dependently and partly in response to these recommendations, has instituted a number of procedural changes including extensive reconsideration of declined proposals.

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References and Notes

1. *Peer Review in the National Science Foundation: Phase Two of a Study* (National Academy of Sciences, Washington, D.C., 1981). The authors of the study are J. R. Cole and S. Cole, with the Committee on Science and Public Policy (COSPUP), not J. R. Cole, S. Cole, as in reference 3 of the 20 November *Science* article.

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Regarding peer review, I question the interpretation given to the study (*1*) of which I was a coauthor with Cole and Cole. I reproduce one of three figures from the main report (Fig. 1); the overall impression is one of substantial agreement of the sets of reviews, disregarding two undependable data points. The corresponding figure of Cole *et al.* (p. 883) suggests greater disagreement because they chose to plot ranks and not to circle the most doubtful points. When ranks are plotted, many near-ties are displayed as sizable separations.

The statistics on variation are best evaluated by assuming an average of four reviewers per proposal, which is close to the NSF norm. On a scale that assigns 10 points to the difference between *Very good* and *Good*, the estimated error standard deviations are 3.8, 3.5, and 4.8 for the three areas of research, the largest being for economics. These bottom-line estimates are far less dramatic than the intermediate calculations stressed by Cole *et al.* Their last analysis compares 2 standard deviations whose ratio would be 1.00 if chance alone determined ratings. By their method I reach a ratio of 1.6 for an average of four reviewers, but the article indicates that the ratio is only 1.16, because a single review is unrealistically assumed and an error is made ($\sqrt{23.67} \neq 4.36$).

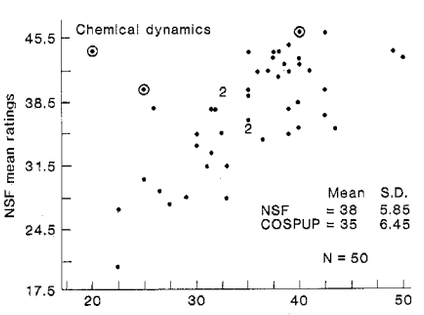

Fig. 1. Mean ratings given by NSF and COSPUP reviewers to proposals in chemical dynamics. Each circled point is based on data from only two COSPUP reviewers. [Reproduced from (*1*), p. 28]

The National Science Foundation deliberately chooses reviewers who bring different kinds of expertise to a proposal. Their properly diverse remarks inform the program director, and disagreement in their rating is not to be disparaged as random or nonrational.

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References and Notes

1. J. R. Cole, S. Cole, with the Committee on Science and Public Policy (COSPUP), *Peer Review in the National Science Foundation: Phase Two of a Study* (National Academy of Sciences, Washington, D.C., 1981). COSPUP, as principal investigator, launched the study and engaged the Coles and asked the late Jack Kiefer and me to join the Steering Committee to provide technical guidance and review.

Other letters about the article by Cole et al. *and a reply from the authors will appear in a later issue.*—EDITORS

Globin Genes

Roger Lewin's article on globin genes (Research News, 23 Oct., p. 426) is highly informative. However, one point should be clarified. Mitiko Gō, who successfully predicted the exon/intron structure of primitive globin genes, is a "she," not a "he." We hope her work will inspire other women in science.

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According to Lewin, Alec Jeffreys has speculated that legumes might have obtained their globin-like gene through a horizontal transfer via an insect-borne plant pathogenic virus. This speculation was taken to imply that insect hemoglobin has three introns, similar to the plant leghemoglobins, rather than the two introns from vertebrate hemoglobins.

Most insects do not have hemoglobin (*1*). The three genera known to have hemoglobin are internal parasites of horses, aquatic flies, and aquatic bugs. All are highly unlikely to transmit plant pathogens. How, then, could insects transmit genes responsible for leghemoglobin in plants?

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References

1. R. F. Chapman, *The Insects: Structure and Function* (American Elsevier, New York, 1969).

I would not wish to saddle Jeffreys with the suggestion of insects alone as the only possible source of a horizontally transferred gene. The important points are these. Leghemoglobin is very similar to the gene in the animal kingdom. No close relative of legumes has a gene that is remotely like leghemoglobin. Horizontal transfer therefore looks likely. The question of the exact source is somewhat secondary. There are, for instance, many invertebrates with globin genes so far uncharacterized. If insects are poor candidates, then there are many more waiting in the wings.—ROGER LEWIN

Scientific Productivity

A few years ago I attended a committee meeting during which the granting of funds for certain research projects was discussed. As is usual in such cases, the grant request forms included an item about the recent publications of the scientists involved. Since the number of publications was assumed by some to correlate with the productivity and effectiveness of the author, a rip-roaring argument ensued about what could reasonably be termed a publication and what not. The merits of including institutional reports together with articles in prestigious journals, short notes versus letters to the editor, longer versus shorter contributions, and publications in local versus international journals were all bandied about, in much the same way as they recently have been in the columns of *Science* (News and Comment, 13 Mar., p. 1137; Letters, 24 Apr., p. 396; 1 May, p. 494; 29 May, p. 986; 25 Sept., p. 1450). On this occasion the chairman voiced the liberating opinion: Surely we are all capable of weighing as well as adding. This promptly put an end to the argument. Subsequently, grant applications have included references to all contributions which the author himself thinks are of sufficient merit to favorably influence the committee's deliberations. In practice the ability of the scientist to judge his own work in this fashion has been an important factor in helping committee members establish the competence of the scientist in question.

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Erratum. In the Research News article, "Drug found to help heart attack survivors" (13 Nov., p. 774) two percentages were transposed. The article should have stated that 9.5 percent of the placebo group died, but that only 7.0 percent of the propranolol group died after 2 years of follow-up.