Opinion

F.A.M.S.

The Council of the Society, at its meeting in New Orleans this past January, approved a plan to increase the frequency and uniformity of AMS prizes and also introduced one new prize. The proposal originated in the Committee on the Profession and was endorsed by the Board of Trustees prior to approval by the Council, receiving enthusiastic support in all three fora: mathematicians in the Society's leadership were virtually unanimous in declaring that mathematics needs more prizes.

That prizes are a boost to an individual scientist's career is well established. In their now classic study of the physics community, Social Stratification in Science (University of Chicago Press, 1973), Jonathan and Stephen Cole looked at the role of prizes in a phenomenon they called "the accelerating accumulation of rewards", whereby slightly better scientists, because of the recognition effect, end up with vastly superior reputations and positions. Prizes, especially those that come early in a career, serve to mark the recipient as especially meritorious, which results in an increase in opportunities. The authors never reveal whether they view the phenomenon as benign or, for that matter, as an efficient way to allocate resources. Perhaps today we would wonder more about the effect on scientists from underrepresented groups or on those working in geographically disadvantaged venues.

As part of their study, the Coles investigated how important the prestige of the prize itself was in this process by asking a representative sample of physicists to rank prizes. As a control, they salted their survey with a few plausibly named but fictitious prizes. Sure enough, the phony prizes were deemed to carry prestige too! The real point is that had there been such prizes, they would have aided in recognition of the recipients as well.

One could debate whether mathematics has its own "accelerating accumulation of rewards" (or whether such a system would be desirable). But even if this system does exist in mathematics, prizes do not seem to play much of a role. In fact, one common theme in the discussions about the recent actions on AMS prizes was the view that mathematics is at a disadvantage compared to other sciences because of its paucity of prizes. Department heads were especially articulate on this point: they believed that many university honors that could justifiably have gone to mathematicians went instead to faculty in other disciplinary units simply because those disciplines have many more prizes than does mathematics.

Of course, there could never be enough prizes to honor all deserving mathematicians. But perhaps we could borrow another recognition device that many sciences employ: in my opinion, the AMS could have a special class of distinguished members identified as "Fellows of the American Mathematical Society". Based on the similar fellows programs of other societies, I would guess that we would name about 50 fellows a year, so that at steady state there would be about 1,500 to 2,000 Fellows of the AMS. Leave aside the problem of how to start (I assume there are at least 200 people who would be in the top 50 of current U.S. mathematicians), and leave aside the problems of formulating the criteria for fellowship and the mechanism for selecting fellows. Can we even conceive of a process that could identify 50 mathematicians a year for this honor?

In fact, we have a good model of that right now. The Society invites about 50 people a year to address its regional and national meetings. While the criteria for selecting fellows would be slightly different, my guess is that once steady state is achieved the set of people who have given an invited address and the set of fellows would be virtually identical. Indeed, we could use this observation to solve the start-up problem: we simply declare the former set to be grandmothered/grandfathered to fellowship status. We could even expand the charge to the existing program committees so that they name the new class of fellows each year as well.

Regardless of how a fellows program might be implemented by the Society, however, I think the benefits of the program are clear. In identifying mathematicians as fellows, the Society would be granting them recognition easily explained to deans and provosts, thereby enabling their universities to honor, reward, and compensate them on a par with their peers in the other sciences.

> —Andy R. Magid Associate Editor

Letters to the Editor

Comments on Tao's Article

Terence Tao's article "From Rotating Needles to Stability of Waves: Emerging Connections between Combinatorics, Analysis, and PDE" (March 2001 issue of the *Notices*) contains many historical inaccuracies. As an example, I will discuss here the origin and early history of Besicovitch sets. Further comments are posted on my Web page at http://www.math.ubc.ca/ ~ilaba/comments.html.

According to Tao, "In 1917 S. Kakeya posed the Kakeya needle problem: What is the smallest area ...? In 1927 the problem was solved by A. Besicovitch ..." (page 294). And further: "Historically, the first applications of the Kakeya problem to analysis arose in the study of Fourier summation in the 1970s" (page 298).

