Mechanisms of Advance

Science as a Process. An Evolutionary Account of the Social and Conceptual Development of Science. DAVID L. HULL. University of Chicago Press, Chicago, 1988. xiv, 586 pp., illus. $39.95. Science and Its Conceptual Foundations.

This book offers a theory of scientific progress in terms of a mechanism based on the selection of concepts, analogous to the mechanism of natural selection that drives biological evolution. Before formulating the theory, Hull gives us a history of taxonomy, panoramic for the first 2500 years and highly detailed for the last 40, and a more sketchy history of evolution theory. These histories are intended to provide evidence supporting his theory of the scientific process, or, less ambitiously, illustrations of what he has in mind.

Hull starts from the assumption that science does indeed progress. Science, he says, is one of the ways in which Western societies establish their beliefs, and "it beats all other ways hollow. There is no contest." He does not at this stage tell us what science is, or whether he thinks there are limits to the fields in which it can succeed. Many people in Western society hold beliefs about such things as the sanctity of human life or the importance of human freedom: it is not obvious to me that these beliefs are based on science. However, I do agree that science has been extraordinarily successful, and that it progresses, in a way that theological belief or artistic achievement do not. One would like to know why.

There are certain theories about the nature of science that cannot account for progress in understanding. Externalist theories holding that scientific views merely reflect class, sex, or racial status clearly cannot. Nor can the theories of Thomas Kuhn, according to which major changes in scientific opinion occur as the result of "paradigm debates," whose outcome is decided by eloquence and political skill. The fact that Kuhn himself accepts that scientific progress occurs does not alter the fact that, if his account of the nature of science were correct, it would not. I think that Hull would agree with these assessments of externalist and Kuhnian theories, although he would not express them so succinctly (or, perhaps, so crudely). An important question is whether his own theory escapes the relativist trap.

Hull's thesis is that there is an analogy between science and biological evolution: both are examples of a selection process. (He would not like the word analogy, because he sees the statement "A is analogous to B" as implying that A is less fundamental than B. I do not intend this. I mean only that there is a formal resemblance. I am reluctant to use the word isomorphic, because the claim is of formal resemblance, not formal identity.) There are, he argues, conceptual lineages, which change as a result of selection within and between lineages, just as evolution occurs by selection within and between populations. Brains, books, and magnetic tapes are the vehicles that carry the competing concepts, just as organisms are vehicles that carry competing genes. This view requires him to argue—reasonably, I think—that conceptual systems such as Darwinism or cladism do not have essences, any more than species have essences: at any one time, there will be a set of ideas most of which are held by most Darwinists, but there is no essential core of ideas that does not change. Unchanging essences are incompatible with evolution by natural selection.

But what is special about science? Would not the above description apply equally well to the theological schools that competed and replaced one another in the Byzantine church? It is at this point, I think, that difficulties begin to arise. Hull argues that the structure of science is such that behaviors that benefit the institution as a whole—for example, early publication, citation of others' work, honesty—also benefit individuals: virtue and self-interest go hand in hand. But why should this be any more true of science than of theology or politics? I do not think that analogies with biology help us much at this point. Thus there are, in biology, situations in which separate "vehicles" cooperate for the good (survival and reproduction) of some larger whole: cells cooperate in organisms, and sterile workers cooperate in ant colonies. The explanation for such cooperation is that the replicators (genes) in the cells are identical (or, in the case of workers in a colony, similar) in different members of the group and will be transmitted to future generations only insofar as the group as a whole, organism or colony, is successful. Now an analogous argument might explain the loyal cooperation of the members of a tightly knit research group, but would explain equally well the cooperation of the members of a religious sect or of a group bound together by a common political or artistic program.

The essential difference, of course, is that ideas in science are subject to experimental test, whereas those in other fields are not. It does not pay to lie, because you will be found out. Hull would accept this: he refers to the importance of "checking" ideas. But it is odd what a small part this plays in his account of science. His whole emphasis is on the social structure of science: this structure is important to him for the same reason that population structure is important to an evolutionist. This bias in his work is pervasive. I can give only one example, but it is an important one, because it concerns the outcome of the battle between the cladists and the pheneticists, which is the core of the historical evidence he reviews in the first part of the book. In explaining the success of cladism, he offers two explanations: the pheneticists "branched out too quickly," and cladism "appeared to be promising to systematists no matter the stage of their career." If such essentially sociological *kinds* of explanation were correct, scientific progress would be incomprehensible or meaningless. We would be back with relativism and the Byzantine church.

There is another difficulty in explaining scientific progress by a selective mechanism: most evolutionary lineages show no obvious sign of progress. They change, but that is all. An obvious explanation, which would be in the spirit of Hull's book, is that scientific concepts are faced with an unchanging selective requirement—correspondence to a nature that is unchanging, at least in the respects that science attempts to explain. In contrast, species are faced with ever-changing selective requirements, presented by their competitors, their predators, and their prey. Perhaps a more fundamental difference is that science is a single enterprise in a way in which an evolving ecosystem is not. In biology, the horizontal transfer of genetic material between lineages is probably rare, and hybridization occurs only between close relatives. If this were not so, cladism would be a hopeless enterprise. In the history of ideas, distant hybrids are common and crucial. Hull is aware of this difficulty but argues that "cross-lineage borrowing . . . does not seem to be as common as all the talk about syntheses would lead us to expect." I find him unconvincing on this point. For example, he argues that those who produced the merger between Mendelism and Darwinism "were not very familiar with some of the theories they were merging." This seems an odd remark to make about Haldane and Fisher. To give other examples, my own best-known contribution to biology has been to merge ideas from economics (game theory) and evolutionary theory. On a larger and more important scale, molecular biology arose from the merging of genetics, microbiology, and several threads from the physical sciences. On a still larger scale, a fundamental feature of science is the requirement of consistency between disciplines: we could not tolerate a situation in which biologists supposed that the laws of chemistry were different from those accepted by chemists. It is for this kind of reason that, at best, there may be an analogy between scientific change and evolution, but not an isomorphism.

