Postcards from the NSF

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Abstract

We provide an overview of the workings of the National Science Foundation and the proposal review process, as well as some guidance in writing proposals for funding.

1 Introduction

Our purpose in writing this is simple: to increase the efficiency of the allocation of funds by the National Science Foundation (NSF). We hope to do this by improving the research community's understanding of the proposal review process and offering some basic advice that will aid in the writing of proposals. While the internal workings of the NSF are generally quite impressive, proposals for funding are not always well-written and often suffer from some basic flaws. Poorly written proposals waste the time of the principal investigators (PI's in the sequel), reviewers, and NSF staff. In addition, flawed proposals also result in missed opportunities due to poor communication of the true scientific merit of a project. It is clear from serving at NSF and on the NSF review panel that there is a significant number of proposals that are non-competitive primarily due to easily corrected deficiencies. Improving the quality of proposals can lead to a more efficient allocation of funds.

2 Some Background on the NSF

The National Science Foundation is an independent agency of the U.S. Government, originally established by the U.S. Congress under the National Science Foundation Act of 1950. The NSF's expressed purpose is to support, through grants and contracts, fundamental scientific research and programs to strengthen the nation's research potential and education programs at all levels. The NSF is the only federal agency dedicated to the support of education and fundamental research that covers all scientific and engineering disciplines. Any qualified scientist or engineer working in the United States can submit a proposal for the funding of a research project.[1]

NSF activities are organized into eight branches called "directorates." These directorates are: Biological Sciences; Computer and Information Science and Engineering; Education and Human Resources; Engineering; Geosciences; Mathematical and Physical Sciences; Social, Behavioral and Economic Sciences; and Polar Programs. In addition to investments in core research and education, the NSF attempts to identify and support specific

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[1]The NSF particularly encourages women and members of minority groups to apply. For instance, see the discussion of the ADVANCE program below.

emerging opportunities that hold exceptional promise to advance knowledge.[2] This may take the form of an emphasis on a particular directorate, a special initiative within a directorate, or a special initiative that cuts across directorates. For instance, NSF's budget for the 2003 fiscal year emphasizes investments in six interdependent priority areas: Mathematical Sciences; Social, Behavioral, and Economic Sciences[3]; Biocomplexity in the Environment; Information Technology Research; Nanoscale Science and Engineering; and Learning for the 21st Century Workforce.

The SBE directorate is organized into two divisions: the Social and Economic Science (SES) division and the Behavioral and Cognitive Science (BCS) division. Furthermore, within these divisions there are further subdivisions into programs. The two programs that are of the most direct interest to economists are the Economics Program and the Decision Risk Management Science Program; both located in the Social and Economic Science division.

The NSF budget has grown over time. In 1952, the NSF funded only 28 research grants. In contrast, in the year 2000 the Foundation funded more than 10,000 new grants. In year 2001, NSF total budget was $4.537 billion, in 2002 $4.79 billion, while for fiscal year 2003 the budget request has been for $5.04 billion, an increase of roughly 5% over the previous fiscal year. SES's budget in 2001 was about $59.2 million, in year 2002 $59.8 million (just over 1% of the total NSF budget), and of this Economics had a budget of a little over $22 million, while DRMS (Decision Risk and Management Science) approximately $5 million.

In recent years the Economics Program has received about 300 to 400 proposals annually and between a quarter and a third of those are funded. Most proposals to the Economics Program are on the order of one, two or three hundred thousand dollars (generally summer salary for one or more researchers for a period of two or three years plus overhead for the submitting institution); but some are much smaller (for instance for funding of a one-time conference or a specific experiment), and others are much larger

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[2]The NSF budget is decided by the U.S. Congress on a yearly basis and Congress oversees the NSF operations. Some of the budget is specifically targeted at areas that the Congress and/or NSF administration feel are particularly timely and important.

[3]SBE has recently been designated by Congress a priority area. An increase in funding is expected in the next years devoted to explore the rapid changes in the nature of human activities, and the effects of new technologies on human activities.

(for the support of a large data center or long-run program involving many researchers).

