Letters

Peer Review at NIH

In a recent letter (10 Sept., p. 984), S. Walter Englander comments on the selection of National Institutes of Health (NIH) study section members and the assignment of priority scores. NIH obviously shares his desire for the peer review system to operate at the highest level of competence, fairness and efficiency, and so welcomes any suggestions for improvement. However, Englander's letter does not fully and accurately describe the two important aspects of peer review about which he is concerned.

When selecting study section members, NIH exercises great care and follows sound management methods. The identification of potential study section members is the responsibility of executive secretaries. Since they are present, as managers, at hundreds of detailed reviews of research grant proposals three times each year, these individuals are most knowledgeable about who are the experts in the scientific areas reviewed by their study sections. In addition, executive secretaries routinely consult colleagues within the NIH community, highly respected experts in the scientific community, and study section members, present and past, when new study section members need to be identified. Thus, NIH believes that its executive secretaries are best aware of both the need for specific scientific competency, as well as the scientists who may be able to meet the review needs of the study section.

It is also important to note that NIH has made an increased effort in recent years to identify female scientists, members of minority groups, and others who may wish to serve on study sections but have not been easily identified. For this purpose, NIH established a consultant file that now lists more than 9000 individuals. NIH welcomes continued suggestions from the entire spectrum of the biomedical and behavioral scientific communities.

Regardless of the method used to identify potential study section members, only those who are judged to meet the necessary levels of scientific competence, maturity, fairness of judgment, and sufficient standing among their peers in the field are submitted by executive secretaries for further consideration by NIH. These submissions are reviewed by several layers of NIH professional staff, who alert the executive secretary to any questionable nominations that might have been made. Only after the review and approval process has been completed will the director of NIH invite an approved new study section member to serve. Thus, the selection process is systematic and under careful management. While the executive secretary is certainly the key individual in the process, NIH as a whole is involved and has a vital interest in seeing that only the most appropriate scientists serve on the study section.

Regarding the assignment of priority scores and Englander's concern that softness or "noise" in the system prevents accurate judgments, several points need to be made. Study sections have uniform guidelines by which to assess the merit of research proposals under review. The executive secretary is responsible for ensuring that these guidelines are followed. The criteria for new research project grant applications include (i) the scientific, technical, or medical significance and originality of the research; (ii) the appropriateness and adequacy of the experimental design and methods; (iii) the qualifications and experience of the investigator(s); (iv) the reasonable availability of resources; (v) the reasonableness of the proposed budget and duration of support in relation to the proposed research; and (vi) where an application involves activities that could have an adverse effect upon humans, animals, or the environment, the adequacy of the proposed means for protecting against such effects. NIH considers these criteria to be sound and sufficient to enable the peer review system to identify the most meritorious research grant applications for support.

For applications that they recommend to be approved, study section members must communicate with each other in the least encumbered manner to obtain the most accurate scoring of priorities. To achieve this goal, members vote in 0.1 increments, from 1.0 (most meritorious) to 5.0 (least meritorious); and, when they wish, they may indicate the number that they feel best describes their level of support for a given research effort. Each member votes privately and independently, and the chairperson and executive secretary foster open discussion on the rationale for any differences of opinion, especially if a member's evaluation varies significantly from the consensus.

Englander's criteria for determining a priority score based on various degrees of solid science, innovation, and potential importance are, thus, in part, the same criteria used by study section members. Whether these attributes of meritorious research should be assigned equal weight is precisely the judgment the members are called upon to make. In some proposals, the importance of the work may outweigh innovative aspects; in others, an innovative idea may outweigh a less than perfect experimental design.

We at NIH welcome comments and ideas such as those expressed by Englander. The soundness of NIH peer review procedures is vital in order to maintain excellence in biomedical and behavioral research in the United States.

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Academia and Industry

I was pleased to read the cautionary notes in Philip H. Abelson's editorial "Differing values in academia and industry" (17 Sept., p. 1095) and would like to add a few of my own. While interactions with industry have played and will continue to play an important role on the university research scene, they should not be expected to replace a significant portion of the research funding now provided by the federal and state governments. The reasons are many: those Abelson discusses in his editorial and those which have concerned various university groups, as outlined in recent articles in the News and Comment section of *Science* (9 Apr., p. 155; 28 May, p. 960; 11 June, p. 1200; 18 June, p. 1295; 6 Aug., p. 511; 17 Sept., p. 1122).

Support from industry tends to be directed toward specific fields, those which are "hot" and of potential commercial interest for the donor. Rarely is support given in a broad enough area to be considered "uncommitted" funding.

While this is understandable, it has the effect of distorting the efforts of the faculty as they tend to "migrate" toward the fields attracting support, often at the expense of more "classical" disciplines that form the base on which new areas have to be built. Some of this is inevitable and already occurs as a consequence of federal government support. In the case of large industrial grants it may be carried to an extreme because of the need for eventual profit on the part of the donor.

