The mortality rates from leukemia, about which we were more concerned, were not significantly different between both areas. Nevertheless, the size of the population investigated in this period was relatively small, and further study is necessary.

Growth and development of children. Measurements of head circumference, body weight, and height of the children below 12 years of age in the high-background area (3239 persons) and the control area (2991 persons) showed that differences in growth and development of children between these two areas were not statistically significant.

Conclusions

The radiation level in some regions of Yangjiang County, Guangdong Province, is about three times that in the neighboring control areas, but lower than that in some parts of high background radiation areas in India and Brazil. However, the distribution of exposure rates in the investigated regions is relatively even, and there is a high density of people whose families have lived there for many generations. Results of the health survey carried out between 1972 and 1975, which did not demonstrate any significant difference between inhabitants living in the high-background and control areas, suggest that the size of the population investigated may be not large enough to reveal minor increments of detrimental effects at such a low dose range of ionizing radiation. Or there might be a practical threshold dose; that is, the possibility that the dose-effect curve had a zero slope at these doses cannot be ruled out. For the reasons given above, further investigation of a larger population is necessary.

Additional Readings

1. T. L. Cullen, "Dosimetric and cytogenetic studies in Brazilian areas of high natural activity," *Health Phys.* **19**, 165 (1970).
2. E. P. Franca *et al.*, "Status of investigations in the Brazilian areas of high natural radioactivity," *ibid.* **11**, 699 (1965).
3. A. R. Gopal-Ayengar *et al.*, "Evaluation of the long-term effects of high background radiation on selected population groups on the Kerala coast," *Proc. 4th Int. Conf. Peaceful Uses At. Energy* **11**, 31 (1972).
4. J. V. Neel and W. J. Schull, *The Effect of Exposure to the Atomic Bombs on Pregnancy Termination in Hiroshima and Nagasaki* (Publication 461, National Academy of Sciences–National Research Council, Washington, D.C., 1956).
5. E. E. Pochin, "Problems involved in detecting increased malignancy rates in areas of high natural radiation background," *Health Phys.* **31**, 148 (1976).
6. Advisory Committee on the Biological Effects of Ionizing Radiation, *The Effects on Populations of Exposure to Low Levels of Ionizing Radiation* (Division of Medical Sciences, National Academy of Sciences-National Research Council, Washington, D.C., 1972).
7. United Nations Scientific Committee on the Effects of Atomic Radiation, *Ionizing Radiation: Levels and Effects*, vol. 1, *Levels* (United Nations, New York, 1972).
8. H. Yamashita, in *Biological and Environmental Effects of Low-level Radiation* (International Atomic Energy Agency, Vienna, 1976), vol. 2.

Innovation and Scientific Funding

Richard A. Muller

It is difficult to judge the performance of scientific funding agencies, for, like physicians, they often bury their mistakes. Rejected proposals usually mean doomed projects. If the projects survive rejection and succeed, it is rare that they achieve recognition soon enough to alert the funding agencies that mistakes are being made. In 1978 I was given the Alan T. Waterman Award of the National Science Foundation and the Texas Instruments Foundation Founders' Prize for research that initially had been rejected for funding by the National Science Foundation (NSF), the Department of Energy (DOE), the National Aeronautics and Space Administration (NASA), and the Department of Defense. I felt an obligation to make my experience known, not because I thought it unique, but because of my unique position as the recipient of the awards. A discussion with Dr. Frank Press of the White House Office of Science and Technology Policy led to meetings with agency heads and testimony before the Committee on Science and Technology of the U.S. House of Representatives. This article is an adaptation of that testimony.

I was able to proceed with the rejected projects by "circumventing the system." I had been advised by my mentor, Luis Alvarez, to spend money designated for other projects on the unfunded work. He said that if the projects were successful, nobody would question the propriety of having done this. I was helped by our NASA funding monitor, who allowed us to designate a fraction of one of our grants as "seed money" for new projects, as long as the amount was small and remained "low profile." In addition, I was able to obtain some seed money from the Lawrence Berkeley Laboratory, although those involved felt that they were taking a risk, since the projects were not immediately relevant to the DOE's mission.

