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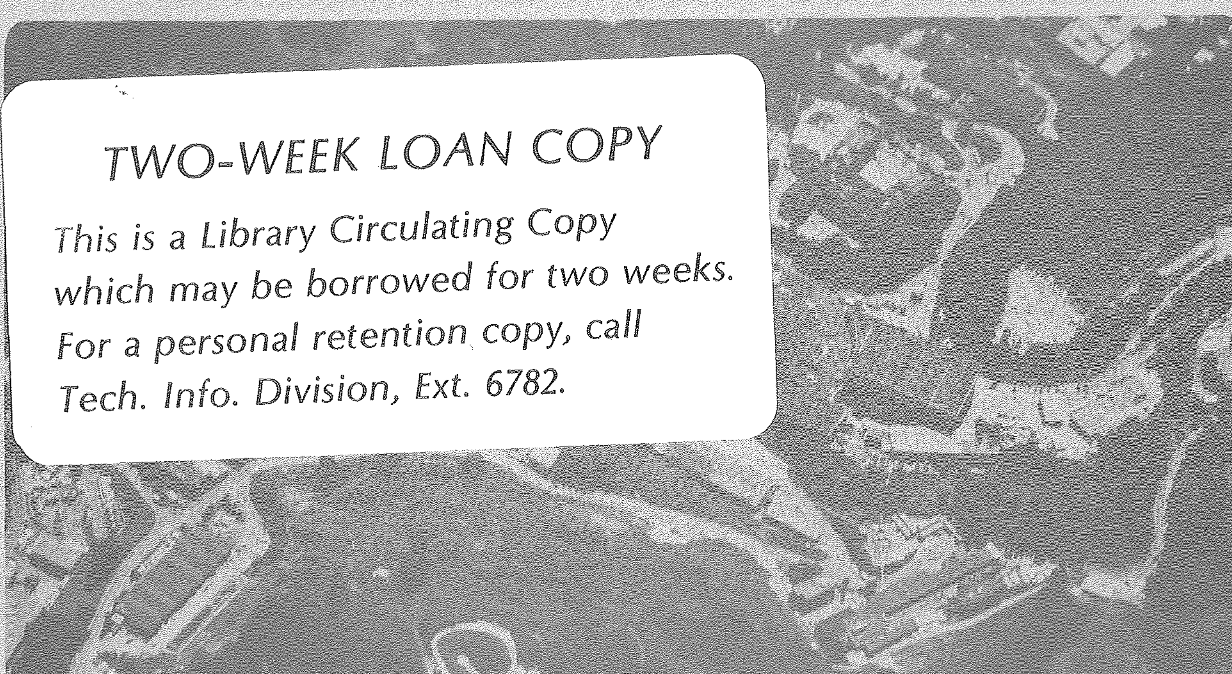

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INNOVATION AND SCIENTIFIC FUNDING

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It is difficult to judge the performance of the scientific funding agencies, for (like physicians) they can often bury their failures. 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. However, in 1978 just such a situation occurred. I was given both the Alan T. Waterman Award of the National Science Foundation and the Texas Instruments Foundation Founders' Prize for research that had been initially rejected for funding by the National Science Foundation, the Department of Energy, the National Aeronautics and Space Administration, 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 to testimony before the Committee on Science and Technology of the U.S. House of Representatives. This article is an adaptation of that testimony.

I had been 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 a grant we had 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 Department of Energy mission.

It is well-known in the research community that one cannot expect a proposal to be funded until one has done a considerable amount of work 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.

The situation gradually changed. By 1972 our proposals were scrutinized to make sure we would receive no more than the minimum necessary. 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 once devoted to thinking about new projects was now spent writing and polishing proposals. Tight funding, increasing overhead, and additional constraints on spending has made it more and more difficult to begin new projects. Fortunately the Lawrence Berkeley Laboratory has continued to make seed money available, 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, and I would like to make a few personal remarks about my experience. As I look back on the periods when I was beginning these projects, I recall them as some of the most difficult and stressful of my life. In a very well known quip, Thomas Edison said that "innovation is 1% inspiration, and 99% perspiration". Isabella Conte, who studied innovation in architecture, suggested that there are two stages which must come first: "preparation" and "incubation".

