Statistical Science
1989, Vol. 4, No. 2, 121–150

How to Get Your First Research Grant

B. E. Trumbo

Abstract. In applying for a research grant, a young statistician or probabilist faces keen competition for limited funds. The quality of the research proposed and the qualifications of the researcher are major criteria in evaluating a project for funding. However, chances for funding can be improved if the application is carefully written, based upon an understanding of how it will be evaluated. In applying for support, the young researcher should consider the advantages and disadvantages of the various opportunities for funding. Specifics of the discussion center upon the National Science Foundation of the United States government.

Key words and phrases: Grant application, research proposal, research administration, government support of research, National Science Foundation

INTRODUCTION

This paper is written primarily for able young researchers in statistics and probability with limited experience in applying for research grants. In most countries, funds for research are available from one or more government agencies and from private foundations. The discussion here will center upon the National Science Foundation (NSF), an especially important source of support for young researchers residing in the United States. Wherever possible, however, I shall try to deal with general principles rather than with details. Therefore, established researchers and those seeking support from other sources may also find parts of the discussion useful.

The crucial ingredient of a successful grant application is a really good research idea—the combination of an important problem and a realistic plan for pursuing its solution. No amount of "grantsmanship" can improve a mediocre project. However, in view of the keen competition for limited research funds, the success of a grant application or **proposal** often does depend upon how effectively it presents the researcher's idea and qualifications.

This paper contains my personal views on how young researchers can effectively approach NSF for funding. My opinions are based on my experience as a former NSF program director, but this paper has no official standing.

B. E. Trumbo is Professor of Statistics and Mathematics at California State University, Hayward, California 94542. During the academic years 1974–1975, 1978–1979 and 1985–1986 he was a Program Director at the National Science Foundation handling grant applications in statistics.

Almost all government agencies and foundations publish official informational booklets explaining their programs and specifying how to apply for awards. The latest editions of the NSF booklets are available free of charge from NSF (and perhaps from your university research office). Three of these are required reading for all statisticians and probabilists seeking NSF support:

[1] *Grants for Research and Education in Science and Engineering.* This is the official manual for grant applicants, which includes information on qualifications, criteria, procedures, etc. Notice in particular that United States citizenship is *not* a requirement for most kinds of NSF support. Appendix VII contains a valuable list of NSF telephone numbers.

[2] *Guide to Programs.* Here you will find important general background material about NSF and capsule descriptions of the kinds of research supported by each of its organizational units, called **programs.**

[3] *Summary of Awards in Statistics and Probability.* This booklet, prepared by the Division of Mathematical Sciences (DMS), contains general statistical and budgetary information about the Statistics and Probability Program. It also gives the titles and investigators' names of all grants made under the program during the most recent fiscal year.

Notational conventions: Numbers in brackets, e.g., [1], [2], and [3] above, refer to United States government publications; see the References. Where NSF jargon must be used, it is introduced in **boldface**, e.g., the words **proposal** and **program** above. Required parts of an NSF proposal, as specified in Part I of

[1], appear in SMALL CAPITALS, e.g., the COVER PAGE mentioned in the next section.

DEALING WITH THE BUREAUCRACY

It would not be inaccurate to view NSF as a complex government bureaucracy with hundreds of employees handling tens of thousands of proposals a year, usually taking half a year to respond to any one of them. You will increase your chances for support if you focus upon the particular NSF program that matches your current research interests, deal with the relevant **program director** on a person-to-person basis, and understand what happens to a proposal after you submit it to NSF. To set the stage for the discussion on proposal preparation in the next section, this section gives you an inside view of some aspects of NSF organization and procedures. (See Mittal (1987, 1988) for more detailed information on some of the topics covered here.)

Starting Near the Center of the Labyrinth

Theoretical research and some methodological research in statistics and probability are handled by the Statistics and Probability Program (S&P). In seeking a home at NSF for your research, this is the logical place to start. A look at the titles of grants and the names of investigators listed in [3] should leave you with very little doubt as to the boundaries of this program.

If your research deals mainly with applications to the physical, biological, social, behavioral or computer sciences, then [2] will help you to learn about other programs that may support your work. Flournoy (1988) and [8] provide additional information on many of the applicable programs. Another useful perspective can be found in [3], which references research funded jointly by several programs. See Levine (1986) if you contemplate submitting a proposal to a program in the social or behavioral sciences.

The Measurement Methods and Data Improvement Program of the Division of Social and Economic Science is one program that has been an important source of support for statisticians doing research in statistical methodology. As this is written, it seems likely that this program will strengthen and broaden its support for statistical research beginning in 1989.

Personal Contact

Typically, the two program directors for S&P are professors who have taken leave from regular university positions to work at NSF for 1 or 2 years. In the past, they have split the program along a fuzzy boundary between statistics and probability according to their own professional areas of interest. Some programs have permanent NSF employees as program directors. In any case, you can expect an NSF program director to be a professional scientist in the general area of your work and to be knowledgeable about academic life.