3   What Happens to an NSF Proposal?

Before offering some basic advice regarding the writing of a proposal, let us describe the process through which proposals are evaluated. Having a good understanding of this process will aid in the writing of the proposal, as it helps in understanding what needs to be communicated and to whom it needs to be communicated.

The Participants in the Review Process

Proposals are generally seen by three sets of people: outside reviewers; a panel of researchers who meet with the NSF staff to process the reviews and sort through the different proposals to establish priorities for funding; and the NSF economics program officers who serve as the ultimate interface between the reviewers and the formal funding apparatus at the NSF. Let us describe these groups in a bit more detail.

The primary input into the proposal review process are the reviews themselves. Each proposal is sent to six to eight reviewers, two of whom are always from the panel (whose composition is discussed in more detail below), with the remaining being "outside" reviewers. These outside reviewers are generally other researchers who are familiar with the topic and area of research. The outside reviewers are selected by the program officers in part from a suggested list submitted along with the proposal by the PI. While the NSF staff have a wide knowledge of potential reviewers, the list of reviewers submitted with a proposal is still a primary source of information and it is strongly in the best interest of the PI to submit a long and accurate list of potential reviewers.[4] Generally, reviewers who are well-qualified and confident in assessing a proposal are more likely to give it the high marks necessary to receive funding than a less qualified reviewer who would rarely have enough confidence in their assessment to strongly recommend funding.

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[4]Of course, reviewers with a conflict of interest (recent co-authors, students of the PI, the PI's thesis advisor, current colleagues of the PI, relatives,...) cannot be used, and so one cannot "stack the deck" in providing this list.

The proposals are then considered by a panel of active researchers. The panelists are generally university professors who have experience with NSF funding and are active researchers publishing peer-reviewed articles. The fourteen or so panelists are selected so that their collective expertise covers the wide variety of research areas covered in submitted proposals. The panel is also diverse in terms of the institutions represented, as well as ethnicity and gender.[5] Panelists each serve a two year term (so four rounds of proposal evaluations and meetings), are paid a modest honorarium[6]. Each panelist reviews about 25 to 40 proposals each six months and then participates in the discussion when the panel convenes to provide advice to program officers in rating and ranking the proposals.

All proposals are handled from the moment of their submission to the final decision by a "program officer." Economics, being a large program within SES, has three program officers: one permanent and two rotators. The two rotators are usually university professors who have an active research background and who serve at the NSF for one or two years (usually two years). After submission, all proposals are divided among the program officers according to their personal research background. Each program officer is then responsible for identifying and assigning reviewers to each proposal, guiding the Panel's discussion and ultimately recommending and processing the award or decline.

The Review Process and Decisions on Funding

When a proposal is received by the NSF[7], it is sent out to four to six outside reviewers. As mentioned above, two additional reviews are from the panel. The two panelists always complete their reviews, while the return from outside reviewers is more random. Each proposal must at list receive

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[5]There is a restriction that no more than half of the panelists may have an NBER affiliation, and also an attempt to make sure that any given university or research institution has no more than one panelist at time. Panelists are required to leave the room during any discussion concerning a proposal from their own institution or by a co-author, student, or graduate advisor.

[6]This is on the order of two or three thousand dollars a year, but this must also cover a panelist's travel and hotel. Essentially, panelists serve as a public service.

[7]All submission are now through "Fast Lane" – the NSF based web site. Submissions are entirely electronic, as are the reviews. There are two submission deadlines per year (in economics: currently January and August 15-th) and two corresponding reviews and decisions several months later.

an absolute minimum of three reviews and on average each gets roughly six reviews. A good review assesses the potential contribution of the proposed project, describes its strengths and weaknesses, and may provide constructive advice on how to improve the research.[8] If the project is not competitive for funding, then the review should specify why the proposal doesn't measure up. Each review has a bottom line score Excellent/Very good/Good/Fair/Poor based on the two NSF criteria of scientific merit and broader impact, which we discuss in more detail below.