However. Besicovitch himself tells a very different story in Amer. Math. Monthly 70 (1963), 697-706. (See also K. J. Falconer, The Geometry of Fractal Sets, Chapter 7.) Namely, in 1917 Besicovitch was working on a problem in Riemann integration and was able to reduce it to the question of the existence of compact sets of measure zero in the plane which contain a unit line segment in each direction. He constructed such a set in 1919 and published his results in a Russian journal in 1920. About the same time (1917), Kakeya first mentioned the "needle problem". The original question, in which the planar set was required to be convex, was promptly resolved by Pal (1921); the analogous question without the convexity assumption remained open.

There was hardly any communication between Russia and the rest of the world at the time, due to the civil war and the blockade. Hence Besicovitch's work received little attention outside of Russia, and neither did Kakeya's question reach him. Besicovitch was told of Kakeya's problem shortly after he left Russia in 1924, and he resolved it in 1925 by modifying his original construction. His solution was published in *Math. Zeitschrift* in 1928.

It moreover follows that applications of "Kakeya sets" to analysis (the Riemann integration problem just mentioned) are as old as Besicovitch's construction itself. See also Busemann and Feller, *Fund. Math.* **22** (1934), 226–256, where Kakeya sets are used in the context of differentiation of integrals. Furthermore, it is interesting to note that Stein and Weiss (*Trans. Amer. Math. Soc.* **140** (1969), 34–54) used a closely related construction due to Nikodým (1927) to disprove the unrestricted convergence of Poisson integrals.

—Izabella Laba University of British Columbia

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Editor's Note: See also Terence Tao's letter in the June/July 2001 issue.

History of the Woods Hole Fixed-Point Theorem

I recently noticed the following passage in "Interview with Raoul Bott", *Notices* vol. 48, No. 4 (April 2001), p. 379.

"In 1964 Michael and I were together again in Woods Hole, at an algebraic geometry conference.... During that conference we discovered our fixed point theorem, the Lefschetz fixed point theorem in this new context."

I can certainly appreciate that they proved the theorem in the context of an elliptic complex, but I strongly disagree with him in his saying "we discovered," as it suggests that they discovered it completely on their own. What he says contradicts what he and Atiyah said thirty-six years ago.

In fact, in the introduction of "Notes on the Lefschetz fixed point theorem for elliptic complexes", Harvard University, Fall 1964, they wrote: "Our main formula also generalizes a result of Eichler on algebraic curves which was brought to our attention by Shimura during the recent conference at Woods Hole on algebraic geometry. In fact, this work resulted precisely from our attempt to prove Shimura's conjectures in this direction."

Also, their article in *Bull. Amer. Math. Soc.* **72** (1966), 245–250, contains the following sentence: "The first of these [which means Theorem 2 in that article] was conjectured to us by Shimura and was proved by Eichler for dimension one."

I don't remember whether there is a similar acknowledgment in their paper [42] (*Ann. of Math.* **86** (1967)); probably not in the introduction.

A large number of mathematicians participated in the conference, and I think many of them still remember that the theorem came into existence because of my conjecture. I wonder if they can accept the phrase "we discovered."

The same paragraph ends with the following sentences: "The number theorists at first told us we must be wrong, but then we turned out to be right. So we enjoyed that!"

This is completely wrong. As far as I can remember, no number theorist said they must be wrong. After all, I conjectured it in the holomorphic case, and no number theorist was knowledgeable enough to be against its formulation for an elliptic complex. I may be excused to say that these sentences were added in order to say that they "discovered" it without help from the number theorists, of whom I am one.

> *—Goro Shimura Princeton University*

(Received April 13, 2001)

Response to Shimura's letter

Professor Shimura's point is well taken, and I apologize for this gaffe in my interview. Had I the power to replace the two offending sentences, I would gladly replace them by:

> At Woods Hole Atiyah and I discovered how to generalize Shimura's conjectured fixed point formula to the elliptic context, and eventually we were able to establish this generalization by pseudo-differential techniques.

There remains the puzzle of how my original account came about. Unfortunately, an answer to this question involves me in precisely what I was trying to avoid at this late stage of the interview, namely, in relating yet another long story. But so be it, and let that be my punishment for failing to censor my original impulsive account in the final draft.

First, however, this forewarning especially for our younger readers. In his wisdom the Good Lord has endowed all of us with very selective memories, designed to make life bearable even in old age. On the whole we tend to remember even the smallest of triumphs but forget all but our greatest blunders. Please keep this in mind during the following narrative.