If you decide to submit a proposal to the S&P program, you should call ahead, perhaps during the late summer or early autumn. Give a capsule description of the work you intend to do and ask to talk to the program director who will handle proposals in that area. (The current phone number is 202-357-3693; if necessary, you will be referred to other programs.) This call will introduce you to the person at NSF who will be most directly involved with your proposal and it will alert him or her to expect your proposal. This is also a chance for you to ask about any points of procedure that are unclear to you and for the program director to comment on any unusual aspect of your proposal that may need special attention. However, do not expect program directors to act as your research consultants or your private tutors in grantsmanship.

A Matter of Timing

Officially, proposals may be submitted to the S&P program at any time of year, provided that you allow 6 months for NSF to process your proposal. However, periodic events in the academic year and in the Federal fiscal year combine to impose an annual cycle upon the program. In a typical year, for example, the program is out of money between mid-April and mid-October. As a result, the *best* time for most young investigators to submit a proposal to S&P is from mid-September to early November. You should have a discussion with your program director in case your proposal will arrive at NSF before September 1 or after December 15, because it will probably require some sort of special handling—either to hold it long enough for a fair comparison with competing proposals or to improve its chances for a full evaluation before the program budget for the year is spent. (Some programs have firm deadline dates for proposal submission.)

You can help keep your proposal from being delayed in its early travels at NSF by listing the program of submission in the first box on the COVER PAGE, e.g., "DMS—Statistics and Probability." When it has been entered into the NSF computer system and assigned a proposal number, you will receive a post card acknowledging its receipt. This may take several weeks. (Actually, program assignments are ultimately at the discretion of NSF, and so you could find that your proposal has been moved to a different program. If your choice of program was based on a conversation with its program director, such a change is very unlikely.)

A Jury of Your Peers

NSF uses what is called the **peer review system** to evaluate proposals. (Recently, the terminology **merit review system** has also been used.) Under this system, the primary information for evaluating your proposal comes from fellow scientists who perform their services without pay. They are asked to evaluate proposals according to the NSF criteria explained in Part II of [1], including assessments of the merit of your research idea and your competence to do the work. If the applicant has received previous NSF support, reviewers are also asked to comment on the results of it. (See [4] and its references for additional insight into the NSF review process.)

Shortly after your proposal arrives in the S&P program, your program director will scan it and select about five initial reviewers to evaluate it. As we see below, the choice of reviewers is subject to guidelines that help to ensure balance and objectivity. The S&P review process is conducted by mail. (Some programs use **panels** of reviewers, which meet as a group to evaluate large numbers of proposals at a time. Others use a combination of mail and panel reviewers.)

Often the program director must choose additional reviewers if not enough of those in the first group respond in a timely fashion or if differences of opinion arise in the early reviews that he or she wants to pursue. Reviewers are a scarce resource. The sequential approach can be slow, but it uses reviewers efficiently.

How Reviewers Are Chosen

Reviewers of your proposal are chosen to provide two kinds of assessments. Some of your reviewers will be experts in your subfield, selected for their ability to comment on the technical details of your proposal. Probably one or two will be chosen specifically because they can view your proposal in perspective, evaluating the importance of your work to the field as a whole. Often the same reviewer will give evaluations of both kinds.

Among those restricted from evaluating your proposal are people who would profit financially from its funding, colleagues at your campus and your present and recent collaborators and teachers. Partly for this reason, your BIOGRAPHICAL SKETCH must include a complete list of your papers in progress and your publications within the past 5 years, clearly showing all co-authors. You should also list your thesis and give the name of your thesis advisor. If there is anyone else you feel may have a **conflict of interest** (positive or negative, personal or professional) in reviewing your proposal, you should let the program director know this in confidence. (For example, the program director may not know about your business partnership with someone at another university or that a seemingly appropriate reviewer has just married your brother.)

Choosing reviewers is one of the program director's most intricate tasks. At any given time, the number of proposals in an area may be quite large and any one reviewer can be asked to evaluate only a few of them. Thus, the program director must consider dozens of potential reviewers with expertise in each of the relevant specialties and try to make the best choices from among them for each proposal.

According to [1], it is proper for you to suggest reviewers to the program director, and in some programs researchers regularly submit lists of suggested reviewers along with their proposals. However, this has never been a common practice in the S&P program, and so I do not recommend it for young investigators applying to S&P. There are helpful indirect ways to point the program director to the kind of reviewers you think would most fully appreciate what your proposal has to offer.

