Popper on Darwinism

W. D. Russell-Hunter, in his letter (17 Apr., p. 281) commenting on William J. Broad's excellent article on the recent creation-evolution trial in Sacramento, California (News and Comment, 20 Mar., p. 1331), suggests that Karl Popper in a letter to *New Scientist* (*1*) has recanted the following in *Unended Quest* (*2*):

> From this point of view the question of the scientific status of Darwinian theory—in the widest sense, the theory of trial and error-elimination—becomes an interesting one. I have come to the conclusion that Darwinism is not a testable scientific theory, but a *metaphysical research programme*—a possible framework for testable scientific theories.

I think, however, that one must carefully read the fairly detailed development of Popper's ideas in *Unended Quest* and fully appreciate the cautious wording of his "recantation" in *New Scientist*.

In the former work Popper did not deny "scientific character" to Darwinism, although he reiterated that "it is therefore important to show that Darwinism is not a scientific theory, but metaphysical" (*3*). He went on to aver that "its value for science as a metaphysical research programme is very great, especially if it is admitted that it may be criticized, and improved upon" (*3*).

In his letter to *New Scientist*, Popper does admit that the "historical sciences . . . can very often be tested by deriving from them testable predictions or retrodiction" (*1*). This is certainly true for some of the "testable scientific theories" which have been developed within the framework of the Darwinian "metaphysical research programme." But it is not true of the general theory of evolution, the hypothesized common descent of all life which Darwin repeatedly identified as the idea which must be preserved at all costs in order to extirpate from the minds of scientists and nonscientists those dual concepts which he so intensely hated, that is, divine intervention and special creation.

The failure of evolutionary theory to make testable predictions is widely acknowledged, as Broad pointed out. As for retrodictions, can they provide the basis for crucial testing or for conclusive falsification of the general theory of evolution? The answer is no. Failure to find some type of retrodicted data can always be explained away, and often has been. Popper did not affirm in his letter to *New Scientist* (and I predict that he never will), "Darwinism is a falsifiable theory of empirical science." And I suspect that very few evolutionary scientists themselves believe this to be so in their heart of hearts.

ROBERT E. KOFAHL
Creation-Science Research Center,
Post Office Box 23195,
San Diego, California 92123

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1. K. Popper, *New Sci.* **87**, 611 (1980).
2. _____, *Unended Quest* (Open Court, La Salle, Ill., 1976), p. 168.
3. _____, *ibid.*, p. 172.

When I learned from Broad's article that the creationists claim support from Sir Karl Popper for their claim that Darwin's theory of natural selection is not science, I wrote to Sir Karl, whom I have known ever since common Vienna days. He promptly sent me a copy of his paper, "Natural selection and the emergence of mind" (*1*), with a reference to page 344, where he marked the following passage in the margin:

> The fact that the theory of natural selection is difficult to test has led some people, anti-Darwinists and even some great Darwinists, to claim that it is a tautology. . . . Since the explanatory power of a tautology is obviously zero, something must be wrong here. . . .
>
> I mention this problem because I too belong among the culprits, influenced by what these authorities say. I have in the past described the theory as "almost tautological" and I have tried to explain how the theory of natural selection could be untenable (as is a tautology) and yet of great scientific interest. My solution was that the doctrine of natural selection is a most successful metaphysical research programme. It raises detailed problems in many fields, and it tells us what we would accept of an acceptable solution of these problems.
>
> I still believe that natural selection works in this way as a research programme. Nevertheless, I have changed my mind about the testability and the logical status of the theory of natural selection; and I am glad to have an opportunity to make a recantation.

and later Sir Karl sums up:

> The theory of natural selection may be so formulated that it is far from tautological. In this case it is not only testable, but it turns out to be not strictly universally true. There seem to be exceptions, as with so many biological theories; and considering the random character of the variations on which natural selection operates, the occurrence of exceptions is not surprising.

HANS ZEISEL
Law School,
University of Chicago,
Chicago, Illinois 60637

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1. K. Popper, *Dialectica* **32**, 344 (1978).

Fraud, Science, and Safeguards

The article by William J. Broad on fraud and the structure of science (News and Comment, 10 Apr., p. 137) was timely but perhaps a bit too bearish on the current state of morality in research. Any deliberate fudging of the data for personal aggrandizement is to be deplored whenever it occurs; however, given the huge increases in the number of persons doing research, I do not think that the relative frequency of instances of fraud has increased. My guess would be that the safeguard mechanisms are working adequately and that the relative frequency is, if anything, lower than in earlier periods. It would, of course, be good to have accurate data.

A corrective perspective is to view fraud in science in the context of fraud in other areas of endeavor, especially commerce and the professions. When placed in the context of escalating malpractice suits and the clamor for consumer protection agencies and legislation, and the sorts of incidents that have produced these trends, one must conclude that scientists have managed to maintain high ethical standards in a society where personal integrity as a cherished virtue is rapidly disappearing. It is to be hoped that the response of the scientific community to lapses of honor among researchers can serve as a model and an inspiration for other areas of endeavor to "clean up their act." It is easy to lose perspective when one focuses exclusively on individual acts of fraud and to come up with recommendations for corrective measures which may not, in fact, be needed or useful. If something "ain't broke," don't fix it. It is not clear that the standards of scientists need fixing beyond regular maintenance.