For reasons which are now hidden from me, Michael Atiyah and I started our experimentation with a holomorphic fixed point theorem at the very start of the conference. I believe our experiments had to do with the Hecke correspondence in imaginary quadratic extensions. In any case, I have definite memories about my puzzlement that although fixed points were counted with complex numbers, they nevertheless added up to integers in the appropriate circumstances. Our computations dealt with correspondences on curves as well as maps. In any case we finally consulted some of our numbertheoretic friends, and it was at this stage of our deliberations that our computations with the conjectured formula were at first declared to be wrong, but after more careful analysis were found to be correct. This is the incident referred to in my second sentence. A minor triumph, no doubt, but one that lifted our spirits and convinced us that we were on to something. This incident is confirmed by Michael, but not remembered by our consultants.

The next part of my account is even more murky, but I would be less than honest if I did not admit to it here. I seem to remember that we did these or similar computations before we interacted with Shimura! According to my memory, it was precisely during our search for the history of such formulas, and after we had been referred to Eichler's work by several other people, that we were delighted to find an expert on these matters in Shimura, who set us straight and informed us that he had, in fact, conjectured the holomorphic fixed point formula in full generality for some time. Here my recollection is that we were not aware of the general formula before we talked with him. From that time on we of course, and quite properly, referred to the fixed point formula as Shimura's conjecture, but subjectively I always remembered this encounter more as a confirmation than a revelation.

In any case, this interaction now made us all the more determined to find a proof. At this stage, I think, we also discovered how perfectly this Lefschetz formula fitted the Hermann Wevl character formula and found other interesting examples. Simultaneously we mercilessly consulted the large number of algebraic geometers at the conference in this regard, and eventually, in a special seminar devoted to this topic, a proof of the Lefschetz formula in the algebraic context was sketched out. In view of the large number of inputs to this result, it was named the "Woods Hole Fixed Point Theorem". I believe that I served as a sort of master of ceremonies at the event. This proof was sheaf theoretic and used the internal Hom and derived functors but was not considered too difficult by the experts.

These techniques are not directly applicable in the holomorphic category, and so Michael and I, who had mainly been producers rather than actors in the developments so far. turned our attention to this case and eventually to the even more general elliptic version of the theorem. An especially memorable moment for us occurred during a walk in the gardens of the Whitney estate, when we discovered that the Dirac operator fitted into the picture. And, as I remarked earlier, we eventually produced a proof using essentially pseudo-differential techniques.

Finally, a comment on the quotes in Professor Shimura's letter from the contemporary accounts of the Woods Hole story, both of them also written by me, I believe. Alas, here I must plead guilty once again to my penchant for cutting long stories short, for I have a distinct memory of debating with myself whether to include some of the above in those accounts, but at that time and in that context it seemed to me inappropriate. This then is Bott's long, long story. Is it true or a figment of my imagination? I am afraid that will be difficult to determine, given the universal nature of the "Anosov" evolution of our memory with time, which I alluded to earlier. But, true or not, let me end by expressing my sincere regret to Professor Shimura for having omitted his name altogether in my interview. In view of the foregoing, all I can do now is plead for his indulgence for my having committed this "Freudian" lapse.

> —Raoul Bott Harvard University

(Received May 14, 2001)

Mathematical Sciences Initiative

We write in response to the "Opinion" piece about the NSF Mathematical Sciences Initiative by Philippe Tondeur (March 2001, p. 293).

The mathematical research community has been skeptical about such matters in recent years, and it is easy to see why. Historically the backbone of NSF support of mathematical research has been support to Principal Investigators (PIs) on small grants. During two decades of various NSF initiatives, the number of proposals has risen by 50%, while the number of PIs supported dropped, reaching a twenty-year low in 2000. Moreover. the number would be lower still were it not for cuts in the level of support. Everyone who has been involved in the wrenching decisions process knows that the point at which cutoffs occur is well above the point at which work still merits support.

The funding of institutes, of conferences, and of educational initiatives, important as it is to mathematics, has nonetheless done little to alleviate this stark reality. Therefore, a skeptical reader of Director Tondeur's article might expect limited impact for researchers—in particular, little or no additional support of PIs. Responses from the community could range from indifference to opposition.