First, your BIBLIOGRAPHY is a source of possible reviewers for your proposal. Second, you can take care that your abstract (PROJECT SUMMARY) is clear and well focused, ending it with a short list of key words. (If appropriate, this list should contain words that may not be obvious from reading the abstract, such as alternate terminologies.) Reference to key words in recent issues of the *Current Index to Statistics* and to entries in the *Science Citation Index* may help the program director to narrow the field of potential reviewers to those who are especially well-qualified to comment on your proposal.

The Decision

A review consists of two parts: a narrative commentary and a rating. The rating scale is ordinal with official definitions of the categories: excellent, very good, good, fair and poor (abbreviated as E, VG, G, F and P, respectively). Some reviewers treat the scale as continuous, giving ratings such as E−, E/VG or G++.

Based on the reviews of your proposal, reviews of competing proposals and the program budget, the program director makes a recommendation on funding your proposal. The ratings of each review are tempered in view of the substance of the reviewer's comments and in view of how his or her ratings of any past proposals compared with those of other reviewers. Program directors are also aware that dedicated researchers are understandably enthusiastic about the importance of their own philosophies or specialties and that this may cause biases in their reviews of your proposal. Finally, the program director must take into account such factors as the balance of funding among

the various subfields within the program, the age distribution and geographical distribution of those supported, and the need to bring under-represented groups into the mainstream of research funding.

The program director's recommendation is reviewed, mainly on budgetary and procedural grounds, at several higher administrative levels before a final decision is made. Because of this, you cannot expect your program director to make promises or predictions about the outcome of your proposal.

Is "Very Good" Good Enough?

By the standards of the Federal government, NSF is a small agency—its annual budget would keep the Department of Defense going for only 2 or 3 days. In round numbers, NSF spent about $1.6 billion in support of research in fiscal year 1988. Of this total, the S&P budget was a little more than $7 million. Depending on exactly how one counts, this budget supported between 200 and 250 researchers. Even though a roughly comparable amount of funding for statistics and probability comes from other NSF programs and from other Federal agencies, we see that there are many more statisticians and probabilists in the United States than can get Federal support for their work. The Institute of Mathematical Statistics alone has over 2200 members with United States addresses, and the American Statistical Association has over 15,000 members. An increasing number of well-qualified young researchers must compete with each other and with established researchers for the limited funds available.

About 40% of the proposals for new projects were funded by the S&P Program in 1987. Even though the program director's evaluation is a complex and subjective one based on much more than the ratings, it is fair to say that in recent years funding has gone to almost all proposals with average ratings nearer to E than VG, to somewhat less than half of those averaging around VG and to very few with lower average ratings. In my experience, a young investigator in the VG range has a slightly better chance of getting support than an established researcher rated similarly.

Useful Feedback

Whether or not your proposal is funded, you will receive copies of the comments and ratings in your reviews, but you will not be told the names of the reviewers. (If your proposal is submitted jointly with other researchers, only the first-named **principal investigator** receives these copies.) You may find that some personal references have been edited out of your reviews to protect the privacy of others. Example: "This is a far more important proposal than the one by [ ] that you sent me in the same mailing, but somewhat less exciting than the [ ] proposal that I reviewed last year."

After you have read your reviews, you should call the program director to discuss them. You may find out that some of the reviews weighed much more heavily than others in his or her recommendation on your proposal. Knowing the reviewers' identities, for example, the program director may know that one reviewer's "In over a decade of intensive work in this area . . ." presages a lot less insight than another's modest "In the last few years, I have not kept up with this field as well as I would have liked, but. . . ."

Whether or not you receive an award, try to find out how near the borderline your proposal was rated. This information, combined with comments in the most important reviews, can be particularly valuable to a young investigator in deciding how to allocate research efforts and how to improve future proposals.

If at First You Don't Succeed . . .

If your proposal was **declined**, but came close to being funded, you should consider whether to submit another one next year based on the same research idea. Think about how you could use reviewer feedback and your own evolving perspective on the idea to improve the currency, relevance, clarity, focus, depth and/or breadth of your next proposal. It often takes young investigators several tries before the first success.

It is not just a good idea for you to try to improve your proposal before resubmission, it is a requirement. NSF will not review another proposal on the same topic unless you make substantial revisions.

WRITING YOUR PROPOSAL

A proposal to NSF may include the work of one or more researchers. For the present, let us assume that you plan to apply alone. Partly, the task of writing a proposal consists of following the instructions in Part I of [1]. This section looks beyond the instructions to some techniques of effective presentation.

The medium of technical writing most familiar to young scientists is the research paper. *A proposal is not a research paper.* A paper reports work you have done; a proposal outlines and seeks support for work you hope to do. A typical research paper is read by a sizable audience of people interested in its specialized topic; a good proposal is tailored to the diverse needs of the very few who will read it. Most papers are revised between submission and publication; the submitted version of a proposal is the only version that counts.