The periods that I found the most uncomfortable were those of preparation and incubation. During these periods one asks many questions and obtains few answers. I was unsure of myself; my response to colleagues' questions about what I was "up to" was "nothing in particular". At times I hoped I would get a clear answer 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 turned into an experiment. Preparation involves a considerable amount of reading, particularly in new areas of science. Some colleagues felt I was loafing, and I wasn't sure they were wrong. A 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 continuously to clip off the top. The procedures and various restrictions which do the damage were created to achieve a measurably good effect while causing unmeasurably small harm. I perceive that part of the problem with scientific innovation in this country may be the cumulative effect of many small regulations each one of which does "unmeasurably" small harm. I will 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 Berkeley, I am told that he encouraged his graduate students to practice machining in the shops after hours. He knew that the students would become expert machinists much more quickly if they took this opportunity to work on their personal and home projects. Wear and tear on the machining tools would be negligible, and the skill gained would improve research. Now government law prohibits this efficient and effective learning method. As a result few scientists are proficient machinists, and few learn the capabilities and limitations of the 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 placed on foreign travel also have a severe effect on innovation. Science is obviously 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, and participation in topical conferences is an efficient way to meet and talk with them. Yet foreign travel is strictly limited, and that which is allowed has special restrictions (e.g. U.S. carriers must be used) unless the inconvenience is substantial. The importance of several international conferences to my research is clear in my mind, and I believe 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 to 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.

Paperwork is another problem. Every time I fill out a form I can see the reason the form was created, but I doubt that the originator anticipated the substantial fraction of my time that I must now spend filling out forms, or the large fraction of my research budget which is allocated to "overhead" in part so that the more complicated forms can be filled out for me by professionals. I am told that experimental physicists of decades past spent most of their time in the laboratory; I sometimes think that I spend the majority 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 duties. Perhaps due to this apparent conflict, there are rules which tend to suppress these activities. Teaching is one of the best ways to familiarize oneself with new 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 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.

RISK TAKING

A funding agency must not be judged by its failures, or by its "waste" of money, any more than we judge Babe Ruth 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 my time. A person's career in physics is judged by his peers on his accomplishments, not by his efficiency. We should apply the same principle to the funding of science. A funding agency should not be criticized for failures if that agency has a good record of taking risks which succeeded. In fact one should look with suspicion at a funding agency whose projects always succeed, for constant success may be an indication of 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. I am told that 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 had won the awards.

In the 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 which might be counter to some official policy. The dreaded result for funding a project far from the mainstream of scientific work is a "Golden Fleece Award". There are a plethora of rules and regulations which 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 which are risk-free and whose support can easily be defended. As the scientist's career is judged on a long time scale, so must the funding agency and its contract monitors be judged. They must be encouraged to use personal discretion in addition to peer review. They must be expected to resolve disagreements between referees, and not simply to fund those projects for which a consensus exists.

COMPARTMENTALIZATION

To encourage basic research, one must support ambiguous research. (As Werner von Braun 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 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 will illustrate.

Last year Luis Alvarez and three colleagues made a remarkable discovery which gave direct evidence of the cause of the world-wide catastrophe which killed the dinosaurs and many other species 65 million years ago. Alvarez wanted to attend a conference in Denmark to discuss their results with other experts, and I offered to ask for travel funds from our contract monitor, who had partially supported Alvarez' 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 submitting a renewal for an existing proposal; changing an area of research is much more complex. Not only must "seed" research be accomplished, but one must become known to the research community who will to review the proposal. With the present 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 will have to explain to the scientists he has supported in the past why he is turning them down for a newcomer; pressure from a scientist rejected for a new proposal is rarely as great as that from a scientist rejected in his proposal 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 the detection of trace amounts of radioactivity. The method has applications in archeology, climatology, geology, and energy problems. But the most obvious applications are the archeology ones, and because of this I wasn't able to find anybody in the Department of Energy 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, three months after the early work I had done had specifically been cited in the Waterman Award.

