perusal of the list of principal investigators funded by the NSF Statistics and Probability Program (see Trumbo's reference to NSF publication [3]). Search for investigators within 6 years of receiving their Ph.D. There aren't very many! What information is available to describe the population to which they belong? How many faculty in statistics are within 6 years of their degree? What are their credentials? Do we witness a disruptive musical chairs at 6 years following the receipt of Ph.D., or is the process working effectively?

The second group being depressed by a lack of support are those whose research interests derive from the problems of other sciences, rather than from problems intrinsic to statistics or from statistical problems that are interdisciplinary with subfields of mathematics. Balanced opportunities for cross-disciplinary research by young and by senior scientists alike is a community responsibility. When money becomes tight, a natural response is to become territorial. It occurs when there is a redistribution of funds from senior to junior investigators and it occurs when scientific thrusts do not fit within existing program boundaries at NSF. To turn inward, as individuals or as a discipline, is counterproductive. It is time to nurture our own, channel our frustrations and project our destiny as we wish it to be.

I want to thank Professor Trumbo again for adding information to the literature on the statistical community and the factors affecting our growth and development. May we use them to good advantage.

ADDITIONAL REFERENCES

BAILAR, J. C. and SMITH E. M. (1986). Progress against cancer? *New England J. Med.* **314** 1226–1232.

OLKIN, I. and SACKS, J., eds. (1988). *Cross-disciplinary Research in the Statistical Sciences.* IMS, Hayward, Calif.

Comment

Yashaswini Mittal

Writing proposals and receiving federal funding for research is a long and arduous process even for the most experienced and certainly for the novice. The informational booklets published by all the funding agencies are most often not sufficient to allow the reader to grasp the crucial points being made. Thus the above article will be very useful to the beginners as well as the more experienced scientist.

In this discussion I only want to reiterate some points of the article for stress and provide some latest information on the program as supporting documentation. Similar to the author, I am a past program director (though a more recent one) at the National Science Foundation (NSF), and base my comments on that experience, but they are not official in any capacity.

A program director, who sees numerous proposals and reviews each year, gains a unique vantage point which, if shared by all investigators, could be very valuable. How to share this point of view with the investigators themselves without compromising the confidentiality of the program and the peer review system is not an easy problem. Articles such as this are attempts toward a solution. I also feel that more and better data from the program will provide crucial help as well. I have collected some data that I thought to be pertinent, from my part of the Statistics and Probability (S&P) program at NSF in the 1988 fiscal year (FY88). The following discussion will refer to parts of it in places. It should be considered preliminary at best because the size of the data is too small at the moment. However, if such data collection is continued, it will show some interesting insights and trends over the years. The proliferation of electronic hardware and software at the foundation is relatively new. I am confident that in the future, these added tools will bring drastic improvements in collection and reporting of data for all programs at NSF.

Even though a really good research idea is the crucial ingredient of a research proposal, other aspects are important as well. To be precise, a research proposal proposes concrete problems of current relevance and utility, makes a case that this can be done successfully by the proposer in the proposed duration and realistically estimates the cost of doing this research. Each aspect of this description is important, as is indicated by the four stated criteria for selection of a proposal, not just one. The best problem or a set of

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problems in the world will do little in terms of gaining support, unless the proposal makes it credible that the proposer will be able to achieve this *in the proposed time*. I emphasize the last letters because inexperienced investigators tend to write proposals that are too long. A program director (PD) will generally overlook the rule specifying the maximum length of a proposal, but the reviewers will certainly question the overkill. In general, any strategy that makes the reviewer (and the PD) spend inordinate amounts of time on the proposal is overall counterproductive. Our academic system offers little reward for an experienced reviewer beyond the satisfaction of serving the science and its community. This necessarily limits the amount of time a reviewer can spend on any given proposal. A carefully crafted succinct and lucid proposal indicates the commitment and the dedication of the proposer. Such proposals are much harder to write than a long sprawling document of free associating thoughts. The text of the proposal and the supporting documentation should be provided by keeping this perspective in mind.

Some other unequivocal advice I can offer will be in the category of what can and should be expected from your friendly PD. A PD can explain rules, give information and offer advice. Only the first two parts of it are done in an official capacity. Advice is offered as the best judgment of the PD as a person and it should be taken as such. Many situations offer a difficult set of alternatives and the decision in the end has to be the responsibility of the investigator. I wholeheartedly agree with the advice of the article that there are distinct limits to what can be expected of a PD because the Division of Mathematical Sciences has among the highest loads per program director within the Foundation. Please use the electronic mail whenever possible. Also, it is more efficient to have many questions of procedure answered by the program assistant, who is fully (sometimes better than the PD) qualified to answer them.