The Bright Idea

Your PROJECT DESCRIPTION should make a convincing case that your idea can lead to an exciting new development in your field—a fundamental advance, an insight that shows new connections among important ideas in several areas, original results that others will seek to build upon in their work or ideas that will lead to important new kinds of applications. For example, if you intend to fill in a gap in a program of research that an established researcher has pursued successfully for a number of years, you should explain why your topic could become a chapter heading rather than a footnote in The Book about this area. Reviewers will be especially suspicious if it appears that you are proposing to embellish your thesis to produce results that are of limited interest, even if they are technically difficult.

Remembering that the program director and perhaps one or two of your reviewers may not be specialists in your subfield, you should begin with a brief history or survey of recent developments surrounding your research. Show how your idea fits in and why it is important. If you are aware of connections to theoretical ideas in other fields or of potential applications, you should mention them and say whether you propose to explore them. Of course, you must also include enough technical detail that specialists in your area can understand the precise nature of your project.

I have already mentioned that program directors get ideas for reviewers by looking at bibliographies. Properly coordinated with your project description, your BIBLIOGRAPHY can also become an effective way to communicate with your reviewers.

Obviously, you must include all of the papers upon which your work is based. Some reviewers may welcome a reference to a relevant survey paper, if one is available. The ones who are technical experts will expect to see references to their own work on closely related problems, even if your methods do not build on their results. This is not *just* a matter of ego: the best way for reviewers to think about your problem is to see how it relates to ones they already understand well. A BIBLIOGRAPHY that goes on for more than a few pages is probably too long. On the other hand, limiting your references, say, to a few standard graduate textbooks, your thesis and two or three papers by your thesis advisor damages your credibility and fails to provide needed information.

Method, Time and Money

Your PROJECT DESCRIPTION should include a research plan in which you discuss how you intend to attack the problems you propose to solve. It is not expected that you will be able to predict exactly the path that your research will take or that you can foresee what results you will obtain. But it is important to show that you understand what you are up against and that you have some reasonable methods of attack in mind. This part of the proposal is *crucial* for young investigators (and often treated too lightly by more established ones).

You should tell what mathematical techniques you intend to use. If you have some preliminary results, you should indicate how you got them and say whether you expect the same methods to work more generally. If you have decided that the methods other researchers have used for related problems will not work for your problem, you should explain (tactfully) why not.

Another important part of your proposal is a realistic assessment of time, money and other resources you will need in order to do your research. First, consider the resources that may already be available to you such as libraries, computers, data bases, a local seminar series on related topics, etc. (Presumably, you will not be doing your work on the proverbial desert island, and that should be clear from reading your proposal.) Then, decide what funds to ask for, based on an appropriate compromise between what would be ideal and what it is realistic to expect.

The basic annual BUDGET for a grant in the mathematical sciences consists of money for 2 months of salary (usually used during the summer); any necessary money for computing; a $2200 (the 1988 figure) allowance toward travel, publication charges, secretarial help, etc.; plus university overhead charges. Most grants to young investigators are limited to 1 year (occasionally 2) of support at this spartan level. With strong justification, a grant might also include funds for items such as computing equipment or programming assistance. Money for assistance necessitated by a physical handicap should be requested. It is very unlikely that a grant for a young investigator will include graduate assistants, postdoctoral associates, additional paid research time during the regular academic year, etc. (Grants for established researchers sometimes carry a commitment for 3 years of support, subject to annual progress reports and updated research plans. They sometimes include limited funds for graduate students and postdocs; but they rarely exceed the $2200 annual allowance per investigator.)

There are signs that grants in the mathematical sciences are gradually emerging from the "one man and his blackboard" view of funding. Using the information in the last paragraph as a realistic base, you should request what you really need in order to do efficient research for 1 or 2 years. You will not get more than you ask for and a properly defended request for more than the basic amount will not diminish your chances of getting a grant. If your proposal is favorably

reviewed, the worst that can happen is that your budget will be cut.

Program directors have hard choices to make between the number of proposals that can be funded and the amount of money that can be given on each grant. If you ask for 2 years of support, you should say what you plan to accomplish during each of the 2 years. Thus, the program director will understand what will be left out if support must be cut to 1 year, but a 1-year grant will not seem futile. Similarly, you should distinguish budget items that improve efficiency or save time from those that are absolutely essential. Never leave any doubt that a budget item (e.g., programmer, software, computer time, equipment) can actually be obtained if it is funded: give sources, names and specifications. You are not bound by these choices if circumstances change. If you ask for equipment, be sure to address such issues as space, installation, maintenance and security.