Once the reviews come back from the outside reviewers the panelists have a chance to read the reviews on each proposal.[9] The panelists then convene at the NSF headquarters for a two-day meeting[10] to discuss the proposals.

At these meetings, the proposals are discussed one by one. Most of the proposals are handled by sub-panels composed by five to nine panel members.[11] The two panel members who reviewed a given proposal lead the discussion by describing the proposed research and its merits and deficiencies. The panelists give their own opinions, and also report on the opinions of the outside reviewers. Other panelists (not assigned to the particular proposal in question) may also enter the discussion. Through this discussion, the panel tries to reach a consensus on a score for each proposal. Roughly, these scores are possible funding recommendations: "Must Fund," "Should Fund," "Could Fund," and "Decline." Plusses and minuses are added to the "Should" and "Could" categories so that the proposals end up sorted into eight categories. To get an idea, "Must Funds" are proposals that are clearly

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[8]A review of a research proposal is a bit different from a referee's report on a completed paper. Both types of reviews need to address the importance of the contribution relative to the literature, general coherence of the approach, and the appropriateness of the methodology; but a proposal review is necessarily more focused on the bigger picture and potential success of a project while a referee's report needs to go a bit further to pay attention to the minutiae of the research.

[9]Each panelist is asked to write his or her own reviews on the 30 or so proposals assigned to him or her before reading those of the other reviewers.

[10]This happens twice a year, usually April and November, about three months after the corresponding submission deadlines.

[11]The sub-panels are set up to get the best match of the proposals with the expertise of each sub-panel members. The recommendations of the sub-panels are debated and finalized by the full panel at the end of the meeting. There are usually around 150 to 200 proposals per cycle. Operating in sub-panels allows each proposal to get a serious discussion and thorough evaluation. When the reviews are largely in agreement, the discussion will tend to be shorter, while disagreement leads to longer discussions.

outstanding, and in contrast "Declines" are proposals with some obvious deficiency. "Coulds" are proposals that do not have any obvious deficiency, and so could be funded, but seem less exciting or promising than other proposals. The "Shoulds" are ones that show substantial promise, but are perhaps not quite as obviously outstanding as the "Musts." The NSF program officer in charge of a given proposal offers direction in the discussion of the proposal and at times reminds the panelists of important considerations. He or she also takes notes on the discussion of the proposal which are to be included in the "Panel Summary," a document which is ultimately provided to the PI together with the reviews.

Immediately after the advisory panel meeting, the program officers meet to survey all the proposals, consider the funding priorities suggested by the panel, the external reviews and the amount of available funds. All this determines how far down the list of categories funding is eventually provided. The final funding decision reached by program officers generally tracks the ranking suggested by the panel, except for some additional information that program officers may consider. There may be a boost for young investigators, institutions that have not been funded in the past, minorities, or proposals that might be co-funded by some other program at NSF.[12] In the past few years, the funding has roughly cut off near the Should minus/Could plus border. Program officers are ultimately responsible for funding recommendations. The declines are processed first, then the Must Funds are awarded. The less clear decisions on the Shoulds and Coulds are then sorted through, sometimes in negotiations with other programs. PIs are finally notified through Fast Lane of the decision, and given a web address where they can retrieve the reviews on the proposal and the Panel summary.

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[12]Proposals can be helped by being reviewed by more than one program. In the past, the most frequent joint reviews together with the Economics Program have been Sociology, Political Science, Law and Social Science, Geography, DRMS (Decision Risk and Management Science), and Methodology Measurement and Statistics programs. PIs should consider submitting their proposal to more than one program, if appropriate, as this can increase the probability of success of a proposal.

4   On What Basis are Proposals Evaluated?

The NSF's major considerations in assessing proposals are two criteria: 1)intellectual merit of the proposal and 2) its potential broader impacts. These considerations are different in how they matter in evaluating a proposal.

Having a broad impact is neither necessary nor sufficient to receive funding, but is certainly a good thing and a broader potential impact leads to a higher rating of a proposal, all else held equal.[13] Having strong intellectual merit is, however, a necessary condition for a proposal to be funded, even though not sufficient on its own, as funding will also depend on broader impacts and more generally how a proposal compares to other submitted proposals.

