deep) would provide sorely needed experimental data for which the ongoing and low-cost theoretical modeling is no substitute.

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Grant Funding

The continuing deterioration in grant funding makes it more and more important for the peer review system to work with high fidelity. The two stages that determine the quality of peer review are selection of review group participants and the scoring of grants by reviewers. There are difficulties in both these areas that could profit by some discussion.

At the National Institutes of Health (NIH), reviewers are chosen by the executive secretary of each study section. Selection procedures vary from one executive secretary to another and can be as judicious or as arbitrary as the individual makes them. I am unaware of any measure of accountability or self-correcting feedback that is exercised at this most critical stage of the peer review process. Although the two executive secretaries I have worked with were both impressively able, I believe that the variable and mysterious processes used by them and others in selecting review group members would benefit from some open and informed discussion. Similar questions can be raised concerning the National Science Foundation (NSF) system, where program directors (the equivalent of the NIH executive secretaries) not only choose review group members but have the power to override review group scoring and make independent funding decisions.

With respect to the peer review evaluation itself, attention should be directed at minimizing the noise level in these judgments. It is not easy to score multifaceted grant applications onto a one-dimensional priority scale. I believe that a lot of variability in the ranking of grant applications arises from the fact that the scoring scale itself is too undefined, too soft. This softness is apparent in the variable scoring criteria applied by review groups from one grant to another and one meeting to another, in the fairly wide divergences that appear in the scoring levels of different review groups, and in controlled trials that have been performed with test grant applications. The fuzziness of the scoring criteria reduces the effectiveness of novice panel members and loads them and seasoned members as well with an even heavier burden of uncertainty.

Criteria for calibrating the priority scale, which I learned to appreciate during my study section experience and which have since been found useful by a number of my colleagues, can be codified as follows. The scale is made linear by giving roughly equal weight to three criteria of worth.

1.0—solid science; innovative; high potential importance;

1.5—two of the above;

2.0—one of the above (or the equivalent, for example, moderate importance plus fair solidity);

2.5—fairly mundane but with a significant level of quality;

3.0—may contribute something to science, but the chances of doing so are problematical;

3.5 and down—generally poor (interestingly, a disproportionately large share of study section time seems to be spent in subfractionating the space between priority scores 3 to 5).

Among these criteria, innovation is of foremost importance for the progress of science, yet it is remarkable that innovation in grant applications is often penalized. Most good grantsmen have learned to provide a mix heavy in solid science and light in innovation because inexpert reviewers tend to focus their critiques on the lack of certainty that any innovative idea or experiment will succeed. This overworked cliché is simply not a valid criticism of applications that otherwise rank in the top (fundable) categories.

These specific criteria for assigning priority scores are still incomplete. There are, in addition, two major general touchstones that should be considered. One is the track record—the applicant's demonstrated skill in and past contributions to science. Here the reviewer must transcend mere publication counting and assess "quality" and "contribution." It is essential to appreciate that the reviewer's job is not to grade the application itself; the exercise is to gauge where on the above scale the *research to be done* will fall. In trying to predict the future, an analysis of the past can be of great help.

Another determinant that should, in my opinion, be folded into the final score is the level of funding requested, or rather the funding judged necessary to do the better part of the work. There is still left over from the easy money days the philosophy that quality and funding should be judged separately in a kind of double-blind way. However, in a time of limited funding, applications of equal scientific promise but disparate price have to be distinguished because the expensive project will unavoidably preempt funding from other potentially fruitful research. Further, the best way to encourage prudent laboratory economics (and responsible budget proposals) is to enunciate this principle.

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Erratum: In the article "White House steps into lead fight" (News and Comment, 27 Aug., p. 807), John V. Diepenbrock should have been identified as chairman of the finance committee in California for Ronald Reagan's presidential campaign.

Erratum: Figure 2 (left) in the report "Transformation induced by Abelson murine leukemia virus involves production of a polypeptide growth factor" by D. R. Twardzik *et al.* (21 May, p. 894) was incorrect. It showed transforming growth factor (TGF) purified from human melanoma cells rather than from the Abelson virus-transformed rat cells. The correct figure from the very same gel is shown below. Although one portion of the gel was used rather than another, none of the conclusions reached in the report are affected. Both rat and human TGF's have molecular weights of 7400 and have very similar amino acid compositions and NH_2-terminal amino acid sequences.