Many scientists are troubled by the large amounts added to budget requests by universities for overhead charges or **indirect costs.** You should understand that what we have been thinking of as "your" proposal is technically a proposal from *your university* to NSF. Each university computes these charges according to a formula negotiated with an auditing agency of the Federal government. (The same formula is used in applications to all Federal agencies.) Most universities are unwilling to ask for less than their formulas allow. On the other hand, if your proposal requests a substantial amount of permanent equipment, your university may be willing, and may be expected, to pay part of the cost in **matching funds.**

Your Credibility

An important question in the evaluation of your proposal is whether you have the required technical expertise, vitality and self-discipline to carry out your research plan. For an established scientist, his or her recent history of published research on related topics, detailed in the BIOGRAPHICAL SKETCH, provides a strong indication of the performance that can be expected. As a young researcher you may not have had time to establish such a "track record." Furthermore, as we have already seen, some of the people who know you best are prohibited from reviewing your proposal. Thus, you must take full advantage of the opportunities that you *do* have to establish your credentials to perform.

Make sure that all of the work you have done is accessible. If you have finished a paper in the area of your proposal that has not yet been published, list it in your vita and make it an appendix to your proposal. (The same suggestion applies to any important reference not readily available to reviewers.) Volunteer to give seminar talks at neighboring universities and make an effort to give contributed papers at professional meetings. Even if you do not plan to apply for grants, such communication is part of your development as a professional scientist. But if you do, it is highly likely that your audiences will contain some of your present or future reviewers. Let your program director know as far in advance as possible of any talks you plan to give during the time your proposal is being evaluated.

Start work on your proposal early enough that you can develop it through several drafts and still submit it by the desired date. During this process, try to get a variety of colleagues to read your emerging proposal and to offer constructive criticism. Preferably these should include at least one technical expert in the area of your work, an experienced researcher who has written successful proposals and someone a bit outside your area of specialization who can judge whether you have made a generally persuasive case for the importance of your project. If you find yourself having to clarify, explain or amplify some parts of your proposal for these "prereviewers," then you probably need to do another draft. The finished version must speak clearly and convincingly for itself.

Finally, proofread your finished proposal carefully before you submit it. Serious grammatical and typographical errors can obscure your meaning and make you look foolish. To some reviewers minor errors are merely distractions; sterner reviewers take them to be evidence of sloth or incompetence.

STRATEGIES AND OPPORTUNITIES

Applying on your own for a standard research grant puts you into direct competition with other researchers of all ages and levels of past accomplishment. Even though it is a competition judged with sensitivity to the needs of young investigators, it is the least sheltered of the ways you can seek NSF support. This section discusses a variety of alternatives, each with its own advantages, disadvantages and restrictions.

Group Proposals

One alternative to an individual proposal is a joint proposal with other members of your university who share your research interests. Let us look at one *idealized scenario* that emphasizes the differences from going it alone:

> The **principal investigator** (PI) for your group proposal is an established researcher with experience in writing successful proposals. The members of your group are all working on related topics, perhaps including some joint work, and they frequently talk together about

their successes, difficulties and methods. In preparation for the proposal, the group discusses how to make the best case for each project and for the proposal as a whole. Each member prepares material describing his or her individual work (following the suggestions made in the previous section). The PI asks for some revisions and then assembles the final versions into a unified and complete proposal that includes an overview of the work of the group.

The NSF program director handling such a proposal will request individual reviews on each researcher, and only those researchers who receive competitive evaluations will be funded. The standards are the same as for investigators applying alone. But what will the reviewers see when looking at your part of such a proposal? They will see a statement of your project that has benefited from group discussion and the participation of an experienced proposal writer. They will see that you are part of a coherent research group, and they may interpret the arrangement as an implied recommendation of you by the PI. They may think you will be more productive in such a milieu than if you were working alone—and they may well be right.

At its best, this kind of a proposal can help to establish your credibility as a young researcher who is likely to produce important results. Other factors are that funding for the whole group may be approved for a 3-year period and that graduate students or postdoctoral researchers may be supported as part of the group. To the extent that these factors actually help you in your research, they are advantages worth considering. They probably would not have been available to you on your first grant if you had applied alone. For less cohesive "groups," the advantages of a group proposal tend to disappear.

Here is a list of four potential *disadvantages* to you in applying as part of a group: (1) If some of your colleagues are truly outstanding researchers, your part of the proposal may suffer by close comparison, even if it is of high quality. (2) Reviewers may focus their attention on the work of the better-known members of the group and make only superficial remarks about your work. (3) If some parts of the proposal are unimportant or ill-conceived, they may reflect on the credibility of the whole proposal. (4) Reviewers usually tend to sympathize with the difficulties faced by young investigators, but here they may see you only as a part of an established group and judge you accordingly. (The PI may, of course, gently remind the reviewer: e.g., "Jane Doe, the newest member of our group, brings a fresh perspective to this project with her work on. . . .")

Administrative considerations are that you will have access to your reviews only through the PI and that, if NSF supports the proposal, the PI will control the expenditure of all nonsalary funds (e.g., computing time, travel, publication expenses).