Let us discuss these two criteria by first addressing the very important consideration of a proposal's intellectual merit. A proposal's intellectual merit is assessed through the following four points:

- Does it address an important issue?

- Does it propose to answer questions that push knowledge significantly beyond the existing literature?

- Does it propose to use methods that are appropriate for answering these questions?

- Does it have some likelihood of success based on the training or past record of the PI?

Failing on any of these points is reason enough for a proposal to fall into the "decline" category described above and to fail to be funded.

Let us discuss each of the above points in turn.

The most basic consideration in intellectual merit is why the questions to be investigated are important. This may seem self-evident to the PI, but need to be carefully communicated nonetheless. The importance might be

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[13]As of 2002, the specification of the broader impacts of a project by every submitted proposal is required by the NSF. That is, all proposals must clearly describe the broader impacts of the research, otherwise the proposal will not be considered eligible. Some proposals are very foundational in their focus and so may not have very broad immediate impacts. This does not preclude funding, but does place a higher hurdle on intellectual merit.

due to new advances in modeling or theoretical knowledge, a development of a new methodology, or it could be an advance in the understanding of some observed phenomenon, or the testing of existing models. In each of these cases the proposal needs to communicate why the investigation is likely to lead to knowledge that should be considered important and will help advance the science. This should be addressed directly and clearly. We should mention that it is critical that the project should be basic science and not be "consulting-like" work. A question could be of obvious practical importance, but not be appropriate for NSF funding if it will not advance scientific knowledge, but is instead simply an application of pre-existing knowledge.

Of course, addressing an important issue is not enough. The project also needs to add significantly relative to previous research in the area. [14] Assessing the contribution of a project may take different forms for different types of projects. For instance, if the research is a positive analysis of some institution, then we might ask what new understanding of the workings of this institution will we obtain. What will this new understanding be useful for? If the work is instead some sort of foundational theory or econometrics, then one might ask how will the new techniques, theory, or methodology be used? How will it aid other researchers in advancing knowledge? If the project involves creation of a new data set one can ask what new kinds of questions can be examined using the new data? How well designed is the data set and how accessible will it be?

Let us now suppose that a project passes the first two hurdles: i.e., that it addresses an important topic and proposes to answer questions that will lead to a significant advance in our understanding in the area. The next level of assessment looks at how well thought out the project is in terms of how it approaches the specific questions being asked. In particular, how appropriate is the methodology proposed for answering the questions at hand? If it is a proposal to develop new theory, then how appropriate and robust is the

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[14]Something that goes hand in hand with assessing the contribution to the literature is also seeing how well aware of the literature the PI is. If the PI is unaware of research that speaks directly or sometimes indirectly to the question at hand, then one is less confident that a new understanding will emerge from the project. This can often be a stumbling block for inter- disciplinary work, especially proposals that end up being seen by more than one program at the NSF. The wider the coverage of a project, the more difficult it is to be on top of all of the relevant literatures. This is generally understood by the reviewers and panelists, but it is still important that the project not re-invent the wheel.

modeling? Does it capture the essential features of what is being modeled. If it is foundational theory, then how versatile and applicable will it be? If it is an empirical testing of a model or hypothesis, then how appropriate is the data set? Are appropriate statistical techniques being used and have the possible endogenous relationships been carefully thought through? How powerful will the test be? Will the conclusions be interpretable beyond the specific data at hand?

The final piece of the intellectual merit puzzle involves assessing the likelihood of success of the project. To some extent this involves the track record of the investigator(s). Do they have the appropriate skills and knowledge to carry the project to its conclusion? How likely is this project to lead to articles in top journals or the best field journals? If the PI's have received past funding, how successful were their past projects?