It is well known in the research community that one cannot expect a proposal to be funded until a considerable amount of work has been done on the project. When I began research in 1965, our research group often received more than the minimum support necessary for our projects, and the excess money was used to seed new ideas. Only a small fraction of these ideas led to a formal proposal. If the proposal was funded, it could provide seed money for the next idea.

This situation gradually changed. By 1972 our proposals were scrutinized to ensure that we received no more than the necessary minimum. Rarely did we receive the total requested. By 1976 few of our proposals received enough money even to sustain a project, and we had to obtain support from more than one agency. Much of the time we had devoted to thinking about new projects was now spent writing and polishing proposals. Tight funding, increasing overhead, and additional constraints on spending have made it more and more difficult to begin new projects. Fortunately, the Lawrence Berkeley Laboratory has continued to provide seed money, making it possible for our research program to continue to evolve.

Innovation

I have originated several projects termed innovative by the award committees and others. The periods when I

The author is professor of physics at the University of California, Berkeley, and faculty senior scientist at the Lawrence Berkeley Laboratory, Berkeley 94720.

0036-8075/80/0822-0880$01.00/0

was beginning these projects were among the most difficult and stressful of my life. As Thomas Edison once quipped, "innovation is 1 percent inspiration, and 99 percent perspiration."

Isabella Conte, who studied innovation in architecture, suggested that there are two stages that must come first in the innovation process: preparation and incubation. They were the stages that I found the most uncomfortable. During preparation and incubation one asks many questions and obtains few answers. I was unsure of myself; my response to colleagues' questions about what I was doing was, "Nothing in particular." At times I hoped I would learn that an idea I had would *not* work, just to relieve the anxiety of doubt. Fewer than one in ten ideas outlived a week; of those that did, fewer than one in ten became an experiment. Preparation involves a considerable amount of reading, particularly in new areas of science. Some colleagues thought that I was loafing, and I was not sure they were wrong. The director of a national laboratory accused me of arrogance for suggesting that I could contribute to a field of research in which I had no experience.

The periods of preparation and incubation are the most fragile in the innovation process, and more attention should be paid to them. Many of the procedures followed in the scientific funding process have the unintended effect of suppressing these stages. To stop the growth of a tree it is not necessary to chop the tree down; it is sufficient to continuously clip off the top. The procedures and restrictions that do the damage were created to achieve a measurably good effect while causing unmeasurably small harm. One of the obstacles to scientific innovation in the United States may be the cumulative effect of many regulations, each one of which does "unmeasurably" small harm. I shall give examples to illustrate how features of the present funding system tend to suppress innovation.

When E. O. Lawrence was the director of the Radiation Laboratory at the University of California, Berkeley, he encouraged his graduate students to practice machining in the shops after hours. He knew that they would become expert machinists much more quickly if they took this opportunity to work on personal projects. Wear and tear on the machining tools would be negligible and the skill gained would improve research. Now government law prohibits this effective learning method. As a result, few scientists are proficient machinists, and few learn the capabilities and limitations of machine shop tools. Without this knowledge (acquired during the scientists' spare time), the scientist is unlikely to be able to design state-of-the-art hardware.

Restrictions on foreign travel also have a severe effect on innovation. Science is international in scope, and participation in foreign conferences is exceedingly important in the preparation stage. The number of experts in a given area is small; topical conferences provide an excellent way to meet and talk with them. Yet foreign travel is strictly limited, and that which is allowed is encumbered by special restrictions (for example, U.S. carriers must be used) unless the inconvenience is substantial. The importance to my research of several international conferences is clear to me, yet I attend such meetings far more rarely than I should. I do not know whether the restrictions on foreign travel were created to save money, benefit U.S. airlines and the balance of trade, or prevent the appearance of a boondoggle. But I am sure that a cost-benefit analysis would show the foolishness of these restrictions when applied to basic research, especially if the substantial harm to preparation could somehow be quantified.