Collaboration at a Distance

You need not be at a major research university to participate in a group project. Meaningful collaboration can extend beyond university or geographical boundaries. I suspect that new communications technologies will encourage such arrangements. (See Aborn and Thaler (1988) and their references.)

There are several ways in which NSF can support joint research on one topic at several locations. Presently, these include cross-referenced proposals that are concurrently evaluated by overlapping sets of reviewers (by informal arrangement with the program director), subcontracts (see Part I of [1]) and Research Opportunity Awards (see Chapter 7 of [2]). Other ways, specifically tailored to the needs of mathematical scientists, are being developed now and may soon be available. Talk to your program director in advance about the best arrangement for your circumstances.

Postdoctoral Researchers

If you received your Ph.D. recently (less than 5 years ago for NSF), the PI of a proposal or of a continuing grant may request you as a postdoctoral researcher on his or her project. In this case, the PI vouches for you as a promising candidate to do research of high quality and agrees to be your mentor during the period of support.

Depending on a variety of factors, an NSF program director may decide to evaluate the request for your postdoctoral support using reviews, recommendations or a combination of both. The conflict-of-interest rules that apply to reviews of a proposal do not apply to recommendations for a postdoctoral position.

The plan may be for you to work closely with the PI on research already under way or for you to work fairly independently on different, but related, problems. If you are working on a problem of your own, you can expect that you will be asked to write a description of your proposed work for the PI to pass on to the program director for evaluation.

Postdoctoral positions were once rare on NSF grants in the mathematical sciences, but in recent years increasing amounts of money have been expressly reserved for funding postdocs. NSF recognizes that **postdoctoral researchers** in the mathematical sciences may work more independently than **postdoctoral associates** in the laboratory sciences. In particular, a postdoc on a grant in the mathematical sciences may be a tenure-track member of the faculty at the PI's institution (contrary to what you may suspect after reading Appendix V of [1]).

Other Opportunities for Young Researchers

Currently, there are three other methods of support for recent Ph.D.s:

Mathematical Sciences Postdoctoral Research Fellowships (see [5] and Chapter 5 of [2]),
The NSF-NATO Postdoctoral Fellowships in Science and Engineering (see [6] and Chapter 6 of [2]), and
Presidential Young Investigator Awards (see [7] and Chapter 6 of [2]).

All three involve keen competition among candidates from diverse disciplines, are evaluated by specially chosen panels of scientists and rely heavily on letters of recommendation for evaluation.

These awards are administered by different parts of NSF and have different restrictions, deadline dates, criteria and evaluation procedures. (For example, the first two of these require United States citizenship or native residency in a United States possession.)

Opportunities for Under-represented Groups

NSF has a variety of programs for members of groups that have not historically received their share of research support: persons employed at predominantly undergraduate institutions, women, minorities and the handicapped. If you are a member of one of these groups, you should look at Chapters 6 and 7 of [2], request additional information brochures where available and consult your program director to see whether you are eligible for one or more of these programs. If so, you should strongly consider applying.

The criteria and methods of administration of these programs have changed somewhat from year to year. Therefore, it is especially important to make sure that you obtain up-to-date information. Read the eligibility rules carefully, because they are sometimes different than the name of the program might lead you to expect. For example, you may apply under the Research in Undergraduate Institutions (RUI) program if your *college or university* qualifies as a predominantly undergraduate institution—even if your department offers a master's degree.

To avoid any confusion at NSF, list the program acronym (e.g., RUI) in the second box on the COVER PAGE of your proposal. Notice that, perversely, NSF uses the word **program** in two senses: organizational unit (e.g., S&P) and special funding activity (e.g., RUI).

One Is Enough

One *poor* strategy for trying to improve your chances for funding is to bombard a program with multiple proposals on different topics. I have never known a case where a young investigator has benefited from having several proposals under review at a time. In each yearly cycle, you should select the one topic you consider to be the most promising for you to pursue and concentrate your full time and energy on presenting that project to best advantage.

If two exciting ideas are very closely related, either in subject matter or in method of attack, perhaps they could be combined into a single proposal covering 2 years. Even this should be done with care to make sure that the combined project is not weaker than either of its parts. What you regard as versatility, the reviewers and the program director may view as vacillation.

OTHER AGENCIES

This section digresses briefly from our NSF-centered discussion to make brief mention of other agencies of the United States government that support significant amounts of research in statistics and probability:

Department of Energy,
Department of Defense agencies, including
  Air Force Office of Scientific Research,
  Army Research Office,
  Office of Naval Research,
National Institutes of Health (NIH),
National Security Agency.

Consult the administrative directories of the major professional organizations for the telephone numbers and addresses of these Federal agencies.

