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The Funding Crisis

A visit to laboratories across the land will expose the traveler to cries of alarm in regard to the scarcity of research funds. Too many good proposals are not being funded, too many good investigators are getting less money than they can manage fruitfully, and too many young investigators are having difficulty getting started. Yet cries of alarm will not translate into funding increases without action on the part of scientists.

The total budget for basic research is pitifully low. Federal funds for basic research went from $9.5 billion in 1988 to $10.5 billion in 1989 and to $11.2 billion in 1990. These increases are a minor increment above inflation and certainly do not reflect the increased sophistication of modern research, the number of new investigators entering the field, or the needs of the nation (see J. Palca, *Science*, 4 May, p. 541, and 18 May, p. 803). A country that can squander hundreds of billions of dollars in a savings and loan scandal can afford to spend a great deal more of its total national product on providing the means to a better life and a more competitive nation. We are in great need of bacteria to clean up oil spills, higher temperature superconductors for industry, pharmaceuticals for the mentally ill, polymers strong enough to replace scarce metals, better remote sensing to monitor the environment, more efficient cars, better urban planning, and so forth.

The dramatic changes in eastern Europe mean that in the near future there will be even more competitors in the global economy, and any nation that falls behind in research and development will certainly fall behind in international competition. Thus the scientific community has two tasks. The first is to think into the future for areas in which science can make a contribution toward solving the problems that beset us, and the second is to educate or persuade legislators and the public to invest more money in research. It is not possible for a single central scientific organization to mastermind an overall strategy to carry out that mission. Moreover, Congress and the President will argue correctly that they are the final arbiters in any case. Each discipline must develop its own plans, looking to the future of the country, and then make convincing presentations of well thought-out scenarios. The emphasis must be on what we can do for you, not what you ought to do for us. As Don Langenberg, chancellor of the University of Illinois, said recently, "Nobody ever bought a Buick because they were sorry for General Motors."

What is important is to think big about "little science." There will undoubtedly be some megaprojects, but what the nation and the world really need is a major expansion of investigator-initiated science, because that historically has been the source of great discoveries that have opened new frontiers.

The strategy would be to identify an area, such as the environment, and provide the analysis that a good program requires research in ecology, toxicology, biodegradables, recycling, and microbiology. Furthermore, the analysis must provide convincing numbers that the scattered, inadequate funding in these disciplines needs a major coordinated expansion to, for example, the $2-billion level. The implementation would be achieved by investigation-initiated research in the targeted areas. A similar effort is needed in population control since it is the population explosion that has created most of our current problems and can well undermine our future. Public transportation, auto efficiency, land use, mental health, and solar power are also among the research frontiers that need major expansion as the world comes to grips with a swelling population on a finite globe. The learned societies play a vital role in the advocacy for new monies, since they are the organizations that can make public information available, and they have the expertise to validate legitimate claims and exclude exaggerated ones. What they, and individual scientists, need to do is to develop currently sketchy outlines into well-designed programs and to advocate those programs for little science just as "big science" has done. In the future, however, learned societies should generate cost-benefit analyses of "big" and "little" science initiatives to place them in perspective.

The first priority is to find out how science can contribute to a better world. The second is to deduce how science should be organized to carry out the task. Asking for small increments to keep current programs going is desirable and needed, but conventional approaches may not be politically effective. We will need procedures to improve funding decisions within the infrastructure, but thinking big means a focus on the massive problems that need to be solved and seeing them as a challenge to create realistic pathways to a bright new future.—DANIEL E. KOSHLAND, JR.