LETTERS

OSHA Policy on Carcinogens

A recent misunderstanding of the law and policy under which the Occupational Safety and Health Administration (OSHA) functions regarding carcinogens requires clarification. News and Comment briefings in the issues of 2 July (p. 35) and 16 July (p. 233) suggest concern that OSHA requires epidemiological data on humans before acting to regulate carcinogens in the workplace. This is not the case. As I stated in reply to a query about our cancer policy from Donald Millar, director of the National Institute of Occupational Safety and Health,

> I appreciate the opportunity and the need to clarify the Occupational Safety and Health Administration's (OSHA) regulatory policy on carcinogens in response to your letter of June 15, 1982.
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> In your letter you asked whether OSHA requires evidence of cancer in humans in order to promulgate regulations to control occupational exposure to a carcinogen. The answer to your question is, of course, no. OSHA does not require carcinogenic evidence in humans to promulgate standards. OSHA may promulgate standards for carcinogenic substances when animal evidence alone is available.
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> The intent of my May 13 letter, however, was to indicate that OSHA cannot promulgate a regulation for a carcinogenic substance solely because the substance has been *identified* as a carcinogen, based either on human or animal evidence. According to the Supreme Court's decision in the benzene case, before OSHA can promulgate any permanent health standard, the Secretary of Labor is required to make a determination that a place of employment is unsafe in the sense that a significant risk to workers is present, and that this risk can be eliminated or lessened by the promulgation of a standard or a change in a standard. In that case, benzene was a proven human carcinogen based upon human evidence. Nevertheless, the Court vacated OSHA's benzene standard because OSHA did not meet its statutory burden to show that long-term exposure to benzene, at the levels encountered in the workplace, presented a significant risk of material health impairment.
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> Thus, it is incumbent upon OSHA to demonstrate significant risk in the promulgation of a standard. We believe that it is possible to meet this burden by evidence derived from studies of either animals or humans. Some of the factors that must be considered in making the determination of whether a risk is significant are the following: the strength of the evidence of carcinogenicity; the number of workers exposed to the substance; the levels to which the workers are exposed; the best estimates of exposure levels associated with potential tumor induction in man or animals; and the molecular similarity to other known carcinogens. When making this determination, OSHA considers all reports, studies and other evidence and encourages all segments of the public to participate in the rulemaking proceeding.
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> OSHA values the opinions both of the International Agency for Research on Cancer and the National Institute for Occupational Safety and Health. Their and your expert opinions are very useful to us in our standard setting activities. As I noted above, we cannot promulgate a standard solely because there is evidence of carcinogenicity, but we must demonstrate that there is a significant risk to workers covered by the Occupational Safety and Health Act from workplace exposure to a particular substance.
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> OSHA looks forward to continuing exchanges with you on these issues of mutual concern as we both strive to reduce or eliminate workplace hazards. We hope to be able to work even more closely with you in the future to provide suggestions and advice to you regarding OSHA's research needs, just as you provide suggestions to us regarding our regulatory activities.

I hope this will clarify the inadvertent confusion that has been engendered about this matter.

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The Adaptationist Program

Roger Lewin (Research News, 11 June, p. 1212) describes the "problem" that Stephen Jay Gould and Elizabeth Vrba (*1*) have with the "adaptationist program." A new term is suggested by Gould and Vrba "because one thereby recognizes as important a phenomenon that modern evolutionary theory has neglected." Later in the article, however, Lewin writes that this phenomenon "traditionally has been described as preadaptation." Obviously preadaptation has not been neglected at all. Any comparative behaviorist can supply examples. This is classical Lorenzian ethology [see almost any textbook of animal behavior; for example, (*2*)]. Quite commonly the point has been made that today's adaptation probably evolved from yesterday's behavior or structure having either a different function or no obvious function per se. The comparative method, the method of Gould and Vrba, has been a staple of evolutionary biology for more than a century. The conclusions of Gould and Vrba are only semantically different from those of most comparative ethologists.

There is, I suggest, a basic difference in training and outlook that separates paleontologists and others employing the comparative method from behavioral ecologists and others who frequently employ the adaptationist program. The former are students of the major trends in form and function that characterize higher taxonomic categories. These are largely matters of the past. The latter workers study small changes in the present. Although today's small change can be tomorrow's major change, retrospectively, there is a fundamental difference in methodology between these two groups of evolutionary biologists that seems to be partly responsible for the difficulty in communication between Gould and most modern evolutionary biologists. Only adaptationists study ongoing processes of natural selection by correlation and controlled experiments on natural populations, thus allowing them to use both weak and strong inference methods of testing hypotheses.

Gould's "assault" on the adaptationist program appears to be basically quixotic. Adaptationists employ their program to *generate hypotheses*, most of which they themselves reject. Of course, some of these may be as amusing as Kipling's story of how the elephant got its trunk; but these tend to be rejected early. Critics of the adaptationist program put themselves in the position of attacking a method of generating testable hypotheses simply because it generates some ludicrous hypotheses that become rejected. The same criticism can be made of the scientific method in general. I suggest that they are attacking the wrong target.

Paradoxically, those conclusions that Gould and Vrba favor, namely the existence of functionless behavioral and structural details, stem from hypotheses generated by the adaptationist program; for surely it is only after hypothetical functions have been rejected that the concept of nonfunction gains legitimacy. Similarly, the conclusion of altered function also was reached using the adaptationist program, since only by rejecting alternative paleofunctions and neofunctions does the hypothesis of altered function become tenable.

