiricism, recently developed by Bas van Fraassen in his *The Scientific Image* (Oxford, England: Clarendon Press, 1980). The essays are technical, but paradoxically they are readable and understandable even to readers who have only a rudimentary familiarity with van Fraassen's philosophy of science. Perhaps the primary virtue of the collection is the detailed responses that van Fraassen offers to his critics and, while these responses are not always satisfying, they represent very penetrating insights into the nature of empiricism.

The background for appreciating this anthology is the widespread rejection of positivist empiricism that took place during the sixties. The critics of positivism had directed their attack against the following positivist dogmas. First, observation sentences were epistemically neutral and distinct from the theoretical sentences of science. Second, the theoretical concepts within science were definable solely in terms of observational concepts. Third, the context of scientific justification was distinct from the context of scientific discovery. Fourth, following David Hume, positivists maintained that metaphysical commitments, such as belief in unobservable entities (realism), constituted no essential part of science. Van Fraassen joined with the critics of positivism and rejected the first three claims, but separated himself from these critics by arguing for an agnostic brand of anti-realism which maintains that empirical adequacy alone is a sufficient basis for understanding the logic of scientific

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justification. The heartland of van Fraassen's empiricism, therefore, is that our cognitive life never requires belief in unobservable entities.

Agnosticism, of course, is a concept which is more familiar to theologians and philosophers of religion than to philosophers of science, but it becomes a deeply significant concept within van Fraassen's program primarily because scientific theories contain many terms that "seem to refer" to entities which are in principle unobservable. For van Fraassen one may *accept* such concepts as the electron in the sense that one uses the concept without being required to *believe* that electrons actually exist. One can thus suspend judgment on the existence of electrons just as the theological agnostic suspends judgment on God. More importantly, one can suspend such judgment without suspending conversations about either electrons or God. In effect, for van Fraassen belief and acceptance are distinct. Belief need not extend into the realm of the unobservable.

A strategy that extends throughout the essays edited by Paul Churchland and Clifford Hooker is that, despite van Fraassen's ingenious attempts to avoid realism, he cannot do so without adopting one or more of the first three claims of positivism that he explicitly rejects. Churchland, in an article titled "The Ontological Status of the Observable," argues that van Fraassen's attempts to limit belief to that which is observable involves reintroducing the positivist notion that theory and observation are radically distinct. Churchland argues that if we should be agnostic toward theories as van Fraassen claims, and, if theories are embedded or presupposed by our observations, then contra van Fraassen we ought to be agnostic toward observation as well. For example, suppose that the seventeenth century's use of the "pan balance" as an instrument of chemical analysis assumes Sir Isaac Newton's theoretical claim that the world is actually composed of unobservable, quantitatively distinct particles which we call atoms. If we can be skeptical of the atoms as van Fraassen permits, why should we not be skeptical of the empirical regularities gathered from the pan balance?

Van Fraassen's response to such an argument appeals to the property of underdeterminacy. Two theories are underdetermined if they are inconsistent with each other and yet compatible with the totality of available empirical data. We can, for example, imagine two Newtonians disagreeing over the reality of material atoms and yet agreeing that the inverse square law best explains all the available data. The metaphysical debates are thus distinct from the mathematical laws that do the hard scientific work.

Richard Boyd, in his essay "Lex Orandi est Lex Credendi" challenges this notion that metaphysical claims are irrelevant to empirical derivation. Van Fraassen's underdeterminacy response does not face the methodological challenge to the pan balance. Many instruments such as the sand clock yield empirical regularities but we have not the slightest interest in them nor do we feel obligated to explain them. Furthermore, pseudoscience is full of pseudo-instruments, many of which supposedly yield exciting regularities within nature. One needs, therefore, a justification for preferring the pan balance and, with the Newtonian atomic thesis, no such justification is possible. For Boyd, scientific theories do more than spell out mathematical structures that are capable of deriving data. Theories specify for us what constitutes real from merely illusory empirical regularities.