EDITH D. NEIMARK
Department of Psychology, Douglass College, Rutgers, State University of New Jersey, New Brunswick 08903

A paper by B. Miller (*1*) sheds an interesting light on the self-correcting mechanism of science referred to by Broad. Miller describes how an arbitrary adjustment of the data was made in a well-known investigation of turbulent flow in pipes (*2*). This was not a case of fraud. The report had a table giving the actual measurements of velocity and position and a second table and two curves giving the adjusted results in dimensionless form but did not mention the adjustment or its rationale.

Although this is one of the most widely cited references in the field and the data

in question was the basis of the laminar film hypothesis, it was 14 years before the question of the discrepancy between the two tables was raised.

The self-correcting mechanism may have worked, but it was rather slow. I suspect it might be even slower in this era of mission-oriented research, where financial support to check other people's results would be hard to get.

WILLIAM SQUIRE
Department of Aerospace Engineering, College of Engineering, West Virginia University, Morgantown 26506

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2. J. Nikuradze, *VDI-Forschungsh.*, No. 361 (1932).

Evaluating the Social Sciences

As a sociologist, I appreciate William D. Carey's defense of the social sciences in his editorial of 1 May (p. 497). However, it might be worth a pause to consider what, in fact, the social sciences have contributed toward answering "questions that need and are worth studying." I am afraid the record is a poor one.

To put the question properly, we must acknowledge that a great deal of what we know about important social problems has been contributed by people who are not social scientists. These include novelists, journalists, political activists, and perhaps even one or two politicians. What, *in addition*, has been contributed by professional social scientists?

If you ask several different social scientists this question, you will, as usual, get several different answers. (Perhaps what we contribute is a diversity of viewpoints.) We do not have any theories that allow us to predict events with more accuracy than intelligent laymen. Nor do we have any theories that allow us to construct better social systems—schools, police forces, cities, nations—than can be constructed by laymen. Most of us do not understand the society well enough to extract a particularly good living from it, compared to other Ph.D.'s, and we do not even understand microsocieties to the extent that sociology departments run better than other academic departments.

This is not to say that we are wholly ignorant of society and social behavior. To the contrary, we understand these things fairly well. The difficulty is, so does everyone else. I suspect that if we made a list of social problems and then discussed them with random samples of sociologists, physicists, and journalists, the sociologists would not come out any better than the other groups. If we went to sociologists *specializing* in these problems and asked them to predict relevant events over, say, the next 2 years, then I bet they would not do any better than a group of journalists who wrote about these problems. If we asked sociological specialists to suggest solutions to these problems, then I bet (i) that they would not agree among themselves, and (ii) that their suggestions would not look particularly good.

I would not go so far as to say that professional social science has made no contribution at all, but what has been made is a little hard to find.

Do not expect too much from the social sciences. We are trying but it is very hard work. Perhaps, in a few years, we will have better theories and make better predictions and design better systems. Perhaps not. It would be nice to have research support so that we can continue trying.

ALLAN MAZUR
Department of Sociology, Syracuse University, Syracuse, New York 13210

OSHA Policy for Laboratories

I wish to call the attention of *Science* readers to a request for information related to protection of laboratory workers from toxic exposures. The Occupational Safety and Health Administration (OSHA) needs additional information in order to make initial policy decisions on what regulatory approach would be most appropriate for laboratory work with toxic chemicals.

In the *Federal Register* of 14 April 1981 we specified some of the issues OSHA will need to consider, including the basic issue of whether either (i) a general mandatory laboratory standard or (ii) general laboratory guidelines which would be mandatory only for general industry would be preferable to the substance-specific health standards for general industry that are currently applicable to laboratories. Other options which may be suggested and are consistent with the Occupational Safety and Health Act will also be considered.

We are also seeking comment, including economic and technological feasibility information, on a number of other issues. These include exposure monitoring, medical surveillance, ventilation specifications, and certain work practices for handling toxic chemicals.

Requests for a copy of the *Federal Register* notice may be sent to Lucile Adamson, Room N3718, Department of Labor–OSHA, 200 Constitution Avenue, NW, Washington, D.C. 20210.

We hope that the laboratory community will take advantage of this opportunity to contribute to national policy development in this area.

BAILUS WALKER, JR.
Health Standards Programs, Occupational Safety and Health Administration, Washington, D.C. 20210

More History of Calculus

In a recent letter (20 Mar., p. 1258) Kondo points out that a Japanese mathematician developed the calculus independently of, and probably somewhat earlier than, Newton and Leibniz.

Kondo does not mention the much publicized recent discovery of documents in the Cistercian monastery of Stams in Tirol which makes it virtually certain that priority in developing the calculus belongs to a brilliant Austrian monk, Johan Jakob Tschurtschenthaler (1630–1670) from Thaur, a small village near Innsbruck. Tschurtschenthaler, a farmer's son, had run away from home and was educated by the enterprising brethren of the Stams monastery, who quickly realized the intellectual powers of the young boy. The novice showed his gifts by constructing elaborate wind-driven waterworks, a famous clock, and many other mechanical devices. He also invented a new method of triangulation which was used for measuring the heights of mountains with hitherto unknown precision. Tschurtschenthaler was a very skilled mathematician but he seems to have been interested mainly in practical applications of this science. There can be no doubt that his manuscript "Eyne eynfache Methode mithülfe der Gnade des Allmächtigen aus dem Allerkleinsten das Allergrösste zu kalkulieren," which can be dated to the year 1660 or 1661, contains all the ingredients of the calculus as they were developed independently by Newton and by Leibniz a few years later. There is no indication that the manuscript ever left the Stams monastery. Tschurtschenthaler died (by drowning) before he could find any practical use for his ideas.

W. WIESER
Department of Zoology, University of Innsbruck, A-6020 Innsbruck, Austria