Paper work is another problem. Every time I fill out a form I can see the reason for which it was created, but I doubt that the originator anticipated the substantial amount of time that I would have to spend filling it out. A large fraction of my research budget is allocated to overhead, in part so that the more complicated forms can be filled out by other professionals. The experimental physicists of decades past spent most of their time in the laboratory; I sometimes think that I spend most of my time at a desk. I have become a far more expert typist than machinist.

Teaching and consulting have played central roles in my preparation and incubation periods, although to many people they appear to conflict with research work. Perhaps due to this apparent conflict, there are rules that tend to suppress these activities. Teaching is one of the best ways to familiarize oneself with areas of science other than those currently being researched. A course in optics that I taught in 1972 as a part-time lecturer led directly to two research projects that were cited in the Waterman award. A colleague of mine wanted to volunteer to teach a certain course, believing it would help his research, but was not allowed to do so under his research grant. He was required to do full-time research, despite his judgment that a combination of teaching and research would improve his research productivity.

There are so many other examples of regulations which suppress innovation, although most of them seem too petty to list. It is only when one examines the accumulation of these regulations that the seriousness of the problem becomes evident.

Risk Taking

A funding agency must not be judged by its mistakes or by its "waste" of money any more than Babe Ruth should be judged by his strikeout record. Those who award research grants must not be discouraged from taking risks. Congress must make it clear to the funding agencies that it is proper and essential to take risks.

As I mentioned earlier, my own best work was begun during periods when it might have looked to an outsider that I was wasting time. A physicist's career is judged by his peers on the basis of his accomplishments, not his efficiency. We should apply the same principle to the funding of science. A funding agency should not be criticized for its mistakes if it has a good record of taking risks that bore fruit. In fact, one should regard with suspicion a funding agency whose projects always succeed, since constant success may indicate an overly cautious approach. It is easy to fund the established scientist who continues to work in his established field. It is risky to fund the scientist working in an area that is not yet established, or a young scientist working in a field that has many experienced researchers. When Warren Weaver retired as head of the Rockefeller Foundation, he said that his proudest achievement was that he had given substantial research support to all the Nobel Prize winners in medicine and physiology *before* they won the awards.

In U.S. funding agencies there appears to be little reward for initiative; on the contrary, the contract monitors can get into trouble for making a decision that might be counter to some official policy. The dreaded result of funding a project far from the mainstream of scientific work is a Golden Fleece Award. There are a plethora of rules and regulations that must be followed, and it is safer to turn down requests (or to delay them by submitting them to superiors for approval) than to take a chance. Taking a risk by funding an innovative project can lead to trouble, and there are many projects that are risk-free and whose support can easily be defended. But like the scientist's career, the funding agency and its contract monitors must be judged on a long-term basis. They must be encouraged to use personal discretion in addition to peer review. They must be expected to resolve disagreements between referees, not simply to fund projects for which a consensus exists.

Compartmentalization

To encourage basic research, one must support ambiguous research. (As Wernher von Braun once said, "Basic research is what I'm doing when I don't know what I am doing.") Nonetheless, the funding agencies are divided into compartments specializing in different areas of research. This specialization was undoubtedly designed to avoid waste and duplication, and to make certain that the monitors in charge of an area of research are those most expert in that area. However, compartmentalization has particularly bad side effects for innovation, as the following example illustrates.

In 1978 Luis Alvarez and three colleagues made a remarkable discovery giving direct evidence of the cause of the worldwide catastrophe that destroyed the dinosaurs and many other species 65 million years ago. Alvarez wanted to attend a conference in Denmark to discuss this discovery with other experts, and I offered to ask for travel funds from our contract monitor, who had partially supported Alvarez's salary during this research. When I called, the monitor said that although he had been able to justify the salary (as seed money), he could not pay for the trip, since his office was not supposed to support geology. The discovery fell in the wrong category.

