BOOKS: HISTORY AND PHILOSOPHY OF SCIENCE

Uncle Sam Wants You

David L. Hull

Many scientists, possibly most scientists, just do science without thinking too much about it. They run experiments, make observations, show how certain data conflict with more general views, set out theories, and so on. Periodically, however, some of us—scientists included—step back and look at what is going on in science. In doing so, students of science use some of the same techniques that scientists use. For example, we attempt to show that science is progressive by detailing the history of science over the past couple hundred years. Or we argue that scientists have been strongly influenced by their cultures by looking at particular instances of such cultural influence. In this respect, the study of science is reflexive. We gather data about science to show exactly how influential data are (or are not) in science. The result has been the science wars.

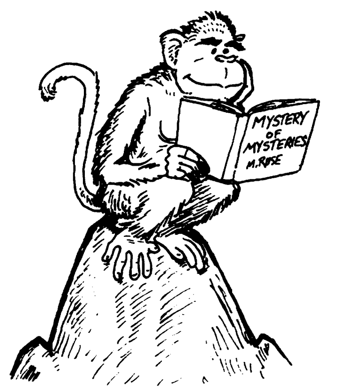
Mystery of Mysteries
Is Evolution a Social Construction?
by Michael Ruse
Harvard University Press, Cambridge, MA, 1999. 320 pp. $27.50, £16.95. ISBN 0-674-46706-X.

The two extreme positions in this dispute over the nature of science are social constructivism and positivism. The most extreme constructivists seem to hold that all of us, scientists included, are helpless victims in the maws of our societies. We all believe what our societies force us to believe. On this extreme view, the appeals that scientists make to reason, argument, and evidence are merely so much show to cover the social origins of our beliefs. The trouble is that constructivists live in precisely the same societies as the rest of us. Somehow they are able to free themselves from the ineluctable hold that society has on them, but strangely the rest of us cannot. Conversely, positivists are portrayed as evil, insisting that scientific world views are totally devoid of any such considerations—in particular of any appeals to values. Scientists simply tell it like it is. Reason, argument, and evidence are all that matter. Most of the issues that others find so fascinating, including metaphysics, are just nonsense. That positivists spent so much time writing on issues that surely count as metaphysical, including the claim that metaphysics is nonsense, hardly warrants mentioning.

As is commonly the case, both sides engaged in the science wars are constantly on the move: advancing, retreating, and covering their tracks, as best as they can, with verbal smoke screens. Combatants on both sides insist that they never held any of the views for which they are famous. Instead, they waffle extensively. Constructivists claim they always acknowledged that reason, argument, and evidence play crucial roles in science. All that they are attempting to point out is that social factors also play important roles in science, at times overwhelming more narrowly scientific factors. More traditional philosophers object to being tarred with the same brush as the extreme positivists. Professional and social factors play parts in science, of course, but so do reason, argument, and evidence; and in the long run, more narrowly scientific factors triumph. Hence, the only difference between the two sides is estimation of relative importance.

As reasonable as these more moderate positions may seem, their advocates still must confront the problem of reflexivity. How do we decide the relative importance of scientific and cultural factors in the decisions that scientists make? If we study science scientifically, then we run the danger of circularity. If we are to study science in some other ways, what are the outlines of these alternatives? No one has suggested any yet. It is here that Michael Ruse, in his *Mystery of Mysteries*, steps into the breach. When John Herschel referred to the mystery of mysteries in 1836, he had in mind the replacement of species through time, what we have come to call biological evolution. Ruse has the evolution of science in mind, and the evolution of science is as mysterious today as the evolution of species was a century and a half ago.

Ruse proposes to investigate the history of evolutionary biology from the late 18th century to the present to determine the influence of various factors in deciding the course of this scientific discipline. He selects a dozen or so evolutionary biologists to study. He begins with Erasmus Darwin as a representative of a pre-Darwinian evolutionist. For the 19th century, he quite naturally turns to Erasmus Darwin's grandson, Charles, and T. H. Huxley. Then, from this century, Ruse discusses Julian Huxley, Theodosius Dobzhansky, Richard Dawkins, Stephen Jay Gould, Richard Lewontin, E. O. Wilson, Geoffrey Parker, and Jack Sepkoski. Because evolutionary theory has been one of the chief battlefields in the war between constructivists and positivists, Ruse could not have picked a more appropriate topic of study. We have all heard, time and again, that the reason Darwin's theory was so individualistic, competitive, elitist, sexist, and racist is that Darwin's society exhibited these same characteristics. Darwin was so callow that he simply read the characteristics of his society into nature.



