Commentary

In My Opinion

Institutes under Review

This year the National Science Foundation (NSF) has begun the process of deciding the future of NSF funding of the U.S. mathematical research institutes created under the auspices of the NSF (e.g., Mathematical Sciences Research Institute (MSRI) and the Institute for Mathematics and its Applications (IMA)). The NSF has called for proposals from other groups of mathematicians to be considered in competition with the existing institutes and has consulted various mathematical groups concerning the community's opinion on more/fewer, similar/different/innovative modes of institutes. My peripheral involvement with this discussion has led me to these thoughts about NSF funding and the role of research institutes.

Rightly so, much attention has been given to the miserable situation of young mathematicians with no secure job. But creating such a secure job is beyond the capabilities of the NSF or new or existing institutes. At best they can increase the number of postdoc positions and "pray" that in years to come the job situation will improve. The need for research support for mathematicians with tenure or tenure-track positions has been somewhat neglected, even though this group makes up the vast majority of active mathematicians. This situation is unique to mathematicians among the scientists supported by the NSF. As a community we have allowed grant support for individuals to erode to a pitiful state. Other sciences have fought this trend and now have a much higher percentage of their active members supported. For example, the top NSF priority of the Federation of American Societies of Experimental Biology is to emphasize the importance of individual investigator grants.

Many established research mathematicians could receive great benefits from a stay of a year or semester at one of the existing research institutes. However, other than sabbatical support from their own universities (which is drying up at a number of universities) the only award senior mathematicians can even apply for is a Guggenheim (and no more than half a dozen of these go to mathematicians each year).There are a few more possibilities for midcareer mathematicians, such as Sloan Fellowships, but such awards are also very limited in number. The research institutes themselves give little or no financial support to many of these very active (often grantless) mathematicians. Some can come on their own money (maybe half salary), but many just cannot afford to do so. These "more senior" mathematicians often carry the major load of work in their department in terms of teaching, committees, administrative chores, etc. A year at an institute allows them to escape and fully immerse themselves in research in a stimulating environment. Furthermore, the institute benefits from the maturity that such mathematicians can bring to their programs, including the vitality of established research and the mentoring of more junior mathematicians. At present there is just no possibility for many very able mathematicians to have such opportunities. This is a loss to the individuals, a loss to creation of research mathematics, and a deterrent to aspiring mathematicians considering their career paths.

My recommendation to the NSF to address this problem is not to fund more institutes, because the expense of creating new infrastructures will inevitably pull money away from the already too-limited pool. The structure of the existing institutes has many positive features and should continue while being open to new ideas and developments that could fruitfully involve both junior and senior mathematicians. If indeed new money can be obtained for institute activities, I would advocate setting up a program of sabbatical-type grants open to mathematicians at all stages of their careers to enable them to pursue their research in a peaceful but stimulating environment. This might be at a "classical" institute or might be at a research group at a university or maybe in an industrial or financial setting. The more flexible and broadly based the program, the more successful it is likely to be. I believe that giving mathematicians the chance to be intensively involved in their research for periods of three months or longer is the most valuable aspect of a mathematical research institute.

—Susan Friedlander
Associate Editor

Letters to the Editor

Of Frogs and Men

Mary Beth Ruskai fails to point out an important property of mathematical modelling in her May 1998 opinion column, "The Decline of Science". It, in any form, is the only way to calculate the consequences of actions and events. To optimize our outcomes, one must inevitably compare the state of the world as it is to all the states it possibly could be: i.e., to events that exist only in the abstract. Therefore, comparison of events is, by definition, an abstract process. Experimentation has nothing to do with this.

As an example, Ruskai mentioned the issue of what's to be done about dissection. The following questions could be formulated more precisely in terms of game theory and fair-division problems. Of course, I can't do that here in the "Commentary" section.

1) Maximize justice. Do all the billions of people who preach about personal responsibility deserve to reap whatever health benefits they think might come from dissecting frogs? No, especially non-vegetarians. They should take responsibility for their

own health. It is not the frog's responsibility.

2) Minimize number of frogs being dissected and maximize humans' knowledge about what frogs look like inside. After animal dissectors cut open the first frog to find out what's inside, and report on it, videotape it, or make a computer model of it, there is no need thereafter to deliberately breed millions more for dissection. Ruskai fails to ask whether we even need to know the notion of external world, objective reality, universality, or truth about organ positions of frogs.

