Letters

Good Science Advice

I write to applaud and second Daniel E. Koshland, Jr.'s, editorial on providing science advice to the President (16 Dec., p. 1489). Appropriate science advice to the President is an essential aspect of modern government for any civilized society today. Koshland's editorial suggests one indispensable reason for this requirement; since a large percentage of scientific research money is provided through public resources, both the government and the scientific community benefit from a close relationship between the President and his science adviser.

There is, however, another reason that government leaders need good science advice. Urgent national and global issues such as the greenhouse effect, depletion of nonrenewable energy resources, population growth, species extinction, and environmental pollution may be amenable to scientific solutions. Each of these issues demands the time, talents, and funding of numerous scientific disciplines.

However, this picture is complicated by the very potential of modern science. It is as though we are standing on the proscenium of a monumental stage. The curtain is about to rise and reveal an understanding of the natural universe that we can only dimly perceive. Projects to map the human genome, build a superconducting super collider, monitor the earth with an array of satellite-based sensors—these are but a few exciting scientific endeavors that await us, and it would be easy to expand this list. Once again, the needed ingredients for their pursuit are the time, talents, and funding of numerous scientific disciplines.

Therein lies another compelling reason for good scientific advice at the highest levels of government. Our society faces an abundance of exciting opportunities for scientific investigation. We are also faced with an abundance of societal problems requiring scientific solutions. Given constraints imposed by federal budget deficits, however, we cannot afford to fund all scientific endeavors now luring us. Priorities must be established, and the scientific community must be involved in this process.

It is vital for the scientific community to become involved in the *political* process of assessing priorities for the support of scientific research. I choose the adjective "political" carefully, for assessment of research priorities involves forging of consensus among a variety of constituencies and requires many nonscientific considerations. Several commentators (*1*) have noted that this assessment must involve consideration of values in three areas: scientific merit, social value, and feasibility and resource consumption.

Thus, I applaud the thrust of Koshland's editorial. It is imperative that the office of presidential science adviser be upgraded so that he or she has close access to the President. That person faces two immediate tasks: one is to develop mechanisms whereby standardized procedures can be developed for assessing various values in scientific projects competing for federal support; the second is to find ways to involve the scientific community in this assessment. As Koshland correctly notes, this person must be accessible to the scientific community. But that accessibility implies an obligation on the part of the scientific community to become fully involved in the political process of making hard, and sometimes difficult, decisions regarding the relative merits of projects competing for scarce funding.

GEORGE E. BROWN, JR.
House of Representatives,
Congress of the United States,
Washington, DC 20515

REFERENCES

1. J. A. Dutton and L. Crowe, *Am. Sci.* **76**, 599 (1988); A. M. Greenberg, *Minerva* **1**, 159 (1963).

Evolution and Family Homicide

If a parent kills a stepchild, M. Daly and M. Wilson (Articles, 28 Oct., p. 519) attribute it to the child's low "contribution to the parent's genetic posterity." If the stepchild is not killed, they attribute it to the parent's evolved need for "maintenance of networks of social reciprocity." If a parent kills a biological child, the authors say that the child must have lost out in the calculus of "strategic allocation of lifetime parental effort." If the child is not killed, it must have benefited from evolved "parental solicitude" based on genetic relatedness.

I question whether these adaptive scenarios contribute anything to a scientific understanding of family homicide. Words like "reciprocity" and "allocation" have a quantitative ring, but in fact there is nothing in Daly and Wilson's theory that permits even an approximate estimate of the frequency of any of the behaviors referred to. Thus there is no way that the theory can be falsified by checking its predictions with experiment. Almost any incidence of killing of either stepchildren or natural children could be made to fit Daly and Wilson's adaptive storytelling. If the frequency of killing natural children had turned out to be greater than the frequency of killing stepchildren, instead of the reverse, Daly and Wilson would have had no trouble "explaining" it by guessing that past selection for "maintenance of networks of social reciprocity" had been stronger than selection for "parental solicitude." Given their apparent notion of the relation between theory and observation, there is no reason to think they would have hesitated to cite such an outcome as a prediction of the theory, just as they claim actual outcome is predicted by the theory.

STEPHEN A. GEORGE
Department of Biology,
Amherst College, Amherst, MA 01002

I am sure that Daly and Wilson's article on evolutionary (that is, sociobiological) explanations for patterns of family homicide is going to be attacked by nonsociobiologists as dangerously simplistic. To ensure that all evolutionary biologists are not tarred with the same brush, and thus to ensure that evolutionary biology continues to play an important role in increasing our understanding of human behavior, criticism from practising sociobiologists is needed also.

One aim of any good presentation of a scientific hypothesis should be to show its superiority over competing hypotheses at explaining a set of facts or at predicting trends. This Daly and Wilson do not do. They present what they see as an internally consistent theory which "predicts" patterns of family homicide on the basis of, among other things, the genetic relatedness and relative reproductive value of the participants. However, they make no attempt to pit their hypothesis against obvious competing hypotheses.

As far as I can see, an economic analysis, based on the relative, nonreproductive, costs and benefits of the homicidal act itself and of the participants to one another, explains all the trends described by Daly and Wilson as consistently as does their "evolutionary" hypothesis. Small children being easier to kill than older ones, and men being able to kill more easily than women, would "predict" their figure 4, A and B, for instance. I will not spell out the economic hypothesis because the point is not to prove or disprove it, but simply to suggest that good science should not ignore competing hypotheses, especially when they can be so readily produced.

Perhaps more important, the evolutionary hypothesis is not as consistent as the authors imply. They switch, with no obvious logic, between several different types of evolutionary argument—sometimes kin selection theory, sometimes life history theory, sometimes sexual selection theory—to explain everything about family homicide. Is anything explained, therefore? Nor do Daly and Wilson indicate that their evolutionary hy-