LETTERS

All in the family

Readers state that the U.S. National Research Council has a "direct and effective manner" and a "reputation for accuracy." Researchers discuss the "methods" and "philosophical basis" of determining "phylogenetic relationships" between living things. A senior scientist says that it is "essential" that regulators at the U.S. Food and Drug Administration have "hands-on experience" doing research in the laboratory. Suggestions are made for strengthening "the precarious nature of the institution of tenure at colleges and universities in the United States." And what is owed to the Ph.D. graduate?

NAS-NRC Independence

The special news report "Is the NRC ready for reform?" by Andrew Lawler (9 May, p. 900) misses essential items of background. The Act of Incorporation for the U.S. National Academy of Sciences (NAS) (approved 3 March 1863, Abraham Lincoln, President) states:

> And the Academy shall, whenever called upon by any department of the government, investigate, examine, experiment and report upon any subject of science or art, the actual expenses of such investigations, examinations, experiments, and reports to be paid from appropriations which may be made for the purpose, but the Academy shall receive no compensation whatever for any services to the government of the United States.

Such investigations are now carried on by the National Research Council (NRC), the "operating Arm" of the NAS that was established during World War I. But these investigations are overseen by the NAS itself, for example, in the systematic review, before publication, of all NRC reports. This review is the responsibility of the Report Review Committee, which represents the interests and the expert knowledge of the members of the NAS (in the formulation of some observers, a "society made up primarily of elderly, white, and male scientists and engineers") and of its associated organizations, the National Academy of Engineering and the Institute of Medicine.

To the best of my knowledge, no other national academy of sciences has such a direct and effective manner of providing scientific advice. I served as chairman of the NAS's Report Review Committee from 1973 to 1981, when I was the vice president of the NAS. In this process, academy members focused clearly on the quality and accuracy of reports; probably none was perfect, but many were revised and often improved by review. Application of the Federal Advisory Committee Act to this process cannot be other than counterproductive; there are recent examples where congressional instructions about the choice of the NRC committee members have had strong negative effects.

The NAS and its associated organizations and review committees constitute a unique and generally effective way of bringing expert scientific judgment to bear on relevant governmental problems. This, not "reform," is the central issue.

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Lawler's article "NRC alters report after complaints" (Special News Report, 9 May, p. 902) cites a 1990 incident in which the NRC is alleged to have changed a report in response to government pressure. The events described are only dimly recollected, having occurred 7 years ago. Sinister motives are implied in the NRC's attempt to ensure report accuracy after inaccuracies were brought to its attention by the President's science adviser, Allan Bromley. I would hope that the NRC would always be ready to correct inaccurate statements when these are brought to its attention. Contrary to any view that the incident is evidence of NRC's failure to stand up to pressure, it is evidence that the organization jealously guards its reputation for accuracy in its reports. If the text changes cited in

the article are evidence of lack of NRC independence, a serious issue has been trivialized. No recommendations were changed in any substantive way, and the small changes that were made in the interest of accuracy were made with the consent of the committee chair and the NRC's Report Review Committee.

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Phylogenetic Analysis

The main thrust of David Hillis's Perspective about historical and current systematic studies (11 Apr., p. 218) appears to be the promotion of the application of statistical procedures to phylogenetic inference, especially those using the maximum likelihood approach. Most empirical systematists, however, use explicitly nonstatistical methods for inferring patterns of phylogenetic relationship. Some reasons for this are as follows.

Morphological data are not amenable to the explicit evolutionary models that underlie the likelihood approach, yet morphology has provided and will continue to provide the basis for our understanding of phylogenetic relationships in most taxa.

The criteria to evaluate models of molecular evolution are poorly defined and extremely unstable, providing a weak basis for building the stable classifications that many consider to be the goal of systematics.

Maximum likelihood is computationally expensive, so it will not find optimal solutions to large taxonomic problems, which, as Hillis notes, are becoming more prevalent.

In many cases where maximum likelihood methods have been applied, such as the gopher-louse data sets reanalyzed and reviewed by Huelsenbeck and Rannala (Reports, 11 Apr., p. 227) (*1*), the same conclusions have already been reached by using strict parsimony methods (*2*).

It is easy for nonsystematists to be seduced by high-profile boosterism of quantitatively oriented methods that appear to provide rigor and reliability. However, many systematists question the philosophical basis of such techniques.

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References

1. J. P. Huelsenbeck, B. Rannala, Z. Yang, *Evolution* **51**, 410 (1997).
2. R. D. M. Page, *Cladistics* **10**, 155 (1994); M. S. Hafner *et al.*, *Science* **265**, 1087 (1994).

Response: The main point of my Perspective was that phylogeny has once again become central to biology and that phylogenies are being used in many new and interesting ways. The papers I discussed were not about phylogenetic inference per se; they were about how to evaluate, compare, and incorporate phylogenies into analyses after they have been inferred (by whatever means). Only one of the papers I discussed used the maximum likelihood approach (the report by Huelsenbeck and Rannala). Although I supported that report, I also explicitly pointed out that the specific procedures the authors were using with maximum likelihood could also be extended to other methods, including parsimony (the method promoted by Brower