Editor's Log, AMSY 2005

As I write this in mid-January calendar year (CY) 2006, we are 25% (or 50%) into federal (or most states) fiscal year (FY) 2006 and one-half (semester system) or one-third (quarters) into academic year (AY) 2005–06. For the officers and committee members of the American Mathematical Society, however, it's still AMSY 2005. More precisely, terms of service for the Society run February 1 through January 31, so that old terms end and new ones begin after the session of the AMS Council which takes place during the January Annual Meeting. At its meeting this January, in addition to committee appointments and other business, which will be reported on in due course in the "From the AMS Secretary" section, the Council took three actions affecting the *Notices*. I want to reflect on them.

The one which will affect readers of the *Notices* most is the decision to remove the requirement, implemented last spring, that users of online access to the *Notices* must log on to the AMS website. The login procedure was partly designed to make it more difficult to do scripted automatic downloads of *Notices* pages. They take place, or took place, in such large numbers that it was impossible to tell from Web use statistics how many individuals were really looking at *Notices* articles online. For example, pre-login, popular articles like Gerd Faltings's 1995 article about the proof of Fermat's Last Theorem might get 20,000 hits annually; last year it got 3,000, and probably many of those were pre-login January and February accesses. Post-login popular articles like Fokas and Sung's November 2005 article on generalized Fourier transforms, or Allyn Jackson's June/July 2005 interview with Martin Gardner, got about 1,000 hits in CY 2005. Of course these statistics do not include the number of potential individual users who attempted to access the articles and were confused or put off by the login requirement. (Registration was free, simple, and required only an email address.) The statistics, however, were not worth this annoyance factor, although it seems only to have affected a small number of users, and the Council, on the recommendation of the Committee on Publications, has eliminated the login requirement to access *Notices* online. So: to the webcrawlers and download scripts reading this online, welcome back.

Another purpose of the login was in part to remind *Notices* readers that the *Notices* is provided online to the entire mathematical community by the members of the AMS, as is still the case post login.

The second action taken by the Council affecting the *Notices* was a revision of the procedures for selecting the chief editor and the editorial board of the *Notices* (and the *Bulletin* as well). On the recommendation of the Long Range Planning Committee, the Council adopted a revised selection process which, in the case of the editorial boards, will be by Council action upon nomination by chief editors or chief editors-elect. The *Notices* Editorial Board plays a crucial role in this publication, and a very different one from those played by the editorial boards of the Society's research journals. Members of the *Notices* board suggest topics for articles, recruit authors for articles, provide expert advice about received articles in their mathematical specialties, and, perhaps most importantly, provide nonexpert advice as well, about articles on subjects in which they are not specialists. Obviously broad mathematical learning helps in this, but also required is a sense of what the general *Notices* reader needs from an article. As with most mathematical editorial advice, recommendations from the editorial board to authors is usually anonymous. But the *Notices* couldn't function without it. I'm glad the Council has recognized the special character of the *Notices* Editorial Board.

The third action the Council took affecting the *Notices* was to reappoint me as chief editor for a second three-year term. The final year of that term will include the fortieth anniversary of my Ph.D. Someone in a symmetric position to me would have received a doctorate in 1929. I would have regarded that as ancient history even then, as no doubt current students feel about 1969 now. Nonetheless, I hope the *Notices* can improve in providing more articles serving graduate students. We have some special features planned which we hope will address professional needs of students, as well as the mathematical needs which we hope are already being addressed.

Notices editors' terms, by the way, follow calendar years, not AMS years. This is further complicated by the fact that *Notices* cover dates, which are usually two weeks later than publication date, are ten to twelve weeks after the contents go into production. So *Notices* editor's years actually begin a third of a year before AMS years. I would like to end these light-hearted calendrical comments with a more serious one. Unlike the terms of volunteers, Math Reviews reviewers points expire on December 31 of the relevant year. These points, which can be applied to AMS dues or AMS publications purchases, are earned by writing reviews, and reviewers are always needed. Mathematicians interested in reviewing for Math Reviews, both to earn points and to serve the community, are invited to inquire at mathrev@ams.org for more information.