Ruse is uniquely prepared to write this book. He has published on the history of

The author is in the Department of Philosophy, Northwestern University, Brentano Hall, 1818 Hinman Avenue, Evanston, IL 60208, USA. E-mail: d-hull@nwu.edu

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evolutionary theory from before Darwin to the present. He is a professional philosopher of science who deals with a wide spectrum of topics, from the role of religion in science to the virtues of formalistic philosophy of science. He also has the courage of his convictions—at the 1982 Arkansas creationism trial he testified that science can be sharply distinguished from non-science, and that evolutionary biology is clearly on the scientific side of the divide while creationism is just as clearly on the unscientific side.

As strange as it may sound, courage is required for writing a book such as *Mystery of Mysteries*. Historians of science usually deal with dead scientists. Dead scientists cannot talk back, nor can they retaliate. Living scientists can. A shiver goes down my spine when I think of how the living scientists Ruse discusses are likely to respond to his book. But Ruse has always been willing to call 'em as he sees 'em. For the past dozen years, he has concluded each issue of his journal *Biology & Philosophy* with a set of editorial booknotes. As each issue arrives in the mail, I turn first to his "Booknotes" with equal parts of anticipation and apprehension. What will he say next, and will it be about me?

For each of the evolutionary biologists that Ruse studies, he asks how do such traditional epistemic values as predictive ability, consistency, and coherence contribute to the biologist's work? He also investigates the influence of what he terms "metavalues," those beliefs that scientists have about science itself. To take one example: in the early days of science, references to God in science were perfectly acceptable, but later such references were excluded. Finally, Ruse examines whether such cultural factors as beliefs in progress, male dominance, and individualism had significant effects on the path that evolutionary biology has taken.

As Ruse works his way from the 18th century to the present, he evaluates each of his subjects, first from a present-day perspective and then according to the standards of the subject's own time. For example, from a present-day perspective, Erasmus Darwin's *Zoonomia* (1794) hardly seems the stuff of science—after all, it is a poem. But in his day, his more serious contemporaries also had considerable doubt as to whether his writings counted as genuine science. Ruse agrees. The influence of epistemic values in the evolutionary writings of Erasmus Darwin was minimal. The effects of other factors were maximal.

When Ruse turns to Charles Darwin, the balance shifts dramatically. Darwin tried to make his theory of evolution look as scientific as possible, with varying degrees of success. One chief difference between the 18th and 19th centuries is that science was well on its way to becoming professionalized in Darwin's day, and one of the chief metavalues of professional science was that it had to be as free of nonepistemic values as possible. Regardless of how important a belief in progress may have actually been at the time, scientists had to act as if it did not influence their activities because such beliefs were not genuinely scientific. Darwin joined in this process of constructing science, and here the constructivist notion of "construction" has some bite. Nineteenth-century intellectuals did not simply discover science. To a large extent, they literally constructed it. Darwin, however, was not entirely successful in presenting his theory of evolution as exemplifying the best epistemic and metalevel values of his day. One of the commonest and most effective objections to Darwin's theory was that it was not genuine science. Ruse concludes that evolutionary thought in the late 19th century is "more epistemically rigorous than it ever was; yet at all levels it is thoroughly impregnated with culture" (p. 80).

As Ruse turns to evolutionary biology in the early 20th century, the difference between the science of then and today diminishes rapidly because the science of the day was rapidly becoming the science of today. The founders of the synthetic, or neo-Darwinian, theory of evolution were concerned to make evolutionary theory even more scientific than it had been, in particular more mathematical. Even so, nonepistemic factors were also operative. Several evolutionists, such as Julian Huxley, wanted to replace Christianity with a secular religion of progress. R. A. Fisher was not only an enthusiastic eugenicist but also a passionate Christian. Dobzhansky embarrassed many of his contemporaries by openly championing religion, including the works of Teilhard de Chardin. For others, including J. B. S. Haldane and H. J. Muller, Marxism played a comparable role, at least at some stage in their lives. Although Ruse has no trouble in setting out the extra-scientific views that these evolutionary biologists held, showing causal connections of any kind is much more difficult, particularly for religion and the substantive content of science. Ruse makes as strong a case as he can in the few pages available to him.