In support of the section "If At First You Don't Succeed ... ", Table 1 splits the 129 funded and 60 declined investigators in FY88 according to how many times they were funded and declined previously. I gathered this data to get some idea of the discouragement factor in the community. This data indicates some interesting conclusions. 1) Declination from NSF is not that uncommon even among the funded investigators and it most certainly is not the "kiss of death" that some young investigators fear (also see the last table). 2) Disproportionately many investigators give up applying after just one or two tries. 3) Being funded several times before is no guarantee of continued support from the program nor is being declined a "fait accompli" for those previously declined.

Assessing the reviewers comments and associating them with the ratings is an extremely difficult task for an investigator. This comes mostly from the lack of a large enough data set that allows comparisons. Experienced reviewers and investigators accumulate some standards and comparisons over a number of years. Even this is shaky at best because the structure and the content of the program as well as the reviewer community keeps evolving over the years. The data in Table 2 on reviewer ratings may show some standards and trends over the years and provide better coordinates for the investigator.

A total of 460 review ratings in FY88 were distributed over the various categories, where E = excellent, VG = very good and G = good. Note that the evaluation sheet does not include boxes for the "half" ratings such as E/VG, but a significant number of the reviewers use them. Taking away the "edge effect" (viz., allocating half of the half-cell-entry to the adjoining entries), the ratings are distributed approximately 30, 40 and 30% in the broad categories of E, VG and G and below. The 70% ratings of VG or above may provide some perspective to investigators in evaluating the ratings of their own proposals. It also sheds some light on the misconception (in my opinion) of

TABLE 2
Proposal ratings

	E	E/VG	VG	VG/G	G	All other
No. of reviews	107	40	152	42	79	40
Percent	23.3	8.7	33	9.1	17.2	8.7

TABLE 1
Funding history

	0	1	2	3	4	5	6	7	8	9+
Funded										
Funded investigators	18	12	15	14	8	12	4	3	2	41
Declined investigators	38	7	5	0	3	0	2	0	0	5
Declined										
Funded investigators	69	27	15	7	8	0	3	0	0	0
Declined investigators	29	10	6	6	5	2	1	0	0	1

TABLE 3
Investigator salaries

Ph.D. age	No.	Funded salary range Min	Med	Max	No.	Declined salary range Min	Med	Max
yr								
0–5	22	26,379	33,218	48,672	14	25,524	33,253	47,997
6–11	27	30,740	41,000	81,000	18	29,502	35,599	56,000
12–17	24	35,010	52,476	75,000	9	36,500	49,685	68,562
18+	56	37,800	65,106	101,322	16	44,000	59,800	77,963

some that the probability and statistics community rates each other harshly.

The hardest philosophical question one faces is "Is it worth the trouble?". I wholeheartedly endorse the thoughts and advice of the author on this question. Table 3 gives the academic year salaries of the funded and declined investigators and shows surprisingly little difference. Maybe what it indicates is that (in terms of salaries) it does not really matter whether the individuals get funded or not but that they "play the game." (I feel, although I have not verified it, that the salary levels indicated in both the tables are significantly larger than the national average.)

One of the constructive ways in which a young investigator may want to enter the "system" is by acting as a reviewer. Unfortunately, young people have a significantly smaller chance of being selected as a reviewer because of the lack of a "track record." In the 2 years that I spent at the Foundation, I paid particular attention to this problem. For example, I tried to assign at least one unknown (to the NSF reviewer system) or relatively unknown reviewer to each proposal that I processed. I introduced over 100 new reviewers to the system in FY87 and 51 in FY88. Unfortunately, only 38 out of the 58 copies of the proposals sent to these 51 new reviewers prompted any response from them. The "no answer" rate (34.5%) among these new reviewers was almost twice as high as the overall rate of "no answer."

I must mention a couple of comments from the article that made me somewhat uncomfortable. Encouraging the investigators to find out "how near the borderline your proposal was rated" may elevate the expectations of the investigator beyond what can be normally fulfilled by a PD. The PD will usually make comments about the "fundability" of the proposal (viz., expressing an opinion about whether the proposal would have been funded if additional funds were available), but it would be extremely rare indeed that a PD will make statements of the kind "Your proposal was sixth from the cutoff line . . " A PD will not only choose, but is required, to stay away from any direct comparisons between proposals. Besides they will be of no constructive value to the investigator. The most useful input from the PD will be in interpreting the reviewers' comments and getting some advice for improving the proposal. A good time to initiate such discussion is in the summer months when the PD is through the crucial work of funding decisions. It is certainly worthwhile to get to know your PD. However, that should not prompt unrealistic expectations about the input from the PD about a specific proposal. Thus, for example, a PD cannot be expected to miss a proposal that the investigator submitted after discussions with the PD but which unfortunately never reached the program.

In closing I once again want to express my strong support for the sound advice offered in the article and thank the author for having taken time to write it.