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.

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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.

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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-

pothesis can produce conflicting predictions depending on what particular branch of evolutionary biology is used. Thus they "predict" violence by husbands against young wives (on the basis of intermale sexual rivalry over females), whereas an evolutionary hypothesis could also predict violence against older wives—on the basis of their lower reproductive value.

Again the point is not to argue for my explanation instead of theirs. In fact, I am fairly sure that some of Daly and Wilson's explanations are correct. However, in a field as sensitive as the one they address, which many will perceive as the subject of genetic control of predispositions to homicide, sociobiologists surely have a duty to be far more circumspect in their presentation of such potentially divisive hypotheses? If they are not, understanding of human behavior will suffer as sociologists, anthropologists, psychiatrists, and psychologists ignore as irrelevant what they see as the overly simplistic theories of sociobiology.

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Response: Readers of our article will not find case-by-case pseudo-explanations of homicides, as in George's caricature. What they will find is a number of novel hypotheses about demographic and circumstantial correlates of fatal conflict within families, derived from consideration of how selection is likely to have acted upon social psychologies, as well as various analyses showing that the hypothesized correlates are indeed powerful predictors of homicide risk. Risks to stepchildren, for example, had never been assessed before an evolutionary model of parental inclinations inspired us to make the relevant comparisons. Our discovery that such children are several dozen times more likely to be slain than genetic offspring seems to us to warrant serious concern rather than dismissal as "storytelling."

George asserts that anything could be "predicted by the theory" with the implication that "the theory" is vacuous. The only general theory informing our article is that of evolution by selection, and his complaint is nothing more or less than the amply refuted old chestnut that the theory of evolution can explain everything and therefore is not science. There are many models within modern behavioral ecology, some of them mutually contradictory. Some will turn out to apply to certain species or circumstances but not others, and some will fail completely. George apparently believes that the recognition of complexities arising from multiple causal factors (such as the countervailing pressures for nepotistic discrimination and for reciprocity with nonrelatives) constitutes a sort of post hoc waffling. Unfortunately, social phenomena are complex and multiply determined. They will not be elucidated by single-factor models nor by doctrinaire hostility to the use of evolutionary reasoning to generate testable hypotheses about human psychology and action.

Of course, any of our particular hypotheses might have failed. They did not, and so we discovered several previously unidentified risk factors for homicide. George suggests sarcastically that if we had found that people typically discriminate against their own offspring, we would claim to have predicted that, too. The truth is that such a discovery would be very surprising in any animal species, but would not constitute reason to disinter Darwin from Westminster before one had made serious efforts to generate potential evolutionary explanations of the unusual case, with testable corollaries.

Finally, by the narrow equation of science with "experiment," George would banish by fiat not only all studies of homicide but virtually all of sociology, epidemiology, economics, and astronomy.

Harcourt's complaint that we "make no attempt" to test our hypotheses against alternatives is baffling. We derived and tested