Van Fraassen's response is that to assume that method justifies instrumental preference and that theory justifies method is an argumentative strategy that is a "recipe for disaster." This strategy not only isolates theory from experiment but also is open to the charge of circularity since it is instrumental or experimental success that supports a given theory. What exactly is van Fraassen's alternative to this recipe for disaster? Is it simply a mistake to attempt to justify methodology? Not exactly! In the *Scientific Image*, van Fraassen seems to provide an evolutionary and pragmatic response to this demand for methodological justification. Scientific methods and experimental designs cannot be justified but they can be explained in the sense that evolutionary theory can explain evolved characteristics. Instead of attempting to axiomatically justify methods, philosophers ought to interpret methods as biologists interpret physiological traits. Such explanations will then show how survival value is enhanced by maintaining or disregarding a particular method. Pan balances have survival value and sand clocks do not.

The compatibility of this evolutionary approach with van Fraassen's anti-realism is challenged by Ronald Giere in his essay "Constructive Realism." Giere argues that, while this evolutionary approach to method makes perfect sense, one cannot accomplish this evolutionary task without viewing scientists as having a "theistic" rather than an "agnostic" attitude toward unobservable entities. To justify this claim, Giere appeals to the experiments by Linus Pauling, James Watson, and Francis Crick concerning the double helix model of the DNA molecule. The actual intentions of these developers are not merely to account for spots on X ray photographs but rather to determine the actual angular relations among the atoms within the unobservable structure of the DNA molecule. The observable spots in the photographs are viewed by these scientific giants as effects of unobservable causes. However, van Fraassen's empiricism is unable to explain these actual beliefs since, according to van Fraassen, one need not accept the driving principle that motivated molecular research during the fifties, for example, that empirical regularities could be explained in terms of unobservable, microbiological structures.

What Giere is doing is claiming that van Fraassen's distinction between accepting a theory and believing a theory is, from the viewpoint of the history of science, a bogus distinction. If one believed only in observable X ray photographic spots and was agnostic toward unobservable molecular structures, the DNA research program of the fifties would be totally incoherent. A realistic attitude toward unobservable mechanisms is required for making sense of the methodology that governs DNA research.

Another issue that permeates this collection is van Fraassen's tendency to overestimate the value of physics within his general theory of science. Special and general relativity, as well as quantum theory, are filled with conceptually paradoxical assumptions that make realistic interpretations of such terms as *mass*, *particle*, *wave*, *distance*, and so on, conceptually confusing. How can the nature of an object change merely by switching our experimental viewpoint toward that object? A realist view of these terms seems to involve an idealist thesis that mere changes of viewpoint alter physical realities. Van Fraassen's agnostic anti-realism certainly blocks such idealistic implications by claiming that theories function merely as heuristic devices that produce mathematical models that compete with one another in terms of empirical adequacy. As heuristic devices, theories are neither true nor false. In addition, theories are not essentially involved in the process of scientific justification, but are to be evaluated pragmatically in terms of their usefulness for producing or discovering mathematical models. One need not worry about the confusing character of subatomic particles since the theory that introduces them does not function descriptively. Rather, it functions merely as an aid for producing purely mathematical structures for correlating data.

However, while this strategy is apparently a sensible way of dealing with the conceptual paradoxes of quantum theory, no reason is offered by van Fraassen for assuming that the construction of such models must be the ideal against which all the other sciences must be judged. The theoretical entities of geology, biology, and so forth are not as conceptually paradoxical as the entities of physics. Tectonic plates are not, for example, observer dependent nor do DNA molecules exhibit the complementarity that haunts our conceptions of the electron. Furthermore, what most troubles this argument by van Fraassen is that it seems to reintroduce the positivist distinction between justification and discovery. Theories for van Fraassen are not descriptive because they are mere heuristic devices. They do not play an essential role within justification. In a sense, for van Fraassen, theories describe *us* more than the world because theories are merely our tools for producing empirically adequate mathematical models. However, what weakens this line of reasoning is its infidelity to the history of science. Theories have been "considered justified" not merely because they explain current data, but because they are viewed as fruitful for producing novel information precisely because they accurately describe what lies beneath the surface of current observation.

This same issue concerning the ontological commitments of quantum theory is tackled by Gary Gutting in one of the most imaginative essays in the collection. Following Wilfrid Sellars, he argues that, since theoretical concepts cannot be defined solely in terms of the observable, and since such concepts are indispensable for scientific explanation, we must "believe" in theoretical entities. Gutting draws an analogy that illustrates his point. He claims that we believe in rather than merely accept the concept of the dinosaur because unobservable dinosaurs explain observable artifacts. He claims that we similarly ought to believe in, rather than merely use, the electron. However, this ontological jump from the existence of dinosaurs to the existence of electrons is, for van Fraassen, troubled by a profound disanalogy. Individual dinosaurs are observable in principle. Electrons are not. The limits of the observable itself are determined by the postulates of the quantum theory and this theory assumes that electrons, as opposed to dinosaurs, will never be observed.