—Andy Magid

Letters to the Editor

Mathematics and Nobel Prizes

Professor Saari mentions the Nobel Prize in Chemistry won by John Pople (*Notices* January 2006, page 46) and says that Pople's research centered on approximating the solution of the Navier-Stokes equation. But Pople worked on approximating the solution of the Schrödinger equation, which is the fundamental PDE in quantum chemistry, rather than the Navier-Stokes equation. Professor Saari also says that "half of all Economics [Nobel Prize] winners and several more from Chemistry and Physics…used a significant amount of fairly sophisticated mathematics." But the richest source of Nobel Prizes related to mathematics is surely Physics: Einstein, Schrödinger, Dirac, Heisenberg, Glashow, Weinberg, Salam, Feynman, Schwinger, Gell-Mann, Gross, Politzer, Wilczek and many more. Moreover, Physics is dripping with mathematics (Lie algebras, PDEs, cohomology, supersymmetry, Calabi-Yau manifolds) which is more sophisticated and more interesting than anything used in Economics.

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The note by Donald Saari in the January 2006 *Notices* can be expanded a bit by including Nobel Laureates who have also done mathematics.

The most notable of these is Sir Ronald Ross (1857–1932), Nobel Prize in Physiology or Medicine in1902 for demonstrating that malaria is transmitted by mosquitoes. However, he found less support than he expected and spent much of his later life doing mathematics which he had done intermittently since his youth. Most of his work was unpublished or published either by himself or in a journal he edited. His principal interest was in iteration, both numerical iteration for solving equations and functional iteration. He also wrote some papers on epidemiology.

Another is Joshua Lederberg, who received the prize in Physiology or Medicine in 1958. His molecular studies led to a paper: "Hamilton circuits of convex trivalent polyhedra (up to 18 vertices)", *Amer. Math. Monthly* **74** (1967), 522–527.

Finally, the work of Cormack and Hounsfield in developing computer-aided tomography (= CAT scanning), which received the prize in Physiology or Medicine in 1979, was largely the numerical inversion of the Radon transform.

As Saari notes, a great many of the prize winners used substantial mathematics in their prize-winning work and other readers may suggest further examples.

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In your January issue Donald Saari lists some of the mathematicians who have won a Nobel Prize. Included in his list was John Pople whose Ph.D. was alleged to have been on PDE. That may be true but Pople, whom I knew at Cambridge in the fifties, was a theoretical chemist and the title of his Ph.D. was remarkably succint, just the one word "Water". Another Cambridge mathematician who went on to win the Nobel Prize in Economics was my former student James Mirrlees, and we should not forget the only mathematician to win the Nobel Prize for Literature—Bertrand Russell.

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The Clash of Knowledge and Certainty

Brian Davies forsees the future of mathematics when category 1 math, "theorems proved in the usual sense" is eclipsed by category 2 math "theorems perhaps proved" ("Whither Mathematics?" December 2005, *Notices*). The "perhaps" refers to proofs that are too long for any one individual to understand, and/or that rely on lengthy computer programs to check computations that have no intuitive explanation. (One example he cites is the classification of finite simple groups.) Perhaps. But I forsee two other categories of math overwhelming the first two, category 3 "conjectures supported by experimental evidence" and category 4 "conditional math", meaning theorems proved under the assumption of a category 3 conjecture. An example of category 3 is the Riemann Hypothesis, and category 4 is represented by the literature of theorems proven under the assumption of RH. I expect that as experimental mathematics grows and prospers, more and more interesting conjectures will emerge, and mathematicians will be impatient to explore their consequences.

These are changes in the practice of mathematics that I view favorably, no doubt because I am actively involved in developing them. But I can appreciate that some mathematicians may regard them as somewhat alarming. Don't they threaten to undermine the standards of rigor that the mathematical community has labored for centuries to enshrine? Perhaps. But I would rather view it as a clash between two great impulses: the quest for knowledge, and the quest for certainty. As long as these impulses pull together, everyone can be happy. But when they pull in opposite directions, as has happened before (think of the development of calculus), I believe we will be happier knowing more, with some uncertainty, rather than knowing less, with more certainty.