Next, let us turn to discussing how the broader impacts of the proposed research might be assessed. This is essentially looking at where the proposed research might have an impact beyond the immediate contributions to the field. This includes the question of how the proposed research is applicable to society and its problems and realities, or how the project might promote teaching, training, and learning. Will it have some pedagogical value? Will the proposed project broaden the participation of underrepresented groups (e.g., gender, ethnicity, disability, geographic etc.)? To what extent will it enhance the infrastructure for research and education, such as facilities, instrumentation, networks, and partnerships? Will the results be disseminated broadly to enhance scientific and technological understanding? Again, NSF requires now that all proposals address this component of the research.

Finally, let us also mention that there is some attention paid to the budget by the panelists (and sometimes also by outside reviewers). This is mostly along the lines of seeing whether the budget seems to be in the right ballpark, or is asking for some things that seem unnecessary or unreasonable. For very large proposals, there is some added concern about overall importance. This may not be directly reflected in the scores that a proposal gets, but will often be communicated to the NSF program officers during the panel meeting. This may not end up affecting whether a proposal is funded or not, but might end up in a revision to the budget if the project is funded. The bottom line, in fact, is that almost all budgets are revised, mostly to comply with the norms that the Economics Program decides every year. In general,

when preparing a budget the investigator should ask for what is reasonably necessary to develop the research project. In any case the budget should not be the most time consuming item in the proposal preparation phase.

Disagreements in the Proposal Review Process

Given the number of reviewers who assess a proposal (including outsiders and panelists) and the potential variance in their backgrounds, it is not unusual to reach differences in opinions. So what happens when the reviews are in some disagreement? Often there is a simple and clear reason for this: one reviewer gives a high rating based on the reputation of the PI and basic interest of the problem, while another gives a low rating based on some deficiencies in the particular project itself. Essentially, the positive review may not have addressed the full list of criteria listed above for assessing intellectual merit. This type of proposal is likely to fail if the deficiencies appear to be genuine. Another possible conflict is simply in disagreement about the basic importance of the issues, or in assessing one of the other criteria. Such disagreements between reviews lead to the longest panel discussions, generally with a real interest in reaching the right consensus. If ultimately the panel does not reach a consensus, which however happens quite rarely, the program officer decides.

5   Writing a Proposal

Now that we have a good idea of the proposal review and evaluation process, we can discuss some basic points about writing a proposal.

First, there is the choice of a project to propose. The right maturity of a project is an important consideration. The PI must be far enough along to provide a healthy view of the methods and questions, but not so far along that the project is already largely completed. Also, a project will not meet the significant advance in understanding criterion if it is largely extensions and/or generalizations of recent work, even if that recent work made important advances. In short, looking closely at the four points above will help in choosing a project which has the right potential for a successful proposal.

Assuming a good project has been identified, the writing of the proposal should largely be targeted at answering to the evaluation points described above: What is the big picture and why is this an important topic? What

will the likely contribution from this proposal be? Why will this be of interest and/or useful? What are the specific questions to be addressed in the proposal, and why are these of interest relative to the broader goals? What are the techniques, data sources, models, and preliminary results?

In answering these questions there are several things to keep in mind.

- Quality of writing matters a lot as a proposal must clearly and concisely answer to each of the points discussed above.

- The summary statement and introduction should clearly outline what the topic is, why it is important, what questions the project will address, and what type of methodology it will employ.

- The proposal should be targeted at a reasonably general audience of researchers beyond those in the narrow area, and not be full of jargon. In particular, the panelists assigned to any given proposal are likely to be in the general area of research in question, but perhaps not so familiar with the details of the specific area of the project.[15]

- The proposal should correctly put the project in perspective relative to the previous literature, but at the same time not read like a literature survey. Discussion of related literature should focus on what the value added of this project will be and what is missing from the previous literature. While this necessarily includes some discussion of the accomplishments of the previous literature, it should be limited to those pertinent to the project in question and not be a long discourse.[16]

- The discussion of methodology needs to be sufficiently detailed so that the reviewers can understand how this will be approached, but the proposal should not have the same level of derivation that would appear in a research paper. You want to convince the reviewers that you know what you are doing, but you do not want to exhaust their patience.