This response to Gutting and Sellars is, however, a bit unsatisfying. How can theory take an epistemic back seat to the observable when theory itself determines the limits of the observable? For, if theoretical concepts can determine the limits of the observable, they would seem to be in some sense independent of empirical data. There is, in short, a profound incoherence in claiming that a theory has no existential import even though the theory determines the limits of existential import by determining the limits of the observable.

This takes us to the core of van Fraassen's agnosticism. Can we suspend judgment about the realities postulated by a theory when we admit that, in fact, the theory determines the limits of reality by fixing the limits of the observable? Throughout his responses, van Fraassen justifies his agnosticism by offering a rhetorical question: what does belief in the existence of electrons do over and above the mathematical formulas of quantum theory? The answer is that believing that electrons have certain properties provides us with a reasonable justification for disregarding our failure to observe electrons. Our failure to directly observe them is theoretically expected and therefore is excluded from the set of falsifying evidence. Ontology precedes epistemology in the sense that the limits of significant observation are determined by "the way the world is" rather than by any empiricist policies.

Finally, what makes this collection especially valuable for all philosophers, be they professional or nonprofessional, is that it translates the perennial debate between realism and empiricism into a contemporary idiom that mixes the terminologies of both science and philosophy. One can no longer defend or even formulate philosophic empiricism or realism without a deep appreciation of both contemporary and historically significant episodes within the history of science. Nor can we practice a sophisticated form of science without recognizing the epistemic and metaphysical commitments that are at the root of science.

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In Search of the Person—Philosophical Explorations in Cognitive Science. By MICHAEL A. ARBIB. Amherst, Mass.: University of Massachusetts Press, 1985. 156 pages. $9.95 (paper).

Professor Arbib, who teaches computer and information science at the University of Massachusetts, gave the 1983 Gifford Lectures jointly with Professor Mary Hesse of Cambridge, England. These are due to be published under the title *The Construction of Reality.* His present book, he tells us, grew from a short series of lectures in which he tried to share some of the ideas from Edinburgh with colleagues at his home university. Just why this separate book is needed is not clearly explained. The reviewer (and, one suspects, the general reader) is constantly teased by hints that he will find Arbib's position more fully argued in the other volume and by attempts to summarize alternative arguments by Hesse which Arbib does not find convincing. The tone, however, is genial, not at all that of a dissenter who needed to have his say independently of his partner.

Arbib writes as an agnostic who sees the world in secular terms and makes "no appeal to God." "Unlike the theist, I resist any notion of *ultimate* principle though I do not deny the value of a set of well-grounded principles to guide our interactions within a particular sociohistorical context" (p. 126). "Our problem is that we have not yet learned to be fully secular" (p. 129). He is scrupulous in emphasizing that his scientific theories are tentative and that he cannot disprove the theistic view; but he argues that in human behavior there is nothing that demonstrably lies beyond the scope of physical science or transcends space and time. In tone, if not always in content, his message in the name of mechanistic brain science is anti-theistic.

The pity is that in partial justification Arbib is able to cite arguments by some Judaeo-Christian theists who appear to share the idea that transcendent realities like the human soul, or God, are credible only if appropriate gaps are found in scientific explanations of human behavior. He writes as if there are just two options, one secular, with hopes pinned on the eventual success of some mechanistic "schema theory," the other theistic, with the firm (but he

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thinks ill-grounded) conviction that all mechanistic explanations must eventually fail. Except for one footnote he takes no account of a third view, shared by a number of brain scientists who are also theists, according to which a belief in transcendent realities need set no limits *a priori* to the success of mechanistic explanations of human behavior. According to this third view, the kind of schema theory favored by Arbib (among others) ranks as a promising option which has no tendency whatsoever to compete with the Judaeo-Christian emphasis on realities beyond space and time.

