Finally, program officers have the ability to be much more flexible in the nature of what they recommend than is generally recognized. If you believe you have an imaginative approach to supporting your research program, discuss it with the appropriate program officer.

SOME FINAL THOUGHTS

Lastly, but perhaps most importantly, you should never invest too much of your self-esteem as a researcher in the outcome of the proposal process. For the funding agencies, it is not the individual that is being supported, it is the research activity proposed as it fits in the context of the overall program. If your sense of your worth as a researcher rises or falls on the basis of the success of your proposal, you are likely to be doing yourself a disservice.

Declinations can be devastating, particularly when they come for the first time. It may seem like more of a rejection of your research and your ability as a researcher than is the case. Frequently the reviews of your proposal will be very positive, and it is important to take the positive comments and build on them, rather than becoming discouraged. Negative comments should be carefully evaluated for the information they can provide to your future work. Take those making valid points and address them as appropriate in planning the future directions of your research. It is important to remember that a broad range of what program officers would term fundable work is declined. Generally such work is roughly on a par with some of what is funded.

Receiving a declination does not mean that your work will never be supported, rather that it is not being supported at this time. Likewise, receiving an award does not guarantee that your work will always be supported. Independent of the outcome, it is a good idea to discuss with the program officer the positive and negative factors in the decision and how you can improve your position the next time you submit a proposal. Sometimes you will get good ideas for modifying your methods or adapting your line of research to broader questions.

By recognizing that you can and should participate in competition for research support funds in the future, regardless of the disposition of any individual proposal, you help ensure that the process of competing for funds has some positive feedback to your research program and that research in the mathematical sciences remains vital. By approaching the process with imagination and creativity, you help us in the funding agencies remain flexible and responsive to your requirements for research support.

Comment

Edward J. Wegman

Bruce Trumbo's discussion gives an excellent overview of the grant process at the National Science Foundation and with it, some excellent advice on strategies for winning grants. Obviously the processes are different at the Department of Defense (DoD) agencies, so perhaps a few remarks in these directions would also be useful. My remarks, of course, no longer reflect any official view or policy and should not be interpreted to do so. My direct experience relates to the Office of Naval Research (ONR), but by extension also reflects frequent contact with the other DoD agencies, the Air Force Office of Scientific Research (AFOSR), the Army Research Office (ARO), the Defense Advanced Research Projects Agency (DARPA), the Strategic Defense Initiative Organization (SDIO) and, most recently, the National Security Agency (NSA). These agencies along with the Department of Energy (DoE) are sometimes referred to as the mission agencies. This is, I have often thought, a somewhat unfortunate label because it tends to color the attitude investigators, particularly young investigators, have of the agency. The tendency is to understand "mission" as a synonym for "applied," and, hence, to turn off theoretically minded young investigators. In fact, certainly during my tenure at ONR, the type of research funded was quite theoretical, but chosen with a view to its relevance to the mission of the United States Navy and the United States Marine Corps. Because antisubmarine warfare was a clear naval mission, for example, ONR tends to have a strong focus on topics related to sonar and nonacoustic signal processing. Thus, proposals related to time series analysis and stochastic processes tend to be more

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readily accepted than, say, proposals on design of experiments. Professor Trumbo discusses two major criteria for NSF proposals: 1) the quality of the proposed research and 2) the qualifications of the proposed principal investigator. For most of the agencies listed above, I would add 3) general relevance to the agency's mission. Do not, however, make the mistake of interpreting this to mean necessarily applied or project-oriented research. I believe the best way of keeping the differences between the NSF and the "mission" agencies in mind is to understand the difference in philosophical perspective. The NSF regards its clients as the academic community that is also the community from which its grantors are drawn. The "mission" agencies regard their respective sponsors, e.g., the Navy, the Marine Corps, the Air Force, the Army and so on, as their clients and the academic community as their resources for providing the basic technical support to their clients. This is a major difference in perspective.

Perhaps a few general words are in order. The NSF gives grants, but many of the other agencies award contracts. There is a technical distinction in the sense that a grant is a legal gift from the agency to the university or other research organization. The language of the grant may impose certain conditions on the grant, but deliverables are either nonexistent or limited. A contract is a legal instrument between the agency and the research organization that specifies deliverables, usually some sort of reports. The research organization can incur severe liabilities for defaulting on a contract. The mid-1980s have been filled with concerns about competitive procurements and to some extent the other federal agencies have used the grant instrument more frequently. The AFOSR principally uses grants whereas the ARO and the ONR use both. The ONR, the AFOSR and the ARO have the longest history of providing research support beginning in 1946 for ONR and 1952 for the others. They are most like the NSF. DARPA and SDIO are much more demonstration project-oriented and have a comparatively greater emphasis on technology as opposed to basic research. NSA started research funding a few years ago in connection with research on cryptology and has very recently expanded the program to include statistics, probability and applied mathematics.