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[15]It is not so useful to try guessing who on the panel will be assigned to any given proposal. Beyond strict adherence to anonymity, the sheer number of proposals to be reviewed means that proposals are necessarily spread out pretty broadly.

[16]The proposal includes a bibliography (as opposed to a reference list), which is a place where the PI can list related works which are not directly relevant to the project or even discussed in the proposal.

- It helps to provide some preview of the types of new insights that might be gained, such as a preliminary result or conjecture, or a detailed statement of the hypotheses to be examined. This helps clarify the specific questions to be addressed and also ties it together with the methodology to be used.

- The proposal should discuss the major issues and obstacles for the project to overcome in the future.

- Including references to related research previously funded by NSF is generally useful.

- Finally, the proposal must specifically address the two NSF review criteria - intellectual merits and broader impacts – in separate statements in the Project Summary.[17] In particular, the broader impacts resulting from the proposed project must also be addressed in the project description as an integral part of the narrative. That is, the PI must consider how the research will benefit society, promote teaching and learning, outreach to minorities, or enhance society's infrastructures in research and education.

6 Some Common Blunders and Easily Avoided Mistakes

In closing, let us just mention some mistakes that appear often enough to warrant specific mention, especially as they are easily avoided.

- "I am famous and so don't need to be careful in writing a proposal." Funding is not based simply on the PI's past record. Reviewers and panelists take the evaluation of the research project seriously and the likelihood of success is only one of the criteria that the proposal needs to meet. Don't count solely on your track record!

- "Where is the beef?" Some proposals describe in great detail past research but give only few sketchy hints about the current proposed research. Such proposals are also not well received, as it is important that

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[17]The NSF will return (without review) proposals that do not address the two review criteria.

reviewers be able to confidently say that the newly proposed research is well-founded.

- Multiple PIs staple together different pieces and produce an incoherent proposal. Take the time to make sure that the project has a coherent purpose and theme and that the proposal is carefully crafted.

- Proposals that are too brief. Such proposals give an idea of the big picture, but give no idea of what will actually be done or how likely it is to be successful, or what specific new knowledge is to be gained.

- Proposals that are too detailed. These are the counterpart of the too brief proposals. These proposals go into minutiae and long derivations, but often forget to give the overall motivation and a clear picture of what is to be done and why.

- Incoherent proposals that are full of jargon. These assume that reviewers know some very specific results in the literature, and many assume that reviewers will know previous results of the PI's, or that reviewers will download papers/data sets, etc., or read some long appendix. Panelists review 20 to 40 proposals each six months and simply do not have such time. Also, as mentioned above, results that are known to the PI may not be known to the range of reviewers that will see and evaluate the proposal.

- Proposals that are overly long using small font and playing with margins to squeeze too much in. Quality is generally better than quantity when it comes to proposals. Overly long and dense proposals are frustrating for reviewers and especially panelists, given the enormous volume of proposals that must be covered. This is not to say that short proposals are good, but one should always keep in mind whether or not some specific detail is really helping in clarifying the intellectual merit of a project.

- "Trust me" proposals. These make grand claims about new innovations and yet are not precise enough so to indicate whether anything will really be learned.

7   Special Initiatives and other Special Categories

As mentioned earlier in addition to the standard funding areas, there are also special initiatives that may have temporary (or in some cases permanent) funding. The SES homepage has a list of special funding opportunities, and additional programs are listed under the Cross-Directorate activities link. These initiatives are more focused on specific topics than a program (such as the Economics Program) would be, but also more interdisciplinary in nature. While usually housed under the guidance of some particular division and program, proposals to special initiatives are often reviewed by reviewers and panelists in several programs and funding might eventually come from a variety of sources.

If a project is specifically well matched to a special initiative, then it should be directed to that initiative rather than an open program. While special initiatives often provide a higher chance of success for proposals since money is specifically targeted at a narrower area, they also lead to some additional challenges in writing a proposal. The additional challenges usually stem from the fact that the proposal may be reviewed by researchers from different disciplines. This requires that the PI(s) be aware of a broader set of literatures and write a proposal with special care to being jargon-free and not assuming that reviewers will know (or agree with) approaches or methodologies that might be thought of as standard in the PI's own discipline.

