(2), based upon 1200 proposals submitted to ten different programs, showed that an average of 67 percent of the variance in decision was explained by the mean rating. Of the 150 proposals used for Phase Two (*1*), on only 12 did the decision differ from the decision that would have been made by using mean ratings in a mechanical way.

2) Making verbatim reviews available and encouraging appeal of a decision the applicant believes to be unfair may be improvements in peer review, provided appeals receive equitable review. A study showing the number of appeals, the type of applicants who appeal, and the outcome of these appeals would be enlightening.

3) We strongly agree with Humphreys' point that the significance of our results must be interpreted in the light of self-selection of NSF applicants. (Space restrictions prevented us from expanding on our references 2 and 7, which dealt with self-selection.) It is possible that if we had asked a random sample of American scientists to write and submit proposals there would have been greater variance in the proposal means and a corresponding reduction in the ratio of reviewer variances to proposal variances. Therefore the relative significance of chance might have been reduced. However, this is not the situation NSF actually faces.

4) We agree with Hower and Westley that the use of panels improves the peer review process. The way in which panels work is discussed in the Phase One report (*2*). For one field included in the experiment, economics, NSF did use a panel. In that field the rate of reversals for COSPUP ratings compared with NSF mail ratings (28 percent) was very similar to that for COSPUP ratings compared with the NSF decisions (24 percent), decisions which were influenced by the panel. We note that the panel-augmented decision agrees strongly with the NSF mean ratings. The panel does reduce the reversal rate somewhat in the top quintile. There is no evidence yet that the reversal rate would have been lower if the COSPUP experiment had used either a substitute program director or a panel to make the decisions. A detailed examination of substantive comments made by reviewers for cases in which differences between the COSPUP and the NSF reviewers would have led to reversals suggests that the reversals were a result of legitimate intellectual differences rather than of "errors" by reviewers.

5) We agree with Kyburg that writing a proposal can be a very useful experience. However, it can also become an end in itself, resulting in a displacement of goals in which scientists spend almost as much time applying for funds as using them to produce new science.

We are pleased that Singer notes that the full report deals with questions other than funding reversals. COSPUP decided not to include in the Academy reports our analyses of additional topics which we believe shed light on peer review. These include a discussion of the effects of self-selection; data on peer appraisals of the reputations or "track records" of NSF applicants, and a comparison of consensus on reputations with consensus on proposals; and an analysis of pooled data on the probability of a reversal as a function of the number of reviewers as well as the variance structure of ratings of the proposals.

Finally, it should be noted that, although our experiment was based upon only 150 cases, the conclusions on consensus replicated those from the Phase One data on 1200 proposals (*3*). The variance structures of reviewer ratings in the ten fields studied were remarkably similar to the data produced by the experiment. Since reversals were found to be substantially explained by lack of reviewer agreement, we believe we would have found a similar reversal rate if the experiment had been replicated on the 1200 Phase One cases.

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The Curies' Nobel Prizes

In closing their informative description of Kai Siegbahn's research recognized by the 1981 Nobel Prize in physics (6 Nov., p. 629), Jack M. Hollander and David A. Shirley note that "the 1981 Nobel award to Kai Siegbahn is the fourth time that a father and son have both received the Nobel Prize." They conclude with mention of the Braggs, the Thomsons, and the Bohrs.

It is curious that the authors interested themselves with father-son Nobel laureates, rather than with the more general category of parent-child laureates. If they had considered the latter category, they would surely have included the mother-daughter and father-daughter awardees: Pierre and Marie Curie (1903 for radioactivity), Marie Curie (a second award in 1911 for the discovery of radium and polonium), and their daughter Irene Joliot-Curie (in 1935 for artificial radioactivity).

If we scientists are to claim that opportunities in science are open to women on a fair basis, as I have in the past, we must be careful to recognize women when they succeed.

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AAAS Election: Regression Toward the Mean

Last year we noted a change in the way the sexes fared in AAAS elections: The previous advantage that women enjoyed, once nominated, had greatly diminished (Letters, 6 Feb. 1981, p. 532). The 1981 election (21 Aug., p. 863; 4 Dec., p. 1115) shows a continuation of this pattern, albeit at a slower rate. Here are the percentages of those nominated who were actually elected, in contests with both sexes represented (omitting one grossly unbalanced race where 14 men were matched against a lone woman):

Year	Male (%)	Female (%)
1979	38.9	70.0
1980	44.6	58.8
1981	46.4	56.4

Thus a Tendency becomes a Trend. As properly cautious social scientists, we should, however, wait for a fourth year's data before advancing a Theory.

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Erratum: In the article "Women scientists and engineers: Trends in participation," by Betty M. Vetter (18 Dec., p. 1313), a study by C. Rose was incorrectly cited in reference 12. The correct citation is C. Rose, *Academic Employment and Graduate Enrollment Pattern and Trends of Women in Science and Engineering* (Final Technical Report to the National Science Foundation, Evaluation and Training Institute, Los Angeles, Calif., 1978).