Professor Trumbo emphasizes personal contact with the program officer. I would certainly give this a strong endorsement for someone trying to break into the federal funding cycle. The program officers at the mission agencies are usually not as closely bound to peer review as those at the NSF. Indeed, because it is principally their judgments that formulate which technical areas are relevant to the mission, they have substantial discretion. It is a mistake in any case to simply send in a proposal "out of the blue" and expect it to do well. If an investigator is an unknown quantity to a program officer, he or she may be quite unwilling to risk offering research support. After all, the program officer's own salary and reputation within the agency ride on the composite quality of the research program he or she stewards. If there has been a steady interaction, if the proposal is fine-tuned for quality and relevance and if the program officer has had enough personal contact to have built up some confidence in the proposed principal investigator, then there is every reason to believe some modest initial funding may be made available. I have often suggested that one way a person who is not well established can demonstrate his or her professional abilities is to propose to organize and hold a conference under some agency's sponsorship. Although a research grant for 2 or 3 years may cost $100,000, $200,000 or more (at ONR the average contract size is about $80,000 per year), $5,000 or $10,000 is sufficient to organize a conference. A program officer is much more likely to be able find this sort of funding than funding for a full blown research program and the opportunity to interact with the program officer and the agency will afford the principal investigator an opportunity for exposure. The bottom line is do not be greedy and ask for everything. A successful small scale start can rapidly lead to bigger things.

I'd like to close my discussion by pointing out several fiscal realities. First, the DoD agencies go through a competitive fiscal process known as the POM (Program Objectives Memorandum). The program officers write proposals for large scale projects within their organization and compete with other project officers for funding. Thus, the program officer relishes new ideas, particularly larger scale ideas that help his program become more competitive. The result of this competition is generally a new program known variously as thrust programs, research opportunity programs or accelerated research initiatives. These represent brand new sources of funds not previously committed to other investigators. Because the competition is much less stiff in these programs, it is very fruitful to stay aware of what they are and even, if possible, help in their development. This is a very big incentive for staying in close contact with the program officer because they are usually anxious to develop a suite of proposals addressing the new area and hence more than willing to reveal what new directions are. The second fiscal reality pertains not only to the DoD agencies, but also to NSF. Consider for example a hypothetical $2,000,000 on-going program. The program officer will generally plan to carry a project in the program for 3–5 years. This means that in any 1 year only some 20–33% of the funds will be available, say in our example $400,000–$667,000. Perhaps a

5–10% inflation factor is built into the continuing projects reducing funds available for new projects to between say $240,000–$600,000. Perhaps 50% of the investigators with projects finishing will successfully compete for funding for new projects, reducing available funding for totally new investigators to perhaps $120,000–$300,000. At an average contract size of $80,000 this means one to four new projects. Thus, although the overall budget for a program may seem large, the actual discretionary funds available for new principal investigators is comparatively small.

The advice given by Professor Trumbo is based on the perspective of a person who has served as program officer very ably a number of times. He deserves the thanks of the community both for the service rendered as program officer at NSF and for sharing his insights in the present article. The reader may also be interested in several other references related to research funding, notably Solomon and Wegman (1985) and Wegman (1986, 1987).

ADDITIONAL REFERENCES

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Rejoinder

B. E. Trumbo

The discussants have gone beyond the scope of my paper in several useful directions; it is a pleasure to thank them all for their thoughtful comments. Professor Wegman has given a clear account of the differences in philosophy and practice between NSF and the DoD agencies in the United States. Professors Zidek, Smith, Dall'Aglio and Bernardo have provided valuable insights into grants processes in Canada, the United Kingdom, Italy and Spain. Apparently, each national system for research support has attractive features that might profitably be emulated in other countries.

In addition to these descriptions of various funding programs, the discussions deal with a wide variety of important and controversial topics. On many of these I am content to let the discussants have the last word, but I have selected a few topics on which I would like to agree, disagree or speculate.

COST-BENEFIT ANALYSIS

Professor Zidek urges prospective applicants to consider whether the disadvantages of research support outweigh the advantages. This is valuable advice; the benefits of getting a grant are so clear that it is worthwhile to note the potential difficulties, both practical and philosophical. However, the overall tone of this section of his commentary is too negative for my taste. This is partly because not all of the potentially unfavorable factors in his long list are likely to affect any one applicant and partly because I think several of them are overdrawn, especially in the context of the paper. I offer the following perspectives:

- It does take time and thought to write a good proposal, but (as I have already said and as Dr. Sunley reiterates) much of this work is beneficial to the applicants' research program—whether or not the proposal is funded.
- It is hard to imagine that the kind of grant a young investigator is most likely to get (e.g., summer salary, some computer time or equipment, a little money for travel, etc.) will impose onerous administrative burdens. Furthermore, grants administrators at some universities are really quite helpful in dealing with the paperwork that *is* necessary.
- Investigators need not pursue topic-oriented funding programs that might divert them from "free inquiry" into their real research interests, "erode the quality of education" in their universities or violate their consciences. Perhaps the ideal "strategy" is for each researcher to decide what kind of research he or she does most expertly, enthusiastically and proudly, and then to seek support for it from all available sources. The vast majority of NSF funding is for **unsolicited proposals** on topics of the applicant's choosing.
- As Professor Wegman points out, the mission agencies support a great deal of basic theoretical research, which from the investigator's point of view may be quite unrelated to any application, military or otherwise. (I strongly suspect that