On the contemporary scene, Ruse sets out the disputes between two pairs of adversaries, Richard Dawkins and Stephen Jay Gould and Richard Lewontin and E. O. Wilson. These paired evolutionary biologists disagree with each other about a lot of things: the data, the implications of data, the nature of science, and the social responsibilities of scientists. Gould and Lewontin are allies, both scientifically and politically. Both opt for a hierarchical view of evolution and profess allegiance to some sort of Marxism. In doing so, they acknowledge (as Marxists should) that their scientific and political beliefs "interpenetrate" each other, to use an embarrassingly androsexist term. One major objection they raise is that other scientists are naïvely unaware of this interpenetration. In saying so explicitly, they have transgressed one of the most fundamental and widespread metalevel beliefs about science—that it is value free.

Dawkins and Wilson are allies in their battles with Gould and Lewontin, but they also compete with each other professionally over which direction sociobiology should take and who should lead the movement. The use of the term "sociobiology" is only one element in this contest. Because this term belongs to Wilson, he and his followers use it extensively; Dawkins and his followers do not. Dawkins and Wilson can hardly be considered Marxists. Gould and Lewontin portray them as apologists for the capitalist status quo and all the evils that it entails, possibly unconscious apologists but apologists nonetheless. Lest the correlations seem too pat, however, one of Dawkins' most effective allies, John Maynard Smith, is a first-rate evolutionary biologist with Marxist credentials at least as impressive as those of Gould and Lewontin.

While these four biologists are familiar to many, Ruse's final two subjects, Geoffrey Parker and Jack Sepkoski, are anything but household names. They are both first-class scientists, but they shy away from talking to reporters or publishing in more popular outlets. Parker spends his days following cows around in their pastures so that he can study the colonization of their pats by dung flies. Sepkoski collects and summarizes data, generated largely by others, on the occurrence of fossils to see what patterns he can find. Neither man has any enthusiasm for notions of progress, nor are they in the least interested in substituting science for religion. They publish technical papers in technical journals, and that is pretty much that. When we turn to the questions that led Parker to study these unglamorous insects, however, the story shifts significantly. He uses dung flies to study such socially charged topics as sexual selection and parent-offspring conflict. Parker might not extrapolate from his findings on dung flies to human beings, but others do. Except for the fear that we are currently participating in yet another mass extinction, Sepkoski's work does not bear very directly on the human weal.

Looking back on the history of evolutionary biology as exemplified by this dozen or so biologists, Ruse sees a steady increase in the influence of epistemic factors from Erasmus Darwin to Jack Sepkoski and a corresponding diminution of cultural factors. Yet to make these extrapolations from his data, Ruse has to introduce another consideration—professional versus popular science. Present-day scientists exclude reference to the cultural from their professional publications, reserving it, if used at all, for their more popular writings. Most of the evolutionary biologists that Ruse discusses have published at least some popular works. Instead of being thankful for scientists like Steve Gould, Richard Dawkins, Paul Ehrlich, and Carl Sagan for taking so much time away from their professional pursuits to educate the general public, we "Saganize" them. Nor is Ruse himself immune. After all, *Mystery of Mysteries* is itself written for a wide audience.

Ruse can be sure that his fellow professionals, from the safety of their isolated areas of expertise, will not be kind. Some will claim that Ruse's attempt to evaluate the merits of the various sides of the science wars by recourse to data simply shows which side he is on. More reasonably, they may note a sampling of a dozen scientists over a 200-year period does not seem adequate to Ruse's task. Perhaps if he had included yeoman scientists in Darwin's day, he would have found a different pattern. Ruse acknowledges this potential criticism and responds by challenging others to do better. The readers of Ruse's spirited and ambitious book get to enjoy one more salvo in the science wars.