There are currently two regular competitions that cut across the entire NSF: the ADVANCE Program and the EPSCoR Program. The ADVANCE Program is aimed at increasing the participation of and advancing women in academic and engineering careers. The ADVANCE program offers funding to individuals (both women and men) and institutions. It is geared to support new approaches to improve the climate for women in academic institutions and to facilitate their advancement. The EPSCoR Program (Experimental Program to Stimulate Competitive Research) is aimed at increasing the geographical diversity of research awards. EPSCoR states are traditionally characterized by low NSF funding. Proposals coming from an EPSCoR state are reviewed exactly as the other proposals. However, when funding decisions are made, these proposals have an advantage since they may be partially funded from a special pot of EPSCoR (non program) money.

In general, special initiatives and special programs may change over time. For instance, the ADVANCE program replaced the POWRE competition. The ultimate advice is, therefore, to rely on the NSF web page for an update on current programs and initiatives.

Another special category of proposals are the "CAREER proposals." These are reviewed once a year and are intended to make relatively large awards to young (untenured) outstanding scholars for a four to five year research agenda and an associated innovative educational plan. These are for scholars of particular note whose research agenda is well above the average in terms of quality and likelihood of success.[18] CAREER award proposals are a bit different from a standard proposal as the time horizon is longer and an associated educational plan is necessary.

Finally, there are other sorts of projects that demand differences in proposal writing from the basic overview we presented above. For instance, NSF provides funding for conferences, research centers and data sets. These will not be as narrowly defined as a particular research project, but will require more description of other aspects, such as the researchers involved and how this might contribute to knowledge more generally.[19] Another special category is "accomplishment based renewals." These are for renewed funding for an existing project that has been particularly successful and holds additional promise for future research. These tend to be shorter proposals, mainly based on the merit of the work to date and some clear idea that there are important related issues still be to explored.

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[18]Awardees of a CAREER proposal are then eligible for the Presidential Early Career Awards for Scientists and Engineers (PECASE). Candidates are selected from among the most meritorious new CAREER awardees. The PECASE program recognizes outstanding scientists and engineers who, early in their careers, show exceptional potential for leadership at the frontiers of knowledge.

[19]For example, a proposal for the creation or maintenance of a data set requires much more description of questions such as: the precise nature of the data to be collected, why it is of value (what new information or hypothesis tests it will provide), how it is comprehensive enough to be useful in further research, and how it will be made available to researchers.

8   A Closing Remark

A common question that we have come across in talking to researchers who are thinking about submitting a proposal is whether or not a researcher has a disadvantage if he or she is employed at a department that is not ranked in the top twenty in the nation. What is often underlying this question is whether the review process is biased in its evaluation of proposals. While a quick look at the awards suggests that top departments have a higher than proportional percentage of their researchers funded, the funding rates *as a percentage of proposals submitted* are more equal across submitting institutions. That is, lower ranked departments submit fewer proposals and have fewer awards, which often leads to a similar success rate. Our experience suggests that the review process is a very careful one and that the panelists and reviewers are quite conscientious in providing fair evaluations of proposals.

Of course, as economists we understand that proposal submission is an endogenous process, and that there are a wide variety of differences across institutions which will ultimately affect decisions of a researcher to submit proposals.[20][21]

Given the limited funds, the competition among proposals is strong and many proposals from well-known researchers at the very best departments are rejected. The first step to getting a grant, regardless of a researcher's affiliation, is to apply.

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[20]The fact that an institution might have fewer researchers who are experienced in applying for and receiving NSF funding can end up making it more difficult for first-time proposers to get good advice in writing a proposal. Institutions also differ in the number of active researchers that they have and the degree to which their administration is experienced in handling research grants. Part of our motivation in writing this article is to help mitigate such hurdles for a researcher.

[21]There are other issues that we will not attempt to dissect here, such as the fact that evaluation of institutions is partly by research funding, or that top departments might look to hire researchers who are most likely to be successful in obtaining funding.