Thus, when Arbib sets the scene on pages 22-23, he draws a parallel between asking "Is there a God-reality separate from physical reality?" and asking "Is there a soul (or mind) separate from the body?" "(Some people will say that) the mind is just a property of our complex brain and bodies as we interact with other people. Other people say 'No . . . , understanding the brain will only explain a certain amount of what people do.'" This leaves out of account the view, by no means peculiar to theists, that understanding the brain might well in principle explain all that people *do*, yet leave deeply mysterious and unexplained what people *are*. The analogy with the question of God's existence is doubly misleading; for in the case of God what we want to know is *whether there is anyone there* over and above the sum total of physical entities, whereas in the case of human beings there is no doubt (for each of us) that there *is* someone there: what puzzles us is how we as conscious centers of awareness relate to the brains-and-bodies that we contingently know to be ours. There may be an observer-language sense of "mind" in which it is not absurd (albeit rather sloppy) to speak of it as a "property" of the brain, just as we might describe the "mind" of an artificial chess-playing machine as a "property" of its structure. Yet, however exciting we might find it to play against a rule-governed chess machine, most of us (including Arbib himself) would doubt that there is "anyone there" (opposite to us, a center of awareness) undergoing the flux of conscious experience that we ourselves know as we play the game; whereas in our own case (as Augustine of Hippo pointed out, long before René Descartes) we would have to be there (as conscious agents) in order to doubt it! (Note that there is not the same analytic absurdity in doubting whether our *brains* exist.) Ontologically, then, our problem is almost the converse of that of the existence of God. Whatever we may say about the mind, it would invert ontological priorities to suggest that *I* am just a property of my complex brain, and so on.

The bulk of the book is devoted to justifying the author's confidence (which I share) in the usefulness of what he calls schema theory as an explanatory approach to individual and social human behavior. This sees brain activity as cooperative interaction and competition between a very large number of schemas or component-organizers of behavior (or better, states of conditional readiness for action or the planning of action). Some of these components are innate, others acquired; but nearly all are liable to be shaped and reshaped by experience, especially through communication with others; and there can also be "innate patterns of schema change" (p. 61). The exposition is mostly nontechnical, readable, and modest. Arbib makes much less sweeping claims than some enthusiasts for Artificial Intelligence (of whose exaggerations he is critical). He has interesting things to say about the development of language, suggesting that the literal function of words should be regarded as a special and atypical case, with the metaphorical as the normal (p. 66). This leads to an illuminating and not unsympathetic account of "the transition in Freud's intellectual career from neurology to mythology," in the course of which Arbib underlines Freud's distinction between "illusion" (which may be true) and "error or delusion" (p. 88).

Turning to the social sciences, Arbib gives evidence of his interactions with Mary Hesse on such themes as the nature of explanation and the possibility of objectivity. Friedrich Schleiermacher, Wilhelm Dilthey, and R. F. Collingwood are criticized for failure to allow for "cultural distance between the observer and the period or culture observed." (It is not clear that we had to wait for schema theory to point out this danger, as suggested on p. 96, nor that Dilthey's hermeneutic approach was blind to it.) The idea of fusing all religions to come up with the right way of looking at the world is rejected as meaningless. Hans-Georg Gadamer's perspectivist notion of the "fusing of horizons," however, is welcomed: "The strategy adopted throughout this volume is a hermeneutic dialogue that takes things and persons as different horizons which we seek to fuse" (p. 98). Get it? I did not.

Earlier and later Marxist notions of scientific objectivity are contrasted, and Émile Durkheim is cited in defense of the idea that ideology can provide "an important tool for mental economy." "The way in which the ideology is internalized," Arbib argues, "does not preclude a critique" (pp. 105-6). With regard to Jürgen Habermas's ideal of symmetrical communicative discourse, he is more pessimistic: "It is not clear to me that this ideal speech situation would yield convergence to agreement on the ultimate good, nor is it clear to me that such convergence would be desirable" (p. 101). The first remark is doubtless fair enough; but just supposing there *were* some ultimate good to be recognized, by what kind of logic would its recognition be deemed undesirable? Arbib's negative judgment makes sense only on the assumption that there is no ultimate good.

On human freedom (the topic of his last chapter) Arbib takes a fairly standard liberal view, labeling it (unhelpfully) "decisionist" as over against Hesse's "voluntarist" position. He warns any secularists who slide into voluntarism that "your secularism has been 'impaired' in that you can no longer hold that all human reality is within space and time" (p. 117). Again, alas, we are referred to *The Construction of Reality* for more detailed arguments. He does, however, emphasize on pages 124-25 that neither his secularism nor his model of society is strictly implied by schema theory. What then, the theologian might ask, is all the fuss about? Perhaps *The Construction of Reality* will tell us.