Congress has assigned different roles to the various Federal agencies that support research. NSF is committed to "support basic research [in science and engineering] across the full range of disciplines" (Erich Bloch, Forward to [2], 1987 edition). On the other hand, the **mission** agencies emphasize research having immediate or potential application to their areas of interest. NIH supports basic and applied research in the medical sciences and in a wide variety of related scientific areas. *Only* NIH may support work in clinical medicine. (Reif-Lehrer (1989) gives advice for NIH applicants.)

It is often a good idea to submit proposals covering the same work to several agencies at the same time. The project descriptions may be identical or they may be individually tailored to the interests of the various agencies. If you apply to more than one agency, you must tell each one of them about all of your proposals. In an NSF proposal, for example, show the acronyms of the other agencies on the COVER PAGE, give details in the STATEMENT OF OTHER SUPPORT and let your program director know immediately of any later changes in this information.

Each agency will make an independent evaluation of the proposal you submit. Any agencies that decide to support your work will coordinate their funding to avoid overlapping awards.

IS IT WORTH THE TROUBLE?

Obviously, the main reason to submit a proposal is to get money for your research. In view of the competition for research funds, some researchers wonder whether it is a waste of time to apply. In making this decision, you should consider a number of factors, including the importance you attach to your research and the likelihood that you will be able to do it without support. But you should also consider that an award is not the only thing you have to gain from the application process.

Introspection and Advice

Writing a proposal focuses your attention on issues you may be all too likely to put aside in your day-to-day work. Ideally, it involves looking at your options and selecting the ones most likely to produce important results, thinking about how your research connects with the rest of your field, getting the opinions of your colleagues, determining what resources you need and setting a realistic timetable for your work.

The feedback you receive from the evaluation of your proposal will let you know what several knowledgeable and influential people in your field think of your idea. Some of them may make useful suggestions. All of them will learn something about you and your work.

Pressure on the System

The amount and kinds of support NSF provides for various fields of science are a result of both planning and reaction. One of the forces NSF reacts to is "proposal pressure." If, over a period of several years, one area of science consistently generates a disproportionate number of VG-rated proposals that cannot be funded, then the amount of money available for that area will tend to increase.

Proposals can also generate ideas for new *kinds* of NSF support. For example, I can recall that I had to make quite a fuss in 1974 to get approval for a few hours of computer time on a theoretical statistics grant. Nowadays, partly as a reaction to many well-reasoned requests, the Division of Mathematical Sciences encourages the use of computers as tools in theoretical research through a variety of funding programs. See Eddy (1986) for a discussion of the impact of these programs on statistics.

Every thoughtful grant application is a vote for increased support of the kind of research it proposes.

If you think you have a promising idea for a research project, I urge you to submit a proposal. The only way to get support is to apply. Moreover, even "failures" contribute to your professional growth, and they may help to improve the funding for statistics and probability in the near future.

QUESTIONS AND ANSWERS

Referees and others who have read this paper since it was first submitted have raised a number of important questions about applying for research support. Full discussions of these could form the basis for another paper. Here I shall try to "answer" some of these questions with brief statements of fact and personal opinion that I hope will be helpful to young investigators.

Q: Are favorably reviewed proposals in all subfields treated alike by the program director in deciding on final awards, or are some subfields more likely to be funded than others?

A: It is not NSF policy for a program to try to "push" any one topic or subfield in preference to another. One of the advantages of the peer review system is that judgments on the relative importance of various topics come principally from the scientific community rather than being imposed by a government bureaucracy.

As mentioned previously, in making decisions on borderline proposals, program directors do take subject areas into account to try to maintain a balance of support among the various subfields of a discipline. The simultaneous effort to achieve a balance between young and established investigators is, however, probably a far more important (and favorable) factor for you as a young investigator.

Q: Should I propose obviously do-able, routine research over imaginative, unconventional topics which entail potentially insurmountable problems?

A: "Routine" work has almost no chance of being funded within the budgets currently available. It is understood that imaginative, pathbreaking research may entail unforeseeable, even insurmountable, difficulties. Reviewers try to assess the chances that a project will yield important results. If it looks as if difficulties are likely to arise in your proposed work and as if your whole project will collapse when one of them is encountered, reviewers will not be enthusiastic. If your topic is rich enough to accommodate alternate paths and if reviewers believe you have the expertise and flexibility to cope, they will not be so concerned about the problems that may arise.

Q: Is it a good strategy for me to propose research that is already completed, but not yet revealed?

A: The process of preparing an effective proposal inevitably involves deep thought about your research, and probably at least a few promising indications or initial successes. It is a personal decision exactly how much to reveal in the proposal about such progress. My own view is that, as a young researcher, you will improve your chances of funding by fully displaying your accomplishments to date. Give your current proposal your very best shot. If your idea is good enough to get funded, it will have consequences, applications or connections that may lead you to your next proposal.