Compartmentalization also inhibits research in areas that have not yet appeared as categories in the funding agencies. I have changed my area of research several times, from elementary particles to astrophysics to radioisotope dating to applied energy research. Staying within an area of research means requesting a renewal for an existing proposal; changing one's area of research is much more complex. Not only must "seed" research be accomplished, one must become known to the research community that will review the proposal. With the fierce competition for grants, one often must develop a personal relationship with the monitor who has the final responsibility for the funding decision. The monitor has to explain to scientists he has supported in the past why he is turning them down for a newcomer; pressure from a scientist whose new proposal is rejected is rarely as great as that from a scientist whose proposal is rejected for renewal.

I experienced such difficulties in two of the projects cited in the awards. Both times a specialist in the funding agency was uninterested in funding a project that seemed so far afield from the work he usually supported, and which would have to draw money from it. In the most recent example, research I was doing in elementary-particle physics led to the invention of a new and very sensitive method for detecting trace radioactivity. The method has applications in archeology, climatology, geology, and energy research. But the most obvious applications are in archeology, and because of this I was not able to find anybody in DOE willing to support the project. Seed support from the Lawrence Berkeley Laboratory enabled our group to proceed at a slow pace. The NSF rejected the project 3 months after the early work I had done was cited in the Waterman award.

The radioisotope detection project "fell into the cracks" between divisions of NSF. It had been sent to the division responsible for archeology, and the monitor in charge was faced with the choice of either rejecting my proposal or supporting it in lieu of archeologists who had received funding from him for years, and who were obviously doing good work. Since it was not even clear that my proposal belonged in his division, it was not too painful to reject. There was nobody in the NSF who had specific responsibility for the area of work outlined in the proposal, so there was nobody who would have to take the blame for rejecting it. The proposal was finally funded after an appeal to the director of the NSF, who sent it to the nuclear science division for reconsideration and rereview.

In retrospect, I can see that the initial rejection of the proposal was due in part to a misuse of the peer review system. I suspect that an innovative proposal is unlikely to get uniformly good reviews, for such uniformity is possible only in well-established areas of research in which a consensus has developed. My proposal was returned to the agency with a mixture of reviews, including several high rankings (A's) and at least one very low ranking (D). It should have been clear that both high and low rankings cannot be correct simultaneously. The low-risk approach for the agency is to reject such proposals, and fund only those that receive straight A's. But it is the innovative projects that are likely to get the mixed grades, and rejecting them outright is not satisfactory. The agency must give such proposals special attention, perhaps having them reviewed again by special referees who have experience with innovative projects.

The Alan T. Waterman Award consists of $150,000 in virtually unrestricted research funds. I feel that I have been able to use this money very effectively to start several new projects; yet I have spent only a fraction of it. I use the money as a guarantee; it enables me to begin research projects and hire people to work on them even though I have no promise of other funding. For several of these projects I have been able to obtain other funds, so I have been able to use the same Waterman funds over and over. The flexibility of the Waterman award allows me to use the funds with a great deal of leverage, and I feel that I am going to be able to return more science per dollar than with the other funding I have received.

Suggestions

The public, through the government funding agencies, has an absolute right to channel research in the directions it considers most appropriate. But although the right to do so is there, it is counter to the public's best interests to exercise this right. The government can best serve the interests of the public by facilitating basic research while minimizing attempts to direct it.

The Waterman award gave me the opportunity to discuss with the directors of the major funding agencies the problems I had encountered. It is clear that they are well aware of the nature of the problems, but it is difficult to find solutions that are acceptable to the wide variety of interests that might be affected. The very existence of this award convinces me that Congress knows that the best way to fund research by good scientists is to give them a free hand in spending their research funds.

I believe that most of the rules and regulations accomplish good, and I would not necessarily advise repealing them. But I believe that basic science is more fragile than the rest of our system. The most effective way to encourage innovation and discovery in science is to remove some of the bureaucratic burden pressing down on basic research. I recommend that federal funds designated for basic research be exempted from as many of these rules and regulations as possible.