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What They Are Saying About Genetic Engineering. By Thomas A. Shannon. New York: Paulist Press, 1985. 103 pages. $4.95 (paper).

Thomas Shannon's book provides a helpful overview and catalogue of ethical issues which surround genetic engineering, a topic previously addressed by Shannon in a book which he edited, *Bioethics* (New York: Paulist Press, 1976). His positions as assistant professor of social ethics at Worcester Polytechnic

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Institute, visiting assistant professor of bioethics at the University of Massachusetts Medical School, and his postdoctoral work at the Hastings Institute provide the extensive background reflected in this book on genetic engineering. This is Shannon's second book in the series, the former entitled *What Are They Saying About Peace and War?*

Drawing upon both the secular and religious traditions in ethics, Shannon provides some fresh insights into the thorny issues which surround genetic engineering. I have chosen to place the major issues which Shannon raises under three headings: first, the goals and limits of science; second, the role and responsibility of scientists, and third, obligations to future generations. Closely related to these issues are changing conceptions of nature and the person.

First, Shannon links the goals and limits of science to alternative views of nature and concludes that, while little can be said in favor of restricting knowledge, a stronger case can be made for controlling applications of science. Nature has been regarded as a limit (Aristotle, Roman Catholic tradition), as a model (Richard McCormick, Leon Kass), and as evolving (Karl Rahner). Both nature as a limit and nature as a model represent static views of nature; those who see nature as evolving, by contrast, adopt a dynamic view of nature and "tend to see change or development as normative rather than exceptional" (p. 37). Shannon proposes that our capacities to engage in genetic interventions place us in a tension between the view that nature is static and the view of ourselves as involved in the shaping of evolution. He sees a need for Alasdair McIntyre's virtue of hope as we face the open future.

The shaping of evolution has come to be viewed as a more realistic possibility, according to Shannon, as the goal of science has shifted from an attempt to understand nature to attempts to "change nature to suit our needs—or wants" (p. 11). Nuclear energy and genetic engineering are two areas where this shift is apparent. Shannon addresses the question of the limits of science by asking whether restriction of knowledge or restriction of action is at issue. He finds little support for the view that knowledge should be restricted and cites the arguments of Key Dismukes, Daniel Callahan, and David Smith in favor of the freedom to pursue knowledge.

Applications of science, however, are in the realm of action, a realm in which restriction is justifiable. Callahan's principles for scientists engaged in changing nature are cited by Shannon, as are those of Clifford Grobstein and Michael Flowers for limiting the applications of gene therapy.

Second, the shift in the goal of science from understanding to changing nature has brought with it a new role and new responsibilities for scientists. Shannon proposes a new model of the scientist—as an advocate for a particular position, application, or cause rather than an explainer of nature or of particular applications. Shannon finds that this new role raises questions concerning the relationship of facts and values. Decisions to pursue research are directed by a cultural orientation. Hence, the expertise of scientists is no longer to be regarded as value-free.

Shannon describes what he believes can reasonably be asked of scientists in regard to responsibilities surrounding genetic engineering. He proposes that among the professional responsibilities of the scientist are the duty to adhere to professional codes of ethics, the duty to consider the likely consequences of a particular research application, and the duty to be aware of the social, political, and cultural context within which scientific decisions are made.

Third, a central issue in Shannon's book is that of obligations to future generations. In chapter 4, "Our Descendants and Their Future," Shannon describes different models of an ethic for the future. The model of stewardship assumes a static view of nature in which the limits of action are set by the orders of nature and society. A model of co-creator, advanced by Rahner and Robert Francoeur, regards nature as dynamic. In this model, humans participate in the evolutionary process with a view toward promoting human and social goods.

Shannon links the view of responsibility toward the future with one's understanding of personhood. He presents the criteria for humanhood set down by Joseph Fletcher and McIntyre and comments that both depart from the traditional model of the person, which is based on a static personal nature within a static social and natural world.

As we face the question of what we are to leave to the next generation, Shannon underscores McIntyre's view that we adopt an attitude of humility toward the future.

