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 pres-