Suggestions for the Mathematical Sciences Initiative

The NSF (National Science Foundation) has announced a Mathematical Sciences Initiative to substantially increase funding for mathematical research [1], [2] and has requested input from the mathematics community to help shape the specific details.

As we begin to think about how we might respond to this, I would like to suggest two areas for discussion.

1. The citizen/permanent resident requirement. According to [1], one of the programs to be emphasized is VIGRE (Vertical Integration of Research and Education in the Mathematical Sciences), which requires that all beneficiaries be either U.S. citizens or permanent residents. The same requirement pertains to the REU (Research Experiences for Undergraduates) program. (It should be noted that this requirement is not in force for the personnel involved in running these programs, such as graduate students who work with undergraduates in the REU component of a VIGRE grant, whereas the requirement does apply to graduate students receiving VIGRE support directly for their own benefit!) I believe this requirement is both discriminatory and counterproductive. As I am involved in the administration of both types of programs at Cornell, it gives me great pain to have to enforce this rule. We as a society have worked very hard to eliminate discrimination on the basis of a host of categories. Why should we now be inventing yet another category of discrimination (which may well become equally discredited in the future)? When a student comes to you and asks to join in some research activity, do you normally ask about the student's residency status? I hope not. But if you wish to support the student with one of these grants, you are forced to do this.

One purported reason behind this policy is that citizens/permanent residents are more likely to remain in the U.S. during their research careers. I see very little evidence for this. Most foreign students and postdocs at U.S. universities are very eager to remain here, largely because of our vibrant research community, not to mention the economic benefits. Many of them obtain tenure-track positions and become permanent residents within a few years. I also know quite a few U.S. citizens who have opted to take permanent positions in Europe, lured away by the quality of the research community there. I think that there would be no detectable difference in the impact of these NSF programs on the well-being of the U.S. mathematical research community if the citizen/permanent resident requirement were dropped.

Another possible justification for the requirement is that current citizens/permanent residents need some shelter from the competition posed by the outstanding foreign students who are, after all, the selected elite in their home countries. I can concede that there may be some merit to this argument, since U.S. taxpayers may be eager to see a boost in the accomplishments of home-grown students. Nevertheless, the inflexibility of the requirement strikes me as a vast overreaction to a limited problem. It would be much more reasonable to set a percentage goal (or even quota) for the number of citizen/permanent resident participants in these programs.

2. The teaching imperative. The current situation, with a large majority of permanent positions in mathematical research being university professorships, is that anyone contemplating a career in mathematical research faces the imperative of having to become involved in teaching. Perhaps this is not such a good situation. Mathematical researchers tend to run the gamut from those who are natural teachers to those who are completely hopeless at teaching to those in between who are capable of learning, with some effort, to function acceptably in the classroom. Those who find themselves at the negative end of the teaching-talent spectrum often find themselves struggling in a position that serves neither them nor their students very well. I know of cases where promising graduate students have dropped out after negative experiences as TAs, and I know a number of foreign mathematicians with high research achievement who cannot effectively obtain positions in this country because of the teaching requirement. So I believe there is the potential to improve the quality of the mathematical research community in this country by creating a substantial number of permanent research positions with no teaching duties.

The details of how such a program should operate will require a great deal of careful thought. There are many possible models: the Institute for Advanced Study, the French CNRS (Centre National de la Recherche Scientifique), the old Bell Labs, the Institute for Defense Analyses, to name just a few. The researchers could be housed in a separate facility or in existing mathematics departments and institutes. They could be allowed complete freedom to pursue individual research interests, or they could be given a certain amount of consulting or applied research duties.

I think that existing mathematics departments and institutes would be very happy to cooperate with such a program if it meant gaining the services of talented research mathematicians at little cost (for example, NSF pays salaries and benefits, and the department provides office space and technical support).

With an initiative of the size being discussed, it should be possible to include such a program without crowding out other priorities. What would be the cost of such a program, and would the benefits be worth the cost? These are questions that need to be looked at carefully, but I think this is an option worth serious consideration.

—Robert S. Strichartz
Cornell University

References

[1] Allyn Jackson, NSF launches major initiative in mathematics, *Notices* (February 2001), 190–192.

[2] Philippe Tondeur, NSF Mathematical Sciences Initiative, *Notices* (March 2001), 293.

