Commentary

Another Opinion

A "Rotator Crisis" at NSF

The Division of Mathematical Sciences (DMS) at the National Science Foundation handles grant proposals and program leadership in mathematics by using a good many temporary program directors, or "rotators", recruited for one- or two-year terms from the universities. According to Don Lewis, DMS director for the past three years, these rotators are critical to the health of the Division because they bring to Washington the perspective and fresh thinking of the active researcher. They are also in short supply, and with Lewis nearing the end of his term, the Division is experiencing something of a "rotator crisis".

In recent years the presence of active researchers as DMS program directors has become more important. The NSF budgeting process now begins with a thought exercise of imagining cutbacks of 20 percent; directors are then asked how they would build that amount back. Naturally, they would like to do so by funding the most exciting new ideas. The involvement of rotators increases the chances that this will happen.

Typically, rotators manage research programs that include, but aren't confined to, their specialty. Of current DMS program directors, thirteen are rotators and eight are permanent staff. Next year, with the routine ending of some two-year rotator terms and the departure of several long-time permanent staff, DMS will need at least one new director in every program and would like to increase the permanent staff (many of whom are former rotators). To fill each position, DMS finds that it has to contact about fifty people—a substantial effort.

Are the DMS rotators worth this effort? I would argue that the answer is yes. Permanent staffers are, of course, essential. These are the people who provide continuity, understand NSF structure, contribute to NSF's scientific and intellectual mission, and see new ideas through. Rotators, however, are equally essential for their perspective on where the discipline is going. These are NSF's eyes and ears into the research community. Only when program directors are intellectually involved in mathematics does NSF have the insight it needs to pursue the most promising directions.

There is no doubt that the rotator's job is demanding. DMS receives about 2,000 proposals a year and supports about 60 percent of the nation's mathematics research at universities. Each program director handles about one hundred proposals a year—choosing reviewers, getting proposals out for review, deciding on awards, documenting decisions, and writing well-documented (and diplomatic) refusal statements.

This heavy workload is one reason it is difficult to recruit top-caliber rotators, but the commonest reason is that people don't want to interrupt their research at what may be the height of their careers. Most rotators find some time for their own work (they get one day off a week for research, which some save to use in the summer), but they need determination to sustain research momentum.

Even so, most rotators find that their time in Washington is immensely valuable. For some, the greatest benefit is to their own research, which is often enriched by reading proposals and working on new initiatives. Others find rewards in broadening their understanding of their field and helping to define the future of mathematics. Still others find new professional contacts and career opportunities; learning how to evaluate programs outside one's specialty and how to secure funding is excellent preparation for a chairship or other senior position. And no one takes a pay cut to come to NSF; after cost-of-living grants, some people actually experience salary increases.

For some, the broader rewards are equally meaningful. They gain a new view of how the mathematics community is viewed in Washington and a chance to correct misperceptions. They also learn empathy for the people submitting proposals and find chances to build bridges between the research community and a bureaucracy of daunting size. We all hear complaints about the handling of grants, the size of awards, and so on; it is the rotators, with roots in the research community, who can best explain the system from the inside and press for changes where they're needed.

Most of all, it is necessary for the good of the discipline that knowledgeable individuals active in research help manage DMS funding. If the best among us step forward, the Division can do a better job in both allocating grant money and securing it through the budget process. For example, NSF selected mathematics as one of its budget priorities for the FY1999. This was a direct response to a presentation by rotators and other program officers who clearly demonstrated the exciting and deep research being carried on by U.S. mathematicians.

In the last three years, DMS has recruited top-notch rotators, and the satisfaction of the community has increased. There is more confidence that proposals are receiving thoughtful and informed reviews. Don Lewis has been creative and forceful in getting ideas from the mathematics community to the budget table. There could be no better legacy for his time at DMS than a stronger involvement by the research community in supporting and volunteering for a task that can strengthen the mathematical enterprise as a whole.

—Phillip A. Griffiths

Phillip A. Griffiths is professor of mathematics and director of the Institute for Advanced Study in Princeton, NJ.