Hull places much emphasis on the infighting and political maneuvering that go on in science. On several occasions, he refers to scientists as having such motives as a desire to "get that son of a bitch." I cannot help wondering how far this emphasis arises from the accident that he took as his study material the behavior of taxonomists, but doubtless people in all branches of science, and in all walks of life, are sometimes motivated by personal animus. Where I disagree most strongly is with his suggestion that such animus may help the process of discovery, by providing the necessary motivation and creating the competition needed if selection is to be effective. I think this is nonsense, and perhaps dangerous nonsense. I accept that disagreements are inevitable and that, when they arise, it is valuable that the different views be expressed as clearly as possible. If, as I think is the case, the phenetic and the cladistic approaches to taxonomy are incompatible, it is important that this should be stated openly, and not fudged. But I see no reason for personal ill feeling. Much of my own work was stimulated by disagreement with Wynne-Edwards, but I have always admired and respected him and have found rational discussion with him a possibility.

Why should personal feelings matter? Essentially, because once a scientist's ego gets over-involved in an argument, he or she is unlikely to admit to being wrong, and unlikely to see any merit in an opponent's case. Since, in most serious debates, there is some sense in what both sides are saying, too aggressive a personal involvement may delay a correct resolution, and may condemn some individuals to a lifelong commitment to an erroneous position. The opinions of the biometricians and the Mendelians were incompatible, but the resolution contained elements of both views: the participants were prevented from seeing this by personal animosity. Thus I agree that personal animus plays a role in science, as elsewhere, but I think it is almost always counterproductive. It is valuable that scientists should discuss their disagreements, because this is the best way of identifying where the difference lies and how it might be settled. But my experience suggests that this is best done in very small groups, when egos are less likely to be involved, or in print, because in print one has time to think of a dirty crack, and then suppress it in the interests of understanding, whereas in debate it is likely to slip out. Large confrontational meetings are usually a waste of time. Hull refers to the macroevolution meeting in Chicago as having become a watershed in evolutionary biology as a result of the opportunity it gave Roger Lewin to write a tendentious account in *Science*. I cannot imagine why he thinks so. Those of us who like a row enjoyed it, but no issues were clarified and none of the participants changed their minds, or even learned very much. Kuhn's *Structure of Scientific Revolutions* had the unhappy effect of convincing some young scientists that the best way of persuading people that one was the inventor of a new paradigm was to misrepresent one's opponents and to be incomprehensible: then one would be seen to be involved in a paradigm debate. It would be sad if Hull's book were to convince the next generation that they should aim to be obnoxious.

No one could read this book without learning many interesting facts and meeting many persuasive arguments. More often than not, I found myself agreeing with Hull's judgments. But although I enjoyed a lot of the details, I am not persuaded by the picture as a whole. Much the most interesting thing that happened in taxonomy in the period since 1950 was the work of Hennig, carried out largely in isolation in East Germany. To the extent that the cladists defeated the pheneticists, they did so because they had the sounder argument, and Hennig had provided it for them. Hull would no doubt reply that Hennig would have had no effect on science if it had not been for the enthusiasm and political skill of his supporters at the American Museum. I am not convinced. Indeed, Hennig's ideas might have spread more rapidly if they had been propagated less abrasively. In any case, they would not have spread at all if they had been wrong.

J. Maynard Smith
School of Biological Sciences,
University of Sussex,
Brighton BN1 9QG, United Kingdom

Farm Problems

The Law of the Land. Two Hundred Years of American Farmland Policy. John Opie. University of Nebraska Press, Lincoln, 1987. xxii, 231 pp., illus. $25.95.

John Opie's survey of American land policies provides the context for his provocative, learned, and polemical contribution to the debate on the nature of the farm problem and the means to solve it. Throughout our history, Opie, a historian, convincingly argues, contradictory goals have produced contradictory policies that are the sources of our current problems.

In the earliest years, when available land seemed limitless, the goal of using the public domain to create a nation of working farmers required making it available in small parcels at low prices on easy credit terms. But the goal of using it to provide government revenues and to finance public improvements required selling land in large parcels at high cash prices and granting large tracts to companies that would build canals and railroads. The contradiction was never resolved; instead both policies were carried out simultaneously. The government gradually reduced land prices and the minimum size of tracts to be sold until, under the 1862 Homestead Act, a settler could get a small parcel by paying only a small registration fee. But at the same time buyers could continue to purchase land in lots as large as they could afford, which, together with huge land grants to railroads, resulted in a vast acreage becoming unavailable to homesteaders.

As public lands rapidly fell into private hands—in five rather than the hundred generations that Jefferson had envisaged—new conditions and new problems required policy changes, but once again contradictory goals produced contradictory policies. Industrialization and urbanization created a growing non-agricultural population that demanded abundant and cheap food, a demand that farmers supplied but often at great personal and social cost. Smaller farmers who found it impossible to compete lost their lands to larger producers who often mined the soil seeking the largest output at the lowest cost even when the long-term effect was deteriorating farm land; and when farmers moved into the high plains beyond the 100th meridian they began cultivation on lands with insufficient rainfall. Public irrigation policies designed to promote settlement and to continue production of cheap and abundant food contradicted policies that allowed water to be diverted to meet the growing urban demand. Non-agricultural users could afford to pay high