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Status of the Classification Proof

We do not challenge the assertion by Brian Davies [*Notices*, December 2005, "Whither Mathematics?"] that human beings (even group theorists!) are fallible, as are computers. However, the informal language of his discussion of the classification of the finite simple groups (CFSG), and his occasional misstatements of fact, could mislead readers into inaccurate conclusions. Here are some examples.

First, Davies's treatment blurs the distinction between two different works: the original proof of CFSG and the Gorenstein-Lyons-Solomon (GLS) project to provide a new proof of a large portion of CFSG. (Cf. p. 737 of Aschbacher's August 2004 *Notices* article.) The original proof was completed in 2004 with the publication of the Aschbacher-Smith proof of the Quasithin Theorem (AMS Surveys and Monographs 111/112). A very small number of constituent theorems exist only as doctoral dissertations, not published in journals. However the assertion that "the proof has never been written down in its entirety" is seriously misleading. Reference to "only about five" (actually, six) volumes being published applies to the GLS project, not to the original proof.

We object even more vigorously to the implication on p. 1355 that the proof of CFSG does not supply "understanding…in full measure". Of course "full measure" is a high standard for us mortals, but some sense of the enormous advances in the understanding of finite groups which have been achieved as by-products of the classification endeavor can be gained easily by a comparison of the post-CFSG textbooks of Aschbacher (Cambridge, 1986) and Kurzweil/Stellmacher (Springer, 2004) with the excellent pre-CFSG text by Marshall Hall Jr. (Macmillan, 1959).

Finally, we believe that perusal of Serre's interview (*Notices*, February 2004) will reveal that the concerns he expressed were in regard to the then pre-publication status of the Aschbacher-Smith monographs.

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Response by Davies

Professor Strichartz goes even further than I have in speculating about the development of mathematics in this century. We have both been influenced by our use of computers, and the new and exciting prospects that they open up for the development of the subject. One can discover many new ideas and results if one is prepared to accept a degree of insecurity, and some of them can even be proved to the traditional standards of rigour. Numerical experimentation can provide not only ideas, but also supporting evidence for the correctness of a proof, when one is able to give one. In some areas of analysis it can be one of the easiest ways of finding a mistake in a proof.

I next respond to the letter of Professor Aschbacher et al. I welcome their distinction between the original proof of the Classification theorem and the GLS project. Both of these have required brilliant insights and heroic commitment. Their statements are entirely in line with Professor Ashbacher's "The status of the classification of the finite simple groups" *Notices* Amer. Math. Soc. **51** (2004), 736–739, but differ in tone from the his more informal paper "Highly complex proofs and implications", in "The Nature of Mathematical Proof", *Phil. Trans. Roy. Soc.* A **363** (2005), 2401–2406, which formed the basis for this part of my article. On page 2402 of that article he wrote

> If we've made mistakes, so that the [Classification] theorem is false and there is some H in $C - L$, then it might be possible to repair the theorem by adding H to L and making minor modifications to the inductive 'proof'. This would be true if the structure of H is much like that of the members of L. But if H has a very different structure, one could imagine that such a modification might not be possible.

I also (deliberately) do not distinguish between a proof of the Classification theorem that at least one of the authors recognizes may be seriously flawed, and the nonexistence of a proof in the sense that many people recognize that term. I fully accept the statement of the authors that group theory has made enormous strides since 1980. The goal of my article was to point out that mathematicians will have to learn to live with such uncertainties, and that this problem will *certainly* get steadily worse in the future, whatever happens in this particular case. By the end of this century many fields will be forced to use theorems that have not been proved to the traditional standards.

I would like to correct an error brought to my attention by Professor R. L. Griess. Although several of the sporadic finite simple groups, for example, those of Lyons, J4, O'Nan and others, were first constructed using computers, Griess proved the existence of the Monster group itself entirely by hand.

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