Letters to the Editor

NSF Funding Not the "Most Promising Investment"

I'd like to take issue with the "Testimony on Behalf of the JPBM" (September 1998 *Notices*), specifically the following paragraph:

> We believe that what mathematics, science, and engineering represent is a top priority for investment in the future of our country. However, the NSF budget has seen no real growth since FY 1995 and was part of the almost threefold decline in R&D budget as an overall percent of GDP over the past thirty years. Thus we have been shortchanging the most promising investment in our country's future. Moreover, given the extraordinary importance of the NSF's mission, we believe the need for a full appropriation transcends any particular approach to budgeting.

I won't argue with the first sentence. However, is real growth since 1995 necessary? If the 1995 NSF budget was a reasonable one, why should we expect subsequent NSF budgets to do more than keep pace with inflation? Recall also that the U.S. government has made great efforts over the last several years to balance its budget. Given the cuts that have been made in other parts of the budget (e.g., welfare reform, which dramatically affected a population much more vulnerable than mathematicians), is it proper to argue that the NSF should be exempt? As to the comparison with the situation thirty years ago, that was during the height of the cold war. While the NSF is one of my favorite relics of the cold war, I do think that the cold war skewed government spending in ways that are no longer appropriate (if they ever were); thus, a large reduction in NSF spending since 1968 seems proper to me.

Moving on, of course fundamental science and mathematics is a good investment. However, if I were to choose a candidate for "most promising investment", I would vote for trying to ensure the education, health, and general welfare of our children; I'm sure that other readers could come up with other similarly worthy projects. Even if the "Testimony" is correct in its later attribution of our "unparalleled prosperity" to investment in basic research, that doesn't mean that we should continue to funnel huge amounts of money into basic research. Perhaps if that money had been spent on other areas (e.g., the arts, social justice, education), we would still have "unparalleled prosperity today", albeit of a different nature, as a result of that investment. Shouldn't we consider trying that?

Finally, the argument that the NSF "transcends any particular approach to budgeting" is almost ludicrous. Proponents of the EPA, the FDA, the DoD, the NIH, the NEA, welfare, social justice, education, tax reform, or indeed almost anything that the government does or could do could make similar claims for the importance of their cause. There are lots of things that the government could spend money on to great benefit; why should the NSF be so transcendentally special?

This is the crux of the matter. I am not arguing that NSF funding should be cut or that it shouldn't increase: I think that spending money on the NSF could do a lot of good. But I think that spending money on lots of things could do a lot of good, and I'm afraid that, as a mathematician, I am inherently biased towards spending more money on mathematics. Thus, I would like to see a discussion of the budget that takes a broader point of view. While the "Testimony" wouldn't be an appropriate place for a broader discussion, it should be able to fit into such a discussion; I found it far too mathematics-biased to do so.

—David Carlton
Stanford University

(Received September 14, 1998)

Warnings about Forecasting Trends

In the "Forum" section of the August *Notices*, the item "Possible Trends in Mathematics in the Coming Decades", by M. Gromov, was published. Here I would like to draw attention to two weaknesses we should try to avoid when presenting such contributions. First, mathematics today is widely and diversely specialized and most mathematicians are narrowly focused. Thus, the danger of special interest pleading. Second, mathematics is very dependent on outside funding. Thus, arguments about utilitarian aspects of it must be developed and presented both forcefully and persuasively and also as to avoid harming the inner life of mathematics.

A way to deal with these two issues is to focus deeper than the views originating from any particular set of fields of mathematics or from any recent select list of major scientific and technological breakthroughs in which mathematics happened to play a critical role. And if we go deeper, then since Galileo and Newton, general modus operandi of science and technology can offer a trenchant picture, one that can also be more easily understood by the wider public. In Galileo's words, the book of Nature is written in the language of mathematics. Or we may say that till the emergence of computers, we witnessed the interaction pattern: Laws of Nature ↔ Mathematics ↔ Humans, in which we humans have used and developed mathematics as a main tool in our formulation of, dealing with, as well as discovery of the laws of nature. More recently, this interactive pattern has enjoyed two major developments. First, with the advent of computers it got more complex, namely: Laws of nature ↔ Mathematics ↔ Computers ↔ Discrete Mathematics ↔ Humans.