Q: But what about my chances for future support if I get a grant now and my research does not turn out as planned?
A: Research support from NSF does not involve a contract to deliver a particular set of theorems; it is a grant to support research on a particular topic. Reviewers of your subsequent proposals will be asked specifically to evaluate the results of your prior NSF grants (awarded to you as principal investigator within the past 5 years). In doing so, they will be much more interested in whether you have obtained important results related to the topics of previous awards than whether your work went exactly as planned.

Q: Should I conceal my best ideas—and perhaps risk losing an award—to prevent potential competitors among the reviewers from gaining research advantages?
A: There is some chance that an unethical reviewer would steal an idea from your proposal, rush to implement it and submit the results for publication before you could establish priority. I think this is rare in practice, but I know that reviewers as well as proposers worry about conflicts of interest that arise from competition in research. Several times I have had a reviewer return a proposal unread, because the title or abstract revealed subject matter too close to his or her own work in progress. I know of one case where reviewer and proposer were using related methods to attack different problems and got together to do joint work.

In my experience, the advantages to young researchers of an effective presentation of proposed work outweigh the risks. And, if you are concerned about the risks, there may be ways you can minimize them. Often you can establish your expertise to handle a topic by referring to techniques and results for which you are already known because of talks at meetings or because of published abstracts or papers. Often you can present a convincing research plan by saying what kinds of results you hope for and by giving a general idea of the techniques you plan to use, but without revealing the crucial technical details that would be really helpful to others working in the same area.

Q: If I get turned down, do I have any rights to appeal?
A: If, after discussion of your reviews with the program director, you believe that your proposal did not receive a fair and reasonable evaluation, you may request a **reconsideration**. (See Part III of [1] for details.) The outcome of the reconsideration process hinges on whether the appropriate *administrative procedures* were followed in evaluating your proposal; it does not attempt to second-guess the *scientific judgments* made by the program director. If carried to its final stages, a reconsideration is performed by NSF personnel who are organizationally far removed from the program that declined your proposal.

There are proportionately very few requests for reconsideration and I think the vast majority of them occur because the applicant does not understand how keen the competition for NSF research funds really is. At the possible risk of discouraging an applicant truly wronged by the system from exercising the right to a reconsideration, I would say that (by far) your best chance for support lies with an improved project rather than with a procedural review of a declined proposal.

Q: If I am turned down this year and resubmit a revised proposal on the same topic next year, will it just be evaluated by the same reviewers again?
A: There does not seem to be any general NSF rule on this, but in the Division of Mathematical Sciences (which includes S&P) there is a well-established tradition that, in normal circumstances, at least a majority of the reviewers of the revised proposal you submit next year will be different from the ones used this year. In order to get valid information on whether your revision is actually an improvement, the program director may well use a few carefully chosen repeat reviewers. In my experience, if the new proposal is substantially better, the repeat reviewers are often the most supportive.

CONCLUDING REMARKS

I want to end this paper with a few words of caution, hope and thanks.

95% Confidence

By definition, official documents such as [1] are applicable to all cases. They can be difficult to read because, in order to be universal, they must use specialized terminology and state general principles that apply across the full range of disciplines supported.

Here I have tried to use official jargon sparingly and to be specific about how the general principles apply to statisticians and probabilists. I hope the result is readable and useful, but the disadvantage is that there

will be exceptions to what I have written. There is bound to be someone for whom the best time to submit a proposal is early July, someone who cannot possibly fit his or her project description into the kind of format I have suggested, someone who will feel it unthinkable to mix the pure quest for knowledge with my "strategies" for funding and someone whose time would be better spent proving exciting theorems than uncomfortably giving talks at meetings. I will consider myself fortunate if I have included 95% of the cases, and I will leave it to you to decide, at each point, whether you are included in the 95% or the 5%.

What Is and What Ought to Be

In this paper I have taken the pragmatic approach of describing the funding picture for young investigators as I believe it to be. This should not be taken as a defense of the *status quo*.

Some agencies have recognized the need to encourage promising young researchers. It should be clear from the opportunities I have mentioned that NSF has given considerable attention to this issue. Even so, all too often, I have had to emphasize ways in which young investigators can adapt to programs that are not ideally suited to their needs. The current picture is a patchwork of exceptions and special programs—some of them quite effective, some not. I think it is time to establish a coherent, adequately funded plan for the identification, development and support of outstanding young scientists that starts at the undergraduate level and continues for several years after the doctorate.

Acknowledgments

I wish to thank Ingram Olkin and Nancy Flournoy for many stimulating discussions and helpful suggestions throughout the preparation of this paper. Judith Sunley and Murray Aborn, the editors and referees and other colleagues (unfortunately too numerous to mention individually) offered constructive criticisms and suggested some of the references. Obviously, the opinions expressed are my own, as are any remaining inaccuracies.

This paper was written while I was on sabbatical leave from California State University, Hayward.

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