Another problem is that funding from industrial donors rarely is large enough to support the work proposed in the area of the grant and hence requires a significant input of university funds. This further diverts the efforts of the university toward particular fields. This leveraging effect is much greater than that exerted by federal support, since the federal government generally pays a greater share of the actual research costs.

Despite all the difficulties attendant upon industrial support, it forms an important part of the "mix" of support for university research and graduate education. Acceptance of such support in large amounts, however, requires careful university leadership to avoid major pitfalls.

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I would certainly agree with Abelson that the mission of industrial research is to make a profit for the industry and that this would on the face of it appear to be in conflict with the basic aims of academic research; however, a situation has developed within the academic biomedical research establishment that is already dangerous to scholarly activity and the traditional pursuit of knowledge. I speak of the problems occasioned by the pinch in federal funding for research. The effect of this pinch in the past 5 years has led many established investigators to pursue "fundable" projects, in areas that are "trendy" and have the glow of immediate application to publicized areas of human concern. This freezes out the smaller scale research activities of individual faculty members, as well as those of the younger investigator, and has already led to abandonment of research projects that should have been done.

Abelson writes of a concern about a loss of the ability of universities to carry out their essential function. In my opinion, this has already occurred in the economic scramble by scientists and universities alike to obtain funding for research and to neglect or place in a secondary position the mission for teaching and education. The search for truth should not be influenced by pressures, political or economic.

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Investigating Solar Activity

Richard A. Kerr's Research News article "Sun, weather, and climate: A connection" (3 Sept., p. 917), reporting the conclusions of a National Research Council (NRC) panel in their report *Solar Variability, Weather, and Climate*, raises fundamental questions about what methods of investigation to use in the environmental sciences. The search for evidence about the sun's effect on climate has a long and frustrating history, doubtless not avoiding the "pitfalls of overenthusiasm and sloppy statistical analysis," although pioneering work in the natural sciences has often been so described. Even a vain search for evidence of the sunspot cycle in tree rings led the astronomer A. E. Douglas to invent tree ring dating.

The NRC panel, however, calls for a shift in emphasis "from the traditional pattern of *searching for evidence* [of a correlation] to a more directed effort at *understanding the physics* of the atmosphere and the solar-terrestrial system as a whole." This brings to mind Lord Acton's dictum "The only thing man learns from history is that man learns nothing from history." The complexity of physicochemical processes encountered in natural phenomena, whether in the atmosphere, the earth's mantle, or its core, is such that great progress has usually come from widely varied observations and studies of their interrelationships rather than by the route recommended by the NRC panel.

Modern plate tectonics originated with a search by Alfred Wegener and geologists before and after him for some explanation of the relationships between the stratigraphical, paleontological, and tectonic records in various continents and from the close fit of different continents. Soon after Wegener's book *The Origin of Continents and Oceans* was published, a general consensus among earth scientists developed that such relationships might well be purely coincidental, and that Wegener's views should be rejected so long as no mechanism could be demonstrated to provide a physical explanation for continental drift and related phenomena. Only a small number of field geologists remained convinced of the validity of Wegener's conclusions, but their arguments were rejected as arising from overenthusiasm and sloppy analyses. It took nearly half a century before quantitative geophysical data became available and were accepted as proof for Wegener's theory. Significantly, the first such decisive quantitative evidence that continents had moved apart came from the study of paleomagnetism, not from the seemingly more direct approach at understanding the physics of the processes likely to be responsible for continental drift and plate tectonics.

It is therefore conceivable that the use of quantitative models for the study of possible interactions between solar activity and the terrestrial atmosphere, instead of continuing the search for evidence, may delay an understanding of solar-climatic relationships by many decades. No doubt correlation can always mislead, but scientific advance often involves search in out-of-the-way places. It should also be kept in mind that "sloppy statistical analysis" tends to obscure a signal rather than create a fictitious one.

An interesting example can be found in the study of radioactive nuclear species such as carbon-14, produced by cosmic rays, which has provided new ways of learning about solar activity in the more distant past (*1*). Also, carbon-14 measurements are relevant to the time scale of climatic variations in the past: nothing is known about solar activity during the time of the most dramatic change on record at the end of the last ice age. We urge that, despite the policy of the NRC report, the widest possible approach be taken in the study of solar-terrestrial relationships rather than confining it to traditional patterns of meteorological research.

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Reference

1. H. E. Suess, *Endeavour* N.S. 4, 113 (1980).

Erratum: An error appeared in the first sentence of the last paragraph of the report "Intracellular recordings from cochlear outer hair cells" by P. Dallos *et al.* (5 Nov., p. 584). The correct version is as follows. "Inner hair cells appear to operate at about one-half the membrane potential of outer hair cells. *The latter* resemble supporting cells in this respect."