Letters to the Editor

Divergence of the Harmonic Series

I read, with not inconsiderable interest, the article by Catherine C. McGeoch: "Experimental Analysis of Algorithms", *Notices of the AMS*, v. 48, n. 3, p. 304, 2001 March. Therein she asserted: "If you want to know how the process really works, implement the algorithm as a program and measure the running time (or another quantity of interest)." I believe I am aware of a rather striking counterexample to her thesis: the inability of the IEEE Standard 754 floating-point arithmetic adherent hardware implementations (ref. `http://www.cs.berkeley.edu/~wkahan/ieee754status/754story.html`) to demonstrate the well-known divergence property of the classical Harmonic Series (ref. `http://www.mathacademy.com/platonic_realms/encyclop/articles/serie.html`)!

I have attempted to demonstrate this classically known property on several modern computer platforms—all sans success! All I obtained was convergence, not the sought-after demonstration of divergence! This anomaly was apparent in C language implementations using "double" as well as "long double", and it was ubiquitous, so far as I am able to ascertain!

Therefore, by means of this rather simple counterexample, I must caution theoretical mathematicians of the possible failure of even modern platforms to fiducially reproduce well-known behavior.

However, it may be possible that I have overlooked something crucial in the successful resolution of this situation. I would, therefore, welcome any advice from your readership thereupon.

Tempus fugit et ad augusta per angusta.

—Joseph Roy D. North
District Heights, MD

(Received February 17, 2001)

Segal and Cosmology

I am bothered by "Einstein's static universe: An idea whose time has come back?", by Daigneault and Sangalli, which appeared in the January 2001 *Notices*. I can't discuss the Big Bang with any real expertise, but I have talked to cosmologists. Their work seems more serious and more credible to me than the article suggests. The article compares I. E. Segal to Giordano Bruno and the inflation theory and evolution of quasars to epicycles. I have to take these comments as allusions to Bruno's colleague Galileo, and they remind me of an aphorism of the physicist Robert Park: "Alas, to wear the mantle of Galileo it is not enough that you be persecuted by an unkind establishment, you must also be right" [What's New, 11 June 1999, `http://www.aps.org/WN/WN99/wn061199.html`].

In order for any theory in science to prevail, it ultimately has to be useful. If we want to help cosmologists, we have to engage them on their terms rather than ours. I don't know that Segal ever seriously tried to do so. I also think that the *Notices* should invite a cosmologist to respond to Daigneault and Sangalli. We might learn a lot if cosmologists explained why they think that the Big Bang is overwhelmingly likely and listed what they consider the real debates in cosmology today.

—Greg Kuperberg
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Amplification

After receiving some feedback on my survey article "From rotating needles to stability of waves: Emerging connections between combinatorics, analysis, and PDE" in the March 2001 issue of the *Notices*, I feel that I should make some additional remarks to ward off any misconceptions that might have arisen from the original article.

Firstly, the results mentioned therein are only a small fraction of the large amount of work and progress accomplished on these problems, and due to space constraints I was able to give only a few representative results on each problem. (I was also advised to keep the reference list to under 10 articles.) Consequently, some authors and their results were mentioned only briefly or not at all, for which I apologize. Far more thorough treatments can be found in the references [1] and particularly [7] of the article. ([1] J. Bourgain, Harmonic analysis and combinatorics: How much may they contribute to each other?, *Mathematics: Frontiers and Perspectives*, IMU/Amer. Math. Soc., 2000, pp. 13–32; [7] T. Wolff, Recent work connected with the Kakeya problem, *Prospects in Mathematics* (Princeton, NJ, 1996), Amer. Math. Soc., Providence, RI, 1999, pp. 129–162.)

Secondly, the main point I was hoping to make in the article was that the open problems posed there appear to be extremely difficult and that deep ideas from other fields could be needed to make substantial new progress. However, this is not to disparage the considerable amount of progress and insight that have already been achieved; in recent years the breakthroughs of Jean Bourgain and Tom Wolff in particular have revolutionized the field. The arguments and ideas coming from these breakthroughs continue to yield further progress on these problems today. Nevertheless, it is my opinion that even with these powerful new techniques, we have about half of the pieces of the puzzle required to solve even the Kakeya problem (which should be the easiest of all the problems listed) and that further ingenious ideas or insights are still needed to obtain a complete resolution.

Shortly after the completion of the first draft of this article, I received the terrible news that Tom Wolff had died in a car accident. This was a great loss not only personally but also to the field; many of the recent developments in the field are due to or inspired by the work of Tom and his students. The rate of progress on these problems will be greatly diminished in his absence.

—Terence Tao
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