The moral problem of what we are to leave to our descendants, Shannon reports, has been approached by utilitarians and contractarians. These views suggest that, when calculating total utility or in determining how to act justly, we should take the future into account. The problem has also been approached by defenders of rights, who say that we ought at least not do harm to future generations. These various theories have in common the prescription that we leave our descendants at least as well off as we are.

The problem of what we are to leave to our descendants is affected by one's view of the human person (how I see myself in relation to other humans) and by one's view of the future. The view that I owe something to my descendants, according to Shannon, rests in part on these two premises: first, that I am a social rather than solitary being, a member of a community which comes from and will be succeeded by other communities, and, second, that I accept an eschatological rather than an apocalyptic view of the future. Obligations to the future do not arise in the apocalyptic view, since "the end of the world is the end of significance" (p. 27). But those who hold an eschatological view accept that what goes on in the present is important and stands in relation to the future which will eventually be reached. Shannon maintains that in the eschatological framework the question of what one ought to leave to future generations is significant.

Shannon's review of the literature on obligations to future generations, however, suffers from an important omission. He does not directly mention a debate over whether or not humans face a genetic decline. Shannon directs his attention mainly to obligations not to harm future generations. The obligations of nonmaleficence (not harming others) have generally been asserted in the ethical literature in response to large-scale threats—for example, environmental pollution, depletion of resources, and so on. As he approaches the literature on genetic engineering, Shannon continues to direct attention to obligations not to harm future generations. He highlights claims of threats—namely, a genetic apocalypse for humanity.

If a serious threat or danger exists, I would agree with Shannon and say that a duty not to harm is defensible as a primary obligation. Other duties—to preserve liberty, to do good, and so forth—are less likely to outweigh a duty not to harm when widespread dangers or threats to health and safety exist.

Shannon is accurate in saying that some believe humanity faces a genetic decline, but he does not cite this important debate in the literature on genetic intervention. A brief account of this controversy may be found in Marc Lappe's article, "Eugenics: Ethical Issues" (*The Encyclopedia of Bioethics*, vol. 1, ed. Warren Reich [New York: Macmillan/Free Press, 1978], pp. 462-68)—particularly the section on "defining the genetic status quo" (pp. 464-65).

The omission of the genetic-deterioration debate would explain Shannon's focus on an obligation not to harm future generations. If humanity faces a rapid genetic decline, a duty not to harm future generations could very well be defended as a primary obligation. However, if we do not face this bleak prospect, other duties may assume greater importance. The implications of a duty not to harm future generations could well include coercive measures to protect the public health. If widespread applications of genetic engineering techniques are justified on the grounds of obligations not to harm future generations, mandatory eugenic measures could readily result. In the absence of a clear threat, however, a duty to preserve reproductive liberty or other duties could be given a higher place.

Despite this omission, Shannon's work—with short chapters and good documentation (although footnote 6 is missing in chapter 6)—is a superior sourcebook for students and serious discussion-groups. The book serves as an introduction to many of the major names and themes in the current debate over the ethics of genetic engineering. The one-chapter excursions into sociobiology and birth technologies—with an extended treatment of surrogate motherhood—broaden the book's range of usefulness for discussion groups. The vocabulary is nontechnical and directed toward a general reading audience.

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Cosmogony and Ethical Order: New Studies in Comparative Ethics. Edited by Robin W. Lovin and Frank E. Reynolds. Chicago: University of Chicago Press, 1985. 416 pages. $19.95 (paper).

In the popular mind the relation between science and religion often boils down to the simple-minded controversy between evolution and creationism. It is a debate that serious scholars usually choose to avoid, but what is also often neglected as a result is the significance of cosmogonies—mythical and philosophical accounts of the creation of the world.

This is something of a pity, since notions about the way the world orginated are linked to broader patterns of thought and tell us as much about world views as they do about the natural world. The essays in *Cosmogony and Ethical Order* are exciting and interesting reflections on these connections. They demonstrate that, when one looks carefully at the various ways in which the beginning of the world has been conceived, "the distinction between scientific truth and moral import . . . is not so easily maintained" (p. 2). The fifteen finely honed essays in this volume explore the ethical significance of cosmogonic thinking from Andean Indian to Freudian, and together they build a convincing argument that notions about how the world began are directly related to ideas about how one should act within it.