The stated enemies in Gould's polemic are holders of "the view that virtually every aspect of an organism is a specific adaptation for some function." Who holds this position? Apparently nobody. At any rate, Lewin does not solicit a rebuttal from them. This straw man is certainly not a fair statement of the adaptationist program, for the latter is a method of generating hypotheses based on our knowledge of the processes and limitations of natural selection. Of course, the program does not imply that a given hypothesis derived from it must be correct, nor does it deny the hypothesis of selective neutrality.

To judge from the attention given by the media to Gould's "assault" on evolutionary biologists who study the present rather than the past, selection in natural populations instead of fossils, and variation among individuals rather than among species and higher taxa, one would think that all the major issues in evolutionary biology concern fossils. In fact, most evolutionary biologists today are not paleontologists. In fact, most of us use the adaptationist program. Most evolutionary biologists work at generating and testing new theories and hypotheses.

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References

1. S. J. Gould and E. Vrba, *Palaeobiology* **8**, 4 (1982).
2. J. L. Brown, *The Evolution of Behavior* (Norton, New York, 1975), pp. 282–328.

Explaining Meteorites

In her briefing (News and Comment, 25 June, p. 1390) reporting the first meeting of the Society for Scientific Exploration, Constance Holden quotes me as stating that a meteorite fall in 1790 convinced the scientific community that meteorites existed. Quite the contrary, I described how reports of this event were ridiculed by scientists of the time. It was, of course, the L'Aigle fall of 1803 that established meteorites as real.

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A featured presentation at the first meeting of the Society for Scientific Exploration was a talk entitled "The meteorite question" delivered by Ron Westrum of Eastern Michigan University. The author described the scientific community as being closed-minded about controversial issues. We may be reacting today with regard to UFO's, sea serpents, and so forth, with the same arrogance that prevented late-18th-century scientists from accepting reports that meteorites actually fell from the sky. His talk was excerpted from a paper (*1*) published several years ago.

Westrum has read the original literature of the meteorite controversy and has his facts in order. Instead of an insightful analysis, however, he produces an indictment: if today's scientists continue in the pattern of individuals who played formative roles in the development of major areas of modern physical, chemical, and geological sciences, they will seriously retard progress. Westrum does not acknowledge that the individuals who worried about the reality of meteorite falls at the end of the 18th century faced a difficult intellectual challenge. The development of a conceptual framework for the recognition of meteorites within a period of two decades was a significant scientific achievement. Those who participated should not be disparaged because the problems they faced 200 years ago can be made to appear trivial today.

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1. R. Westrum, *Soc. Stud. Sci.* **8**, 461 (1978).

Marine Biology on Palau

Last February, while research scientists in North America were enjoying less-than-clement climatic conditions, a group of us were engaged in biological research (specificially, studies of symbiotic prochlorophytes) in and around the coral reefs and shoals of Palau, in the West Caroline Islands. On Palau, seawater and air temperatures remain around 30°C throughout the year, marine organisms can be found in a variety and abundance that can be matched by few other areas, and laboratory and dormitory facilities are available at the Micronesian Mariculture Demonstration Center. We were surprised that the laboratory, which needs support of all kinds, is not used by more marine biologists. With the formation of the Palau Marine Research Institute (PMRI), and its recent incorporation by the Republic of Belau, we hope there will be a resurgence of activity there, at least comparable to that of the period between the wars, when Palau was under Japanese administration. Readers interested in working there (with or without student associates, for several days, weeks, or months) who wish information on individual or institutional memberships in PMRI, may write Keith E. Chave, President, PMRI, c/o Department of Oceanography, University of Hawaii, Honolulu 96822.

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Paleoglaciology

In his review (14 Aug. 1981, p. 752) of *The Last Great Ice Sheets* (*1*), Charles R. Bentley claims that in the book "Strange physical and geophysical ideas are stated as facts. Examples of such ideas are that heat required for melting at the surface of a glacier is partly conducted upward from its frozen bed (that is, against the thermal gradient) and that radioactive heating in the continents could cause isostatic response to the continental ice sheet to occur by flow within the crust rather than within the mantle."

The first example refers to the discussion on pages 224 and 225, which concerns cold ice sheets, not temperate glaciers. The bed is newly frozen, so it is at or just below the melting point year-round. The surface ablation zone, on the other hand, has a mean annual temperature well below the melting point and this is the year-round temperature 10 to 15 meters below the surface. Heat is conducted continuously from the bed to this near-surface depth, so more heat is conducted upward to the surface in the winter and less heat is conducted downward from the surface in the summer than would otherwise be the case. More heat is therefore available at the surface for summer melting.

The second example refers to the discussion on pages 253 and 254, which cautions against the common glaciological practice of using a granitic rock density in computing isostatic sinking beneath ice sheets. Flow within the crust must occur if granitic densities are used, and we used basaltic densities to insure that flow will be within the mantle, not the crust. The only way in which crustal flow could occur would be if radioactive heating made crustal granites softer than mantle basalts.

The review goes on to say that isostatic adjustments confined to the crust "appear to have been used in an argument against the occurrence of bedrock depression beyond the margins of the ice sheet and thus in favor of a maximum, rather than a minimum, mode of ice sheet extent." Lateral flow in the mantle combined with crustal bending is assumed in making this argument (pages 312 and 313).

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References

1. G. H. Denton and T. J. Hughes, Eds., *The Last Great Ice Sheets* (Wiley-Interscience, New York, 1981).