If scientists had made the sorts of calculations that I have suggested above before breeding millions of animals and pursuing dissection, then millions would not be forced to suffer in laboratories each year, and millions of our tax dollars would have been saved. That is what mathematics is meant to do.

—John M. Nahay
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Science Funding and Granting Agencies

Though I, in principle, agree with Arthur Jaffe that "investigator-initiated projects" lie at the heart of our work (*Notices*, May 1998, p. 564), I remain highly skeptical that doubling (tripling, quadrupling, etc.) of science funding will bring much good unless it is accompanied by a fundamental overhaul of the grant distribution system itself.

Operations of major research-granting agencies, such as the National Science Foundation in the USA or the Natural Sciences and Engineering Research Council in Canada, are shrouded by secrecy (R. Gordon, *Grant agencies versus the search for truth*, Accountability in Research **2** (1992), 297–301). Claims that the process is based on objective peer review are of little merit because for all practical purposes it is impossible to verify that all applicants are treated equally and fairly. Thus, instead of the alleged impartiality, the system often degenerates to a notorious "old boys' network" whose members primarily care about arranging lavish funding for themselves. Naturally, such a system fiercely resists any genuine public accountability.

In place of the overblown and bureaucracy-loaded funding agencies, we need a much simpler funding mechanism which will fund many more researchers on a more equitable basis, even if it means lower average grants (e.g., A. A. Berezin and R. Gordon, *Smaller grants for more Canadians*?, Nature **386** (20 March 1997), 212). Only clear incompetence should be a sufficient reason to deny any operating funds. Without such a reform new research dollars (if obtained) are almost certainly largely adding more fat to the already overbuilt empires of the grantsmanship establishment instead of fostering true innovation and risk taking.

—Alexander A. Berezin
McMaster University, Canada

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Analyzing the TIMSS 12th-Grade Exam

The reported results of our American twelfth-graders on the mathematics and physics portions of the Third International Mathematics and Science Study (TIMSS) are dismal, but even more depressing was the performance of our advanced students (honors and AP calculus) on the Advanced Mathematics Test. Of sixteen countries, all European, we were near the bottom in the three areas tested: numbers and equations, calculus, and geometry. If these students are our best, what does it say about our training of the rest?

I recently received an informal report from the Educational Testing Service (ETS) further analyzing the 65 questions on the Advanced Mathematics Test, and it is even more depressing. Some findings:

1. Only 13 of the 65 questions measure content from calculus, and these are at a basic level of the Advanced Placement Calculus AB course, not the more advanced BC course. The questions measure a minimal part of a basic first-semester course.

2. The remaining 52 questions measure topics from geometry, second-year algebra, and precalculus, which would include trigonometry, elementary functions, and analytical geometry.

3. It would be expected that our good Calculus AB or Calculus BC students would do well on the calculus portion of the test.

What this indicates is that our best students have a very poor preparation in the mathematics needed to do calculus. Some examples:

- Only 68% of U.S. advanced mathematics students could answer correctly a multiple choice question (five answers given) asking for the solution set of the inequality

$$5X + 5/3 \le -2X - 2/3.$$

- Only 47% of U.S. advanced mathematics students could identify a triangle with vertices (1,2), (4,6), and (-4,12) as a right triangle with right angle at (4,6)—multiple-choice question (four answers given).

If you wonder about the gaps you encounter in your college or university calculus students' mathematics background, wonder no more.

A possible reason for our students' failing performance could be attributed to the prestige that AP has acquired. Schools get extra gold stars from their district or state for offering AP courses, there is a current clamor in some states for the creation of an AP diploma for students who have successfully passed a certain number of AP courses or tests, and colleges and universities have long given credit for high grades on AP tests. In the rush for status, are high schools pushing students into AP calculus courses before they have the necessary solid background in algebra and geometry? Our country's abysmal results strongly support an answer of "Yes".

The mathematics departments of research universities long ago gave up serious involvement in K–12 matters, including the training of teachers. I think that decision has come back to haunt us.

—David A. Sanchez
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Chowla-Selberg Formula

In the May 1998 issue of the *Notices*, on page 596, Ayoub, Huard, and Williams say:

"In 1967 Selberg and Chowla discovered …" and proceed to state the Chowla-Selberg formula. However, the result was first announced by Chowla and Selberg in *On Epstein's zeta function. I*, Proc. Nat. Acad. Sci. USA **35** (1949), 371–374. Their 1967 publication of a proof was the second proof: K. Ramachandra in *Some applications of Kronecker's limit formulas*, Ann. Math. (2) **80** (1964), 104–148, preceded them with essentially the same proof. This proof is given its most elegant form in chapter IX of A. Weil's *Elliptic Functions according to Eisenstein and Kronecker*, Springer, 1976.