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The scholars of ethics and comparative religion who have participated in this project show that in some traditions there is a direct correspondence between the creative act of God, or the gods, and human aspirations. Arthur Adkins, for instance, describes how the virtue prized by ancient Greeks is akin to the triumphs attributed to Zeus, and Lawrence Sullivan explains how cosmogonic rituals are performed by Andean Indians to conjure up the process of divine creation and orient them to proper worldly action. In other traditions the correspondence is not so direct. In Taoism, Norman Girardot points out, the evolutionary process is thought to be devolutionary—as it is in many Asian traditions—and the moral task is that of maintaining a semblance of social order in a brutish world.

Some views of creation may change over time. Douglas Knight and Wendy Doniger O'Flaherty show the evolution of cosmogonic thinking in the Hebrew Bible (the Old Testament) and the ancient scriptures of India, respectively. Hans Dieter Betz argues that the Sermon on the Mount in the New Testament is an attempt to reassert the cosmogonic presence of God and its moral force at a time when faith in the old Genesis view of the world had broken down. In other traditions, the ethical diversity of the tradition is expressed in multiple cosmogonies. Frank Reynolds shows how strands of moral thinking in Theravada Buddhism are supported by different cosmogonic mythologies, and Kay Warren describes how holding to two cosmogonies at the same time helps Indians in the highlands of Guatemala solve a cultural dilemma. In Islam, as Sheryl Burkhalter suggests, a dual cosmogony is required to explain the creative role of human morality: the original account of creation is complemented by another describing its fulfillment in human history.

What happens when the traditional cosmogonies are no longer able to carry their moral freight? One answer is that philosophical explanations are then devised in order to explain where the world came from and how one is supposed to act within it. This process of developing rational cosmogonies is found in such disparate locales as ancient Greece, ancient China, and eighteenth-century Christian England, according to the accounts given by Adkins, Lee Yearley, and Robin Lovin, respectively.

In our own modern society, science has replaced both mythology and philosophy in supplying notions about the origins of the natural world—and about human nature and moral conduct as well. Douglas Sturm and Yearley examine the cosmogonic views of two thinkers rooted in modern scientific method: Karl Mrax and Sigmund Freud. Sturm sees Marx's view of origins as essential to his notion of history and his understanding of the limits of human freedom; and Yearley regards Freud as having created his own myth of origin in the theory of the primal horde. Like Plato's myths in the *Republic*, Freud's story offers a richer account than mere rationality can provide.

Readers of this journal might feel that the authors of this book have neglected an obvious target for cosmogonic analysis: modern-day theorists of the big bang and its contenders. The editors make passing reference to these theories, however. Paraphrasing James Gustafson, they claim that "a modern scientific cosmogony that sees the evolution of life as continuous with the evolution of matter challenges us to formulate ways of living in a vast universe that seems largely indifferent to our existence" (p. 2). Presumably—although the editors do not say so—this indifference causes us to behave indifferently with regard to morality as well.

The main purpose of the project that led to this book was to explore new ways of understanding ethics across cultural boundaries. In their introduction to the book, Lovin and Reynolds provide a lucid summary of some of the prevailing approaches to comparative ethics and suggest an interesting alternative. They reject the notion that there is only one set of moral laws in the universe and only one form of moral discourse, but they do think that the various forms of ethical thinking can be compared. Their method is a "naturalistic" approach to comparative ethics, by which they mean studying particular moral choices and values as they are embedded in a "comprehensive view of reality" (p. 30). One of the ways of seeing a culture's comprehensive view of reality is to look at its understanding of how the world began. Hence the study of cosmogonies in order to understand the moral discourse of various cultures.

To test the usefulness of this approach Lovin and Reynolds convened over thirty scholars at three successive meetings at the University of Chicago. The meetings were supported by the University and by a National Endowment for the Humanities project on comparative religion administered by Harvard's Center for the Study of World Religions and the Graduate Theological Union at Berkeley. In these meetings the papers were refined and integrated, and clarifications were made in the analytic perspective supporting them. The result is a degree of coherence seldom found in a work of multiple authorship, and a convincing demonstration of Lovin and Reynold's comparative approach. A wise decision of the University of Chicago Press to issue the volume in an inexpensive paperback edition makes it available to anyone interested in comparative ethics, the logic of mythology, and fabulous views of how the world came to be.

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