What is important now is for the community to recognize that this time things really could be different. The writers of this letter were among the members of the most recent triennial Committee of Visitors to the Division of Mathematical Sciences. We are persuaded that there is at present an understanding at the highest levels of NSF that the strength of the mathematical sciences is crucial to science as a whole, that the support for mathematical sciences must increase significantly, and that support of the research of individuals, as well as of groups, is a vital component.

If the current initiative is carried through as presented to us. the number of individuals supported on individual grants, the level of funding of individual grants, the support of research (and researchers) in other ways, and the support of graduate students and postdocs would all increase significantly. We would see a blossoming of opportunities for fostering the research efforts of mathematicians, both within mathematics and in collaboration with colleagues in other disciplines. This would be a major step forward in ensuring the health and the future of the mathematical sciences.

Whether this rosy picture can be realized within the current budget plans in Washington is not at all clear. Recent developments show that NSF and basic research do have some friends in Congress. We need to cultivate these friends by persuading our representatives, and the public in general, of the benefits to society of supporting basic mathematical research. What is clear is that divisiveness or indifference in the mathematical research community would make any change for the better even less likely. We urge our colleagues in the community to inform themselves about the potential impact and the exciting challenges in this initiative, to support the initiative wholeheartedly, and to make that support known vigorously.

> —Sheldon Axler San Francisco State University, —Richard Beals Yale University, —Tony F. Chan Institute for Pure and Applied Mathematics, and UCLA, —Rick Durrett Cornell University,

—C. Ward Henson University of Illinois at Urbana-Champaign, —Blaine Lawson SUNY at Stony Brook, —William W. Symes Rice University, —Carol Wood Weslevan University

(Received April 18, 2001)

The Boris Weisfeiler Legal Fund

Boris Weisfeiler, who was a professor of mathematics at Penn State University, disappeared in Chile during his hiking trip on January 5, 1985. Ten days later his backpack was found on the riverbank of the Nuble River. The official investigation of Boris's disappearance was closed shortly thereafter. In the Chilean press of those years there had been some speculation that Boris Weisfeiler was still alive and was being kept captive in the Colonia Dignidad, a German-speaking settlement with rumored Nazi connections.

During the next fifteen years Boris's family, colleagues, and friends tried unsuccessfully to find out what really happened to Boris, but no additional information was ever available. All the information received by the U.S. Embassy in Chile regarding Boris's whereabouts was classified and kept sealed in the embassy's files and the files of the U.S. Department of State.

Nevertheless, the Chilean lawyer Hernan Fernandez, who has been working on the case on behalf of the Weisfeiler family for two and a half vears, was able to reopen the case in the Chilean courts in January of 2000. On June 30, 2000, complying with the Chile Declassification Project, the U.S. Department of State declassified more than 250 official documents related to the disappearance of Boris Weisfeiler. Since October of 2000 the Chilean Supreme Court and Judge Juan Guzman have been handling the case. In view of the current political climate in Chile, this investigation may become lengthy, and additional legal help will be needed to finally uncover what happened to Boris Weisfeiler. There remains some possibility that Boris Weisfeiler is still alive and is living as a prisoner within the Colonia Dignidad.

The Committee of Concerned Scientists, of which Boris was an active member, with particular concern and expertise on human rights abuses in Soviet mathematics, has set up The Boris Weisfeiler Legal Fund to provide financial support for ongoing investigation.

Tax-deductible contributions can be made by writing checks payable to the Committee of Concerned Scientists, with an indication on the check that it is for the Weisfeiler Fund. They should be mailed to:

> Mrs. Dorothy Hirsch Executive Director Committee of Concerned Scientists 53-34 208th Street Bayside, NY 11364

More information regarding Boris Weisfeiler's disappearance in Chile is available at the Website of the Committee of Concerned Scientists, http://www.libertynet.org/~ccs/, and at http://weisfeiler.com/ boris/.

> —V. Kac Massachusetts Institute of Technology, —D. Kazhdan Harvard University, —G. Margulis Yale University, —B. Mazur Harvard University

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Editor's Note: The AMS Council has endorsed the Boris Weisfeiler Legal Fund; see "Inside the AMS" in this issue of the *Notices*.