From the article by Ayoub, Huard, and Williams it is perhaps not apparent why the Chowla-Selberg formula is so important. In 1978 B. H. Gross in *On the periods of abelian integrals and a formula of Chowla and Selberg*, Inv. Math. **45** (1978), 193–211, one of the most beautiful papers in modern mathematics, gave a new proof of the Chowla-Selberg formula.

Briefly put, Gross finds a family of abelian varieties with complex multiplication by the imaginary quadratic field in the formula. The left side of the formula is a period of one of these abelian varieties, and he shows that there exists a constant period family of abelian varieties, one of which is a factor of the Fermat curve Jacobian, where an explicit calculation gives the gamma function terms on the right side of the formula.

Gross's ideas helped inspire Deligne's work on periods of *L* functions and his later work on absolute Hodge cycles. See P. Deligne, *Valeurs de fonctions L et periodes d'integrales*, PSPM **33** (1979), 313–346. This is perhaps the true meaning of the Chowla-Selberg formula.

—*Oisin McGuinness*
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Huard and Williams Reply

The Chowla-Selberg formula given in our article on p. 596 was first given by Chowla and Selberg in this form in their 1967 paper (formula (2), p. 110) but not in their 1949 paper, which was simply an announcement of results. However, in the 1967 paper it is asserted that the paper was written in the spring of 1949. Therefore, it would have been more accurate for us to have stated that the formula was discovered in 1949 but first published by Chowla and Selberg in 1967. It was not our intention in the article to give a detailed history of each result quoted, as some of them have complicated histories: for example, the Bruck-Chowla-Ryser theorem and the Chowla-Selberg formula. Moreover, we also did not discuss in any detail any further developments inspired by Chowla's results.

—*James G. Huard*
Canisius College
—*Kenneth S. Williams*
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Russo's Speculative Interpretations

I have not yet seen L. Russo's book *La Rivoluzione Dimenticata*, which was so enthusiastically reviewed in the May *Notices*. But I have read the earlier presentation (in *Vistas in Astronomy*) of its startling claim that Hipparchus had a heliocentric dynamical gravitational theory of planetary motion. On this point, at least, I fear that the author's enthusiasm for his ideas has led him to rely on quite speculative interpretations of isolated bits in the texts he cites.

Let me illustrate this with Russo's interpretations of two bits from the few pages Vitruvius devotes to astronomy in his work on architecture. First, he says that "Vitruvius' exposition of the motions of Mercury and Venus is explicitly heliocentric." The words he cites do indeed say that the the paths of Mercury and Venus "circle the rays of the Sun as a sort of center." But Vitruvius immediately goes on to say that this is particularly clear for Venus, because it is prominent as the Evening Star when it follows the Sun and prominent as the Morning Star when it precedes the Sun. Obviously Vitruvius is describing the observed positions of Venus, not some theory of how they arise. Any possible doubt of this is dispelled when we read (two paragraphs later) that Venus completes its circuit "on the 485th day"; that must refer to the observed position on the ecliptic.

Second, Vitruvius says that the outer planets begin retrograde motion when they are in the trigon of the Sun ["cum in trigono fuerint, quod is inierit, …regressus facientes morantur"]. A bit later he asks why this happens in the fifth sign rather than in the second or third signs, which are closer to the Sun. His suggestion is that the force of the Sun runs along a shape like the equal sides of a trigon. Russo decides that "fifth sign" here must mean the fifth point in some geometric diagram, even though it means "zodiac sign" in all the surrounding sentences. He also decides that "equal sides of the trigon" refers to isosceles triangles, despite the fact that "trigon" in this context almost always refers to an equilateral triangle formed by three points on the ecliptic (the second of which thus lies in the fifth sign from the first). He goes on to spend several pages inventing a diagram that resembles something in Newton, though he admits that in his diagram the second and third "signs" are not actually closer to the Sun (he calls this "a natural consequence of Vitruvius' misunderstanding"). Russo's treatment here has obviously lost all contact with the basic observational fact that the outer planets begin retrograde motion when they are roughly 120 degrees away from the Sun.

—*William C. Waterhouse*
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