Science 29 January 1999:
Vol. 283. no. 5402, pp. 642 - 643
DOI: 10.1126/science.283.5402.642

POLICY FORUM

SCIENCE POLICY:
Interdisciplinary Research: From Belief to Reality

Norman Metzger and Richard N. Zare*

Interdisciplinary research is a mantra of science policy. Virtually any meeting on the current state and future of science is leavened by obligatory statements about the importance of enabling researchers to work seamlessly across disciplinary boundaries and by solemn declarations that some of most exciting problems in contemporary research span the disciplines (1). However heartfelt, the nature of the current federal support system--and of the research community--counters these beliefs. Here we briefly discuss the history, structure, and culture that hinder a vigorous national effort in interdisciplinary research, and we conclude by offering a modest remedy.

Past Practices

The historical roots of today's federal research enterprise are at once complex and simple. They are complex in that many forces shaped its parts, but at its root each of these forces is reducible to one of three categories--war, crises, or needs (2). World War II, with its research yields as diverse as radar, synthetic rubber, penicillin, and atomic energy, gave the nation confidence that science could solve problems and that there was a national and hence federal stake in supporting the enterprise. That confidence, exploited by Vannevar Bush and John Steelman, led to the postwar creation or rapid growth of the Office of Naval Research, the National Science Foundation (NSF), and the National Institutes of Health (NIH). National crises such as Sputnik, the Arab oil embargo, environmental problems, and economic competitiveness (especially with Japan) launched or transfigured agencies with substantial federal funding (3). The end result is a large federal support enterprise that was hardly planned, one that is confusing, complex, lacking coherence, and somewhat unwieldy, all of which is really no surprise.

This messiness translates into a system that is adaptable, evolving, and spectacularly successful. And that success was built on a symbiotic relationship between federal agencies supporting science and technology and U.S. universities, which became the postwar research universities, the home of science and engineering in the United States. U.S. graduate schools became envied models in their integration of education

and research, although that mode carried over less strongly to their undergraduate offerings. Peer-informed review, especially in the National Science Foundation and National Institutes of Health, came to be the mode of choice for awarding federal funds for research, although in some instances strong manager models, such as at the Defense Advanced Research Projects Agency (DARPA), have also demonstrated great success. Nevertheless, peer review by definition came to mean judgments by those from a single discipline, and often by those working in the same research area within a discipline. Judgments on research support, which are critical to an academic career, became increasingly more specialized and discipline-bounded. These ways of controlling the flow of research funds were mirrored by structural and cultural changes. Many of the federal agencies supporting research created structures--whether the directorates of NSF or the study sections of NIH--that were reflective of university departments. And a successful career in a research university was largely if not wholly dependent on success in disciplinary research, which in turn was measured by publication, election to various national academies (whose sections are almost wholly disciplinary), and the ability to obtain federal grants. Researchers, especially untenured ones, proposing research programs that move across disciplinary fences have, and do, put their careers at risk.

Current Problems

This coarse portrait has obvious exceptions, both in federal programs and in university tenure policies. Certainly the most powerful exceptions are in the many research programs conducted in the federal laboratories and in industry, where the goals (such as national security in the federal Stockpile Stewardship program or market forces in the creation of cheaper, stronger, and lighter materials) force vigorous and effective interdisciplinary work. Yet we submit that the exceptions are just that: limited efforts to introduce real change in a resistant system, one built on the proven belief that excellence in science meant disciplinary excellence. But if the U.S. research enterprise is so successful, why tinker with it? Why "fix it if it ain't broke"? We suggest that the success has cloaked substantial failures of omission occasioned by disciplinary and similar rigidities on the part of agencies and the research community. Some systematic problems are as follows:

1) *The lead agency has a weak research program.* A prime example is the U.S. Environmental Protection Agency (EPA), which is predominantly a regulatory agency. It has a sizable R&D program, but it has been focused on short-term immediate goals, shaped by the need to react to crises and the agency's regulatory mission. These consequences of the nature of the agency have led to weakness in conducting fundamental and long-term research and to a lack of the interdisciplinary perspective that environmental problems axiomatically require. Although there have been various efforts to address the problem, they remain limited. The upshot is that high-quality work on fundamental issues related to environmental matters continues to be underemphasized in the federal R&D portfolio.

2) *The agency has a strong, successful, but constrained research program.* An example is the enormous investments in molecular biology, immunology, and the like that are made primarily through NIH. This effort is largely reductionist biology, from the molecule up to higher levels of organization. These investments have been spectacularly rewarded and have revolutionized our biological understanding at the molecular and cellular levels. Weaker comparatively and historically is support for so-called organismic biology--ecology, population biology, and so on--where the starting point is a living creature, not a molecule or cell.

3) *Failure to strengthen complementary and supportive sciences in a rapidly growing*

field. One example is the hardware/software/wherewithal for humans to use information technology. Enormous federal investments in the information technologies were not accompanied by investments of similar scale in research on human performance, the cognitive sciences, and similar fields. Such investments might have made the use of information technology both easier and more universal. On a broader scale, there has been a failure to strengthen the links between the social and behavioral sciences on the one hand and advances in the physical and biological sciences and technologies on the other.

4) *Problems left fallow by missing agency programs and university structures.* An example is depletion of oceanic fishing stocks. Understanding the reasons, aside from the simplistic one of overfishing, means synergistically blending the perspectives of many sciences--genetics, oceanography, population dynamics, marine ecology, and so forth. So far, no significant effort to mount high-quality research efforts blending these sciences is under way in the United States.

Some Solutions

These weaknesses in federal support result, we believe, from narrow perspectives by both agencies and universities derived from the forces we have briefly outlined. There are several ways in which we might attack the issue. But first we must recognize some realities of the current system.