The radioisotope detection project "fell into the cracks" between divisions of the National Science Foundation. It had been sent to the division responsible for archeology, and the monitor in charge was faced with the choice of rejecting my proposal, or of supporting it in lieu of archeologists who had received funding from him for years, and who were obviously doing good work. Since it wasn't even clear that my proposal belonged in <u>his</u> division, it wasn't too painful to reject. There was nobody in the NSF who had specific responsibility for the area of work outlined in the proposal, and 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 National Science Foundation, who sent it to the Nuclear Science division for reconsideration and re-review.

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 a 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 received straight A's. But it is the innovative projects that are likely to get the mixed grades, and outright rejection isn't satisfactory. The agency must give such proposals special attention, and perhaps have 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 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 direct research in the directions it considers most appropriate. But although they have the right to do so, it is counter to their 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 the problems I had encountered, with the directors of the major funding agencies. It is clear that they are well aware of the nature of the problems, but it is difficult to find solutions which are acceptable to the wide variety of interests that might be affected. The very existence of the Alan T. Waterman Award convinces me that Congress knows that the best way to fund research by a good scientist is to give him a free hand in spending his research funds.

On the average 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. If one wishes to encourage innovation and discovery in science, the most effective way to accomplish this goal would be to remove some of the bureaucratic burden which bears down on basic research. I would recommend specifically 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 these benefits by institutionalizing a few procedures which reward those who take risks successfully, and reduce the "punishment" of those who take risk and fail. The most obvious solution is to mandate risk-taking, by Congress writing into the guidelines that personal initiative on the part of those who distribute funds is to be encouraged, while recognizing that some mistakes are inevitable.

The most fundamental mistake made by the funding agencies is the implicit assumption 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 National Science Foundation, 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. It is (unfortunately) 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 <u>substance</u> 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 which request "polished" proposals demonstrate a fundamental misunderstanding of the research process, and of the amount of time that can be wasted in polishing a text which will never be widely circulated and which probably will not be funded. I was once tempted to write an "unpolished" proposal requesting nothing more than the considerable funds 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.

The only solution I offer is well-known: give more emphasis to the past accomplishments of the scientist, and less to the proposal. An objection might be raised, that giving less emphasis to the proposal and more to the accomplishments of the scientist would discriminate against the younger scientists. I don't think that this is a valid objection. Even the younger scientists usually have a record of achievement from their Ph.D. thesis and subsequent collaborations with senior scientists. And I would still allow the <u>option</u> of writing a polished proposal if no other way is available. But the <u>requirement</u> of a polished proposal is biased against 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 direction that research leads. We must ease the transition in funding that a scientist makes when he changes fields. One way to accomplish this is with seed money.

Specifically I suggest that each monitor be allowed (perhaps encouraged!) to spend a certain fraction, say 10%, of his money in areas outside his speciality which are an outgrowth of work he has supported. The monitor could make his own decision how to subdivide that 10% among the scientists he monitors; some might get none, and others might be allowed to spend 50% or more of their money on some new development. The mission of the agency should not be considered in distributing this 10%, and I would expect in many cases that the research would fall outside of that mission. When the monitor finds that enough of his research has moved into new areas that the 10% stricture is oppressive, then he should have the ability to move that research to a new section of the funding agency, without the complete loss of funds to his own area (so that he isn't "punished" for having supported 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 restrictions. 10% can represent a large amount of money for 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 eliminate the "punishment" of those who support innovative research, but we should encourage and reward them. I suspect that the least expensive and most effective way to do this would be to give special recognition to those monitors who have done a particularly good job in supporting innovative research, perhaps in the form of a small monetary award. 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 superiors in the funding agency or scientists, but I think that the award committee should be composed of scientists familiar with the problems of innovation, as well as those people in the funding agencies most familiar with the problems of funding science. Personally I can think of several people I would like to nominate for such an award, people who took risks to support my work. There might be a similar award for those who distribute money locally at the national laboratories, from the director on down to the heads of divisions.

OVERVIEW

Innovative science is much like a small child; it 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 now in a golden age of science, and most of us take for granted it will last forever. But golden ages in the past 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.

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