Second, nonlinear mathematics is becoming essential. The reason is that starting with Newton, most of the laws of nature were formulated in nonlinear terms. Lately, state-of-the-art utilitarian pressures oblige us to abandon the earlier linear simplifications and deal with the fundamental nonlinearity of most of the laws of nature. Clearly, this nonlinear revolution in mathematics is not going to be reversed in the foreseeable future, and in fact, it is one of the greatest fortunes mathematics has ever experienced. Discrete mathematics, on the other hand, is not so much about our direct unmediated interface with nature, but mainly about our interface with the phenomena of fast and massive information processing which is going on in our computers. And as such, it is—and it is going to be for quite some time—a most important venture in mathematics.

The above illustrates a way we could avoid both our endless inner squabbles of special interest pleading as well as undue interference from our external budget supporters, who, rather naturally and yet not quite wisely, may have merely utilitarian and also often short-term concerns uppermost on their minds.

In conclusion, it is important to remember that scientific research is only "one part" in the known, while it is "two parts" in the unknown. Namely, the one part known is the earlier knowledge of the researchers involved; and, on the other hand, the two parts unknown are imposed on us by the unknown itself which the researchers face, added to which come the often unknown terms of their respective engagement with the unknown. This "one part known–two parts unknown" aspect of scientific research is important to be kept in mind by both those who may venture into postulating about future trends in research as well as by those who try to judge science by utilitarian measures.

Finally, we should not forget that the more serious science, as well as technology, is merely three centuries old. And contrary to what we may often like to believe, during this short period, we humans—both as individuals and as societies—have not yet managed to understand to a satisfactory extent the ways scientific research may really work. It may indeed happen that when studying the history of science during this period, one may easily get the feeling that, well, science is not done scientifically. Such a feeling may be even stronger among those of us who happen to be older and thus may have pursued science for about one sixth of the mentioned period. With respect to mathematics, for instance, this feeling can only get reinforced when reading a survey such as the 1972 paper of Freeman Dyson, entitled "Missed Opportunities", and published in the *Bulletin of the AMS*. As far as science in general is concerned, the 1962 book of Thomas Kuhn on *The Structure of Scientific Revolutions* contains examples from the history of science which will also strengthen the mentioned feeling. All this should further serve as a warning to those who are involved in forecasting the trends of science, or in funding it, and intend to do so in a responsible manner.

—Elemer E. Rosinger
University of British Columbia

(Received September 10, 1998)

Another View of Laurent Schwartz and Vietnam

It was with great dismay that I read your review of *The Autobiography of Laurent Schwartz*. He may have been a great mathematician, but he was no hero. Together with the likes of Sartre and de Beauvoir, Bertrand Russell, Jane Fonda, Tom Haydn, and a host of other American and European Marxists, his support of the Stalinist North Vietnamese regime helped condemn hundreds of thousands of Vietnamese to summary execution, oppression, economic destitution, and imprisonment in the gulag of "Re-education Camps". The diaspora of boat people who gave up all their worldly possessions, and sometimes their lives, to flee this regime provided the world vivid testimony as to its brutality. The ravage of the community of scholars at Hue during the 1968 occupation by the North Vietnamese and their Viet Cong minions rivals the decimation of Polish intellectuals by occupying Nazis several decades earlier. It is telling that Schwartz condemned the Soviets for their oppression of Jewish mathematicians while saying nothing about the oppression of thousands of Vietnamese Buddhists, Catholics, small landowners and businessmen, and anyone too interested in self-determination. This is nothing more than blatant racism.

—James Chaffee
Austin, TX

(Received October 7, 1998)

The *Notices* invites letters from readers about mathematics and mathematics-related topics. Electronic submissions are best. Acceptable letters are usually limited to something under one printed page, and shorter letters are preferred. Accepted letters undergo light copyediting before publication. See the masthead for electronic and postal addresses for submissions.