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Enabling Scientific Innovation

THESE SHOULD BE WONDERFUL TIMES FOR PHYSICISTS. ADVANCES IN EXPERIMENTAL AND computational capabilities have opened windows to research areas that previously had seemed out of our reach. We now can see in exquisite detail what atoms are doing inside complex materials, and we have the tools to test predictive theories of what's happening. We have been able do this in ways that are interesting not just to physicists but also to engineers, biologists, and many others. Examples abound in this issue of *Science*, which features a special section that focuses on physics in developmental biology (beginning on p. 209). Unfortunately, our ability to take advantage of these opportunities has been eroded in an environment where innovation and interdisciplinary research are systematically discouraged. Present funding levels in the United States cannot support a scientific community large enough to sustain a full range of world-leading research programs and to educate the new generations of scientists and engineers that the country needs. That problem cannot be solved quickly in today's political climate, but government funding agencies ought not to operate in ways that make it worse.

In my area of condensed-matter and materials physics, the U.S. National Science Foundation (NSF) can fund only about 10% of the individual-investigator proposals it receives. [The Department of Energy (DOE) has similar difficulties.] Each proposal is sent to a group of peer reviewers, who rank it on a scale ranging from "excellent" to "poor." NSF then funds only those proposals that receive the uniformly highest reviews. One less-than-"excellent" review, no matter how misguided, is usually enough to doom a proposal. Any proposal that is truly innovative, interdisciplinary, or otherwise unusual is almost certain to be sent to at least one reviewer who will be less than enthusiastic about it. Sensible investigators know not to submit such proposals; as a result, some of the most urgent research areas are disappearing. Consider, for example, the behavior of materials that are driven away from thermodynamic equilibrium. We need to learn how to predict irreversible responses to strong forces for structures ranging from jet engines to biological membranes. These are difficult, deeply fundamental, and intrinsically interdisciplinary research problems. But far too little attention is paid to them these days.

The U.S. system for funding research was designed 60 years ago to function well in times of growth. It is failing now, not because peer review is failing, but because the system as a whole is contracting. Much of today's proposal pressure is caused by underfunded investigators who must compete for multiple grants to survive professionally. They do this under stress from promotion committees and with growing uncertainty about peer review. Such scientists have little time or incentive to be innovative.

What is to be done? The funding agencies, especially NSF and the DOE, must admit that it is not humanly possible to predict, with high accuracy, which research projects ultimately will have the most impact. Peer review, at best, can identify fundable proposals. When there are too many of these, as at present, the agencies must find other ways to decide which to support. That will be hard.

The United States must not abandon the spirit of peer review by relegating these hard decisions to a bureaucracy. The easy alternative would be to choose among the fundable proposals by a lottery. Strange as it seems, that could well do less damage than the present system; but it would be a mindless process, unworthy of U.S. science. Perhaps the funding agencies and the scientific community together can devise a mechanism for assessing the urgency of the problem, setting priorities, and finding compromises. An independent agency such as the U.S. National Academies could coordinate this process. At the very least, it will be critical to find modes of operation that do not discourage the most imaginative investigators just because their proposals are too innovative.

– James S. Langer

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