We cannot obtain all of the benefits of the "free enterprise" system in science while maintaining public funding. But I think we can obtain some of those benefits by institutionalizing a few procedures that reward those who take risks successfully and reduce the punishment of those who take risks and fail. The most obvious solution is for Congress to mandate risk taking by writing guidelines encouraging personal initiative on the part of those who distribute funds while recognizing that some mistakes are inevitable.

The most fundamental mistake made by the funding agencies is in assuming that the ability to write good proposals is equivalent to the ability to accomplish good research. In response to a query I made to the NSF, I was told that a proposal should be as "polished" as a paper published in a major journal. Referees frequently expect all potential problems to be identified and their solutions outlined. Unfortunately, it is not an exaggeration to say that the agencies expect a proposal to outline the anticipated discoveries.

We should not expect research proposals to read like engineering proposals. To require that the solutions to all problems be obvious before the research is begun discriminates strongly against innovative work. The process of solving such problems is often the *substance* of research. In beginning several of my projects I did not know how I would solve all the anticipated problems; but I had confidence that I would be able to solve them.

Agencies that request polished proposals demonstrate a fundamental misunderstanding of the research process and of the amount of time that can be wasted polishing a text that will never be widely circulated and that probably will not be funded. I was once tempted to write an "unpolished" proposal requesting nothing more than the considerable funding required to produce a polished one. We scientists ourselves are much to blame; I know that I too have fallen into the trap of being overly impressed by polish.

It might be objected that, if less emphasis were given to the proposal and more to the accomplishments of the scientist, younger scientists would be discriminated against. However, even younger scientists usually have a record of achievement from their Ph.D. theses and subsequent collaborations with senior scientists. I would still allow the *option* of writing a polished proposal if no other way is available. But the *requirements* of such a proposal suppresses innovative work.

Certain features of the present funding system designed to increase the efficiency with which money is spent should be altered. The most important of these is the strict compartmentalization of the funding organizations, which makes it very difficult for a scientist to follow the directions that research takes. We must ease the transition in funding that scientists undergo when they change fields. One way to accomplish this is with seed money. Each monitor should be allowed (perhaps encouraged) to allocate a certain fraction of his funds, say 10 percent, to areas outside his speciality that are an outgrowth of the work he has supported. The monitor would decide how to subdivide that 10 percent among the scientists he monitors; some might get none, others might be allowed to spend 50 percent or more of their money on some new development. The agency's mission should not be considered when this 10 percent is distributed; I expect that in many cases the research would fall outside that mission. If the monitor finds that the research of those he monitors enters so many new areas that the 10 percent stricture becomes oppressive, then he should have the ability to move that research to a new section of the funding agency to avoid the complete loss of funds to his own area (so that he is not punished for supporting innovative work).

Obviously, such a system could be abused. It is important that Congress make clear that the goal is not to minimize abuse, but to support innovation. Abuse should be dealt with on a case-by-case basis, not by writing new regulations. Ten percent can represent a large amount of money in some areas of science, but it is the percentage and not the amount that is important. I believe that even this small percentage would have an enormously beneficial effect.

Not only should we stop punishing those who support innovative research, we should encourage and reward them. Perhaps the best way to do this would be to give special recognition—a small cash reward, for example—to monitors who have done a particularly good job in supporting innovative research. This would not only reward the monitor, but increase his prestige and alert others to the importance of recognizing and supporting innovation. Anybody could nominate a monitor, including scientists or superiors in the funding agency, but the award committee should be composed of scientists familiar with the problems of innovation and of those persons in the funding agencies most familiar with the problems of funding science. There might be a similar award for those who distribute money locally at the national laboratories.

Overview

Innovative science, like a small child, can be guided and encouraged, but well-meaning attempts to force it in preconceived directions can be counterproductive. The goal of the funding agencies should be to facilitate research, not to direct it. We are in a golden age of science, and most of us take for granted that it will last forever. But past golden ages have come to abrupt ends, conceivably for reasons so minute that they were never recognized by historians. If not abused, our present golden age could continue for a long time. And like a child, it could yield a return that will overwhelm the small investment required.