PERSPECTIVES: PALEOCLIMATE

Ice Age Temperatures and Geochemistry

Edouard Bard

How harsh was the last ice age? This issue is not merely a historical curiosity, because the climate during the last ice age is a test bench for general circulation models (GCMs), which are ultimately used to predict the forthcoming greenhouse warming. Indeed, the last glacial maximum (LGM) was quite different from modern conditions, and the drastic changes that occurred at that time in the complex atmosphere-ocean-biosphere system can no longer be considered simply as small departures from the present-day climate. Moreover, the LGM occurred around 21,000 years ago, which is recent enough to allow us to retrieve reliable climatic information from suitable records. For example, the composition of the atmosphere can be obtained from polar ice cores, and the chemistry of deep ocean waters can be derived from deep-sea sediments.

During the last few years, numerous high-resolution climate records have shown that the last ice age was far more variable than previously considered in the framework of the CLIMAP project during the 1970s and 1980s (*1*). In particular, the last period of maximum ice volume (in a strict sense, the definition of the LGM) does not always correspond to the coldest temperatures. This is clearly the case for the North Atlantic and surrounding continents, where temperature minima occurred during periods of massive surges and melting of icebergs (so-called Heinrich events or HE) originating mainly from the Laurentide Ice Sheet (*2*, *3*). In fact, the LGM took place in the period bracketed between HE1 and HE2, two prominent events that have been precisely radiocarbon dated with accelerator mass spectrometry. The abrupt start of HE1 and the end of HE2 are dated at 15,000 and 20,400 ^{14}C years ago, respectively, as compiled recently by Elliot *et al.* (*3*), which correspond to about 18,000 and 24,000 calendar years ago when using the newest ^{14}C calibration INTCAL98-CALIB4 (*4*). This 6000-year interval, centered on 21,000 calendar years ago, can be viewed as a working definition of the LGM that enables us to gather together and compile climatic data from various records with different time resolutions. Furthermore, 21,000 ± 3000 calendar years ago agrees rather well with an independent approach based on glacio-hydro-isostatic modeling that takes into account relative sea-level curves recorded throughout the world (*5*).

Documenting the LGM climate is evidently an indirect and a posteriori process, being inherently less precise than the use of modern instruments to characterize the present-day climate. For example, there is still some debate about the magnitude of cooling during the LGM, especially concerning sea-surface temperatures (SSTs) at low latitudes and the comparison between temperatures over continents and oceans. Several very recent studies have substantially improved our knowledge of the LGM, and a coherent picture is now beginning to emerge for the tropics. These recent advances are mainly based on climate modeling performed in the framework of the PMIP project (*6*–*8*) and on paleotemperature reconstructions based on new geochemical proxies such as noble gases in groundwaters [(*9*); see panel A of figure], trace element concentrations in corals (*10*) and foraminifera (*11*, *12*), and alkenone distribution patterns in deep-sea sediments (*13*, *14*) (see panel B of figure for a summary of open ocean SSTs based on published data). The new SST estimates based on magnesium in planktonic forams (*11*, *12*) show that CLIMAP SSTs were indeed overestimated in the tropics. Moreover, the observed cooling is on the order of 2°C, which agrees with most alkenone results for the tropical zone (*13*, *14*) as summarized in panel B of the figure [additional alkenone results may be found in Rosell-Melé *et al.* and associated web site (*14*)]. Although the spatial coverage of alkenone data could still be improved, it seems that the tropical cooling was more pronounced in the Atlantic than in the Indian and Pacific oceans (*13*, *14*). A similar conclusion was previously obtained by mapping the $\delta^{18}O$ changes measured in planktonic foraminifera (*15*). This may also explain why coral data for the LGM at Barbados suggest a very low SST based on strontium concentrations (*10*).

Temperature maps for the LGM have been used extensively as boundary conditions for GCMs or as an independent data set to be compared with GCM outputs. Since the first modeling work (*16*) based on CLIMAP reconstructions (*1*), much progress has been made in improving the numerical models and in testing the simulations against paleodata. As part of PMIP (*6*–*8*), tropical temperatures were

The author is at Centre Européen de Recherche et d'Enseignement en Géosciences de l'Environnement, CNRS-Université d'Aix-Marseille III, UMR-6635, Europole de l'Arbois, 13545 Aix-en-Provence cdx4, France, and at Institut Universitaire de France. E-mail: bard@cerege.fr