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## The Tea Leaves or the Record?

Our scientific leadership has been less than brilliant both in justifying public support of basic research and in managing the funds made available. Federal support of basic research was initiated because citizens developed a profound respect for a great record of scientific accomplishments. Nevertheless, scientists, in talking to politicians, have taken the line, "You never know what will come out of scientific research." Then, instead of talking of recent accomplishments, the spokesmen speak of Newton or Faraday.

Despite professing inability to judge significance of research work in advance, a granting agency such as the National Science Foundation tries to do just that in passing on research proposals. In deciding whom to support, considerable emphasis is placed on the content of the research proposal. Knowing this, some scientists are willing to make extravagant promises. All of us can have hopes, and it is just as easy to have high hopes as lesser ones. Panelists are not stupid or easily taken in, but often the proposal is the principal piece of information at their disposal. In considering grants, much more weight should be given to past scientific achievements of the individual than to his merchandising abilities. Men who have produced are likely to continue to do so; those who have promised much and delivered little will continue to promise. The scientist's past stewardship of federal funds should be examined. Although expenditures are audited, there is at present little follow-up on the scientific results.

In evaluating grant proposals we should focus on the man—his accomplishments and the trends in his accomplishments. If he is young and just beginning, we should rely on the judgment of his professors. As his professional development proceeds, more and more weight should be given to achievement. The principle of looking at the record for guidance has relevance beyond the individual proposals. Valuable judgments about whole fields of science can be made by looking at the record.

However, instead of giving predominant weight to trends in productivity of fields of research, the NSF has made allocations for grants for the various sciences largely on the basis of so-called "proposal pressure." The NSF has assumed that the scientific community will generate proposals for grant support in proportions representative of the needs and opportunities of the various fields.

After recommendations have been made on the grant proposals by panelists, the bookkeepers total the sums involved. If the physicists approve of grants calling for \$120 million and the chemists approve of grants involving \$80 million, proposal pressure in physics is considered to be greater than proposal pressure in chemistry. Accordingly, the physicists are given more money.

A little consideration will make it evident that the mechanism of proposal pressure can easily be distorted. Panelists early become aware that the more they recommend the more their science gets. In the past some NSF staff members have been active and successful in encouraging grant proposals for their particular discipline. This led to high proposal pressure in some fields and not in others, and to distortions in the support of science.

The NSF can function as a balance wheel only if it develops a better approach to the advocacy and management of basic research. Such an effort will necessarily involve continuing analysis of trends and accomplishments in scientific research. Increased support for NSF will be available if the foundation makes a convincing demonstration that basic research is continuing to be productive in creating important new insights. In judging individuals, areas of science, and organizations, a look at the record is superior to a look at the tea leaves, and a lot more convincing.—PHILIP H. ABELSON