- Integration of research and education is the sine qua non of the U.S. research system and has propelled the spectacular success of fundamental research in the United States.
- The best ideas often come from the bottom up; that is, from researchers themselves; and some of the most spectacular ideas come from young researchers who are newly tenured or untenured.
- A substantial part of the history of U.S. research has been written by people who, against substantial cultural if not economic odds, have reached out to other fields, merging different perspectives and creating new ideas, even new fields.
- Many programs intended to strengthen interdisciplinary research and to foster partnerships have foundered because the principals never changed their research program, just renamed it to obtain funds.
- Federal structures--both in the executive and the congressional branches, the latter strongly emphasized by the appropriations process--strongly militate against interdisciplinary programs cutting across jurisdictional lines.
- Strong interdisciplinary programs can only be built in circumstances in which strong disciplinary programs already exist. It makes no sense to sacrifice successful disciplinary efforts to appease perceived interdisciplinary needs.
- No matter how promising, projects cannot succeed unless they have a champion in the federal support system. Without a champion, no one understands who is accountable, whose budget is at stake, and who benefits and who loses.

In light of these observations and of the forces that led to them, we believe that a substantial enhancement of interdisciplinary research requires a new program that is owned by several disciplines within an agency or even by several agencies (4). To succeed, this program must have the tacit approval of the apposite congressional committees. Just as critically, it needs the support of the administration and faculties

of the research universities, for these institutions in their strong and understandable commitment to disciplinary strength are at the heart of the problem we've described. In turn, the program has to demonstrate to the research community that its depth, creativity, and intellectual rigor match that of disciplinary programs. The program would offer long-term support--we suggest 5 years--for interdisciplinary research fellows to work on a number of agreed-to broad themes. Some illustrative but certainly not prescriptive examples of such themes might include (i) fundamental investigations to strengthen environmental sciences; (ii) integration of social and behavioral science research with biological research; (iii) the role of the cognitive sciences in strengthening and making more effective future advances in science and technology; (iv) attacking resource issues, such as fisheries depletion; (v) applying contemporary mathematics to complexity issues in research, especially but not solely in biology; and (vi) combining biocentric activities, such as biophysics, biochemistry, biology, bioengineering, biotechnology, biomedicine, bioinformatics, and so forth.

Brief letter proposals for research programs to address these broad themes would be solicited from the university community. The cooperating agencies would use advisory committees, whose membership would blend disciplines from many fields, with no one discipline dominating, to select the intellectually strongest proposals and would provide major and sustained funding for the work. That funding should support not only the principals but also provide for fellowships for graduate students and support for undergraduate students, as well as the requisite equipment. Some of the funds should also go to the departments themselves, to compensate for the time of the principals that is lost to the department.

To be effective, such a program must be of critical mass. We believe that the program should select 10 interdisciplinary teams every year, with each team given an average of $1.5 million annually for 5 years, with no renewal. Each program should be reviewed after 2 years; if found deficient, it should be terminated, with limited shutdown funds. An average of $1.5 million per year seems reasonable, given that the individual programs will have by definition at least two principal investigators. The total annual cost at a steady state would then be $75 million.

We believe that this relatively modest investment would reap substantial returns in enriching and enlarging the national research enterprise, in directly addressing several national goals, and in creating substantial and healthy changes in the fundamental nature of the research enterprise. In today's universities, knowledge is typically extracted from an integrated whole by study units, called departments, where that knowledge is disintegrated and disaggregated in a process famous for its turf battles and jurisdictional disputes. The interdisciplinary programs we propose are an attempt to reintegrate this acquisition of knowledge, both its discovery and its dissemination. If such a reintegration of the knowledge process can be accomplished, then the program will have made great strides in redefining the character of the U.S. research university and in preparing our nation to make scientific and technological contributions to solving ever more complex societal problems.

References and Notes

1. Our use of the term "interdisciplinary research" embraces variants of the term, such as cross- and multidisciplinary. It also includes "interdependent research," a relatively new concept connoting work done wholly within one well-defined discipline, which in time is exploited with great rewards by another discipline; for example, the work done on atomic spin that led to magnetic resonance imaging in medicine.

2. This historical discussion is abstracted from a more detailed one in *Allocating Federal Funds for Science and Technology* (National Academy Press, Washington, DC, 1995), pp. 41–43.
3. Specifically those agencies are NASA, the U.S. Department of Energy, EPA, and the National Institute of Standards and Technology.
4. Program managers typically have responsibility for one discipline or a set of closely related ones. Although that is certainly a very effective structure, we also believe that it needs to be complemented by managers of disciplinary programs meeting as a unit that is empowered to make funding decisions on interdisciplinary proposals. This matrix management scheme would help to deal more effectively with proposals whose disciplinarity does not parse neatly.
5. This article is based in part on a talk the authors gave to the board of the W. M. Keck Foundation, and we thank the board and officers of the foundation for encouraging us to examine this issue. R.N.Z. also acknowledges that his thinking on the issue of interdisciplinary research was stimulated by his service on the organizing committee for a symposium on the "Nature and Dynamics of Interdisciplinary Research," which was held at the Wenner-Gren Center in Stockholm, Sweden, 11 to 14 November 1998.

N. Metzger is executive director of the National Research Council Commission on Physical Sciences, Mathematics, and Applications, 2101 Constitution Avenue, NW, Washington, DC 20418, USA. E-mail: nmetzger@nas.edu. R. N. Zare is past chairman of the National Science Board and is the Marguerite Blake Wilbur Professor of Chemistry, Department of Chemistry, Stanford University, Stanford, CA 94305, USA. E-mail: zare@stanford.edu. Views expressed are the authors' and not necessarily their institutions'.