# Pure and Applied Mathematics

Toward the end of the recent International Congress of Mathematicians in Madrid, there was a panel discussion about whether pure and applied mathematics are drifting apart. The panelists were Lennart Carleson, Ronald Coifman, Yuri Manin, Helmut Neunzert, and Peter Sarnak. John Ball, the president of the International Mathematical Union, moderated. The following reflections were inspired by that session.

I studied at the Courant Institute under Harold Grad, a distinguished applied mathematician. However, I think of myself as having one foot in pure mathematics and the other in applied.

The majority of the audience as well as the panelists were pure mathematicians. So perhaps it would be helpful to ask, What is applied mathematics? A very good answer was provided by the late Kurt Friedrichs, who distinguished himself in both pure and applied mathematics. He used to say, "Applied mathematics consists in solving exact problems approximately and approximate problems exactly." Initial and boundary value problems associated with the Navier-Stokes equations are an example of problems that are extremely difficult to solve exactly and where we look for approximate solutions. Hence computing is an important part of applied mathematics. The Bhatnagar-Gross-Krook equation in kinetic theory and plasma physics is an example of a solvable problem that approximates an intractable one.

Apropos of definitions, Peter Lax remarked once, quoting Joe Keller, that pure mathematics is a branch of applied mathematics; this was echoed by one of the ICM panelists. Some of the greatest mathematicians of the past—Newton, Euler, Lagrange, Gauss, and Riemann—and more recently Hilbert, Weyl, Wiener, von Neumann, and Kolmogorov did both pure and applied mathematics. We are of this world, and nothing comes entirely from within ourselves without reference to the external world.

Sarnak said that proofs are essential in pure math, but they are essential in applied math too, except that the path one takes is rather different. As Carleson said, applied math relies heavily on the inductive method, as opposed to the deductive method preferred by pure mathematicians. In pure mathematics the emphasis is on rigor. However, ideas are far more important. Ideas come from intuition, of course, which in turn comes from living and breathing the subject. Sarnak and his colleague Weinan E are quite right in insisting that even applied mathematicians need basic training in mathematics—roughly what is taught in good North American graduate schools in the first two years of study—but that is not enough. As Grad used to say, one must also immerse oneself completely in the subject to which one wants to apply mathematics. For instance, since he worked in magneto-fluid dynamics, to stay in touch with the experimentalists he regularly visited Oak Ridge and Los Alamos. It is by gaining a thorough understanding of the problems arising in the subject proper that one develops a feeling for it, and with it, intuition. So, as Coifman said, in subjects like biology and informatics we are in a pre-Newtonian age.

In applied mathematics the emphasis on rigor and proof must come at the appropriate stage. Let us consider an example. Feynman had great intuition but didn't care much for rigor or proofs. He says in one of his autobiographical writings that while he was at Cornell he used to talk to William Feller and Mark Kac, the famous probabilists. It is a happy circumstance, for science in general and mathematics in particular, that Feller and Kac didn't dismiss Feynman as a sloppy crackpot but instead patiently listened to him. Thus was born the great Feynman-Kac formula. The moral, I think, is that pure mathematicians, while insisting correctly on rigor and proofs, must be patient and show some respect toward intuition born out of a deep knowledge of a subject. Attitudes like that of the late Paul Halmos— "Applied mathematics is bad mathematics" —are shortsighted. For their part, applied mathematicians, while using intuition as their guide, must recognize the need for and the importance of proofs. On the other hand, it is rare that a single individual embodies all the requisite qualities to a high degree. So often what is needed is a joining of hands of people with disparate abilities, strengths, and points of view rather than a separation or drifting apart.

It is interesting to note that one doesn't always know whether one's work is useful or not. For example, G. H. Hardy is considered the quintessential pure mathematician. However, Joseph and Maria Mayer (she won the Nobel Prize in physics) use, in their classic book on statistical mechanics, a result of Hardy and Ramanujan in the evaluation of the partition function of an imperfect gas. Some say that Hardy's distaste for applications and the pride he took in the uselessness of his work had roots in his pacifism. The use of poison gas in the First World War, which he regarded as an application of chemistry, appalled him and may have had the unfortunate effect of turning him against applications altogether. One cannot but admire such loftiness, however.

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# Letters to the Editor

## Retarded Differential Equations and Quantum Mechanics

G. W. Johnson and I wish to draw attention to the work of C. K. Raju that is related to some of the ideas discussed by Sir Michael Atiyah in his talk "The Nature of Space", which we reported on in the June/July 2006 issue of the *Notices*. Ideas suggesting a link between retarded differential equations and quantum mechanics were put forward some years ago by Raju, and we, along with Atiyah, believe they deserve attention. Interested readers are encouraged to read, in particular, the following papers written by Raju:

1. *Time: Towards a Consistent Theory*, Kluwer Academic, Dordrecht, 1994 (Fundamental Theories of Physics, vol. 65), ch. 5b "Electromagnetic time" (pp. 116–122), and ch. 6b "Quantum mechanical time" (pp. 161–189).
2. *The Eleven Pictures of Time*, Sage, 2003, pp. 298–302.
3. "The electrodynamic 2-body problem and the origin of quantum mechanics", *Foundations of Physics*, **34**, (June 2004), 937–962.

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## The Proof of the Poincaré Conjecture

In recent months there has been considerable attention devoted to the proof of the Poincaré conjecture that was started by Hamilton and completed by Perelman, both among mathematicians and in the media. Although most of the reports are largely true to fact, some incorporate false statements or innuendo which I believe are irresponsible. I am writing this letter to set the record straight.

Let me say first that the Hamilton-Perelman proof of the Poincaré conjecture is a great triumph for mathematics in general and for geometric analysis in particular. I am privileged to have participated in the nurturing of geometric analysis from its infancy to its adulthood. There were the good old days when ideas were shared and new frontiers were explored. It was during this period that the Ricci flow was introduced and investigated by Hamilton. Thirty years later, geometric analysis has reached maturity, and the proof of the Poincaré conjecture is perhaps its most spectacular success to date. I expect many more successes to come.

The achievements of Hamilton and Perelman in solving the conjecture, especially their major breakthroughs on singularities of nonlinear parabolic systems and the structure theorem for 3-dimensional manifolds, are unparalleled. They far exceed the established standards for Fields Medals. I fully support, and have always said so, the award of the Fields Medal to Perelman. (In my view, Hamilton clearly deserves the Fields Medal also, but he is not eligible at this time because of the age restriction.) For anyone to suggest in words or a cartoon that my position has ever been anything but that is both offensive and completely untrue.

Proving the Poincaré conjecture is an intricate and daunting process. In a work of this scale, it is understandable that when Perelman released his manuscripts on arxiv.org, several key steps were merely sketched or outlined. These manuscripts posed a tremendous challenge for the math community to digest. For two years many top experts in the field of geometric analysis worked hard and made steady progress in understanding and clarifying Perelman's papers. At the end of 2005, Cao and Zhu completed a three-hundred-plus-pages manuscript that provided a complete account of the Hamilton-Perelman proof of the Poincaré conjecture. This paper provides the proof in a form that finally can be understood by researchers in the field.

This past summer while I was in China, I held a press conference and also gave a public lecture on the Poincaré conjecture. My press conference addressed a group of Chinese reporters. Its intention was to encourage young Chinese mathematicians and scientists to be more ambitious and seek the frontiers of research being done worldwide and not just in China. Young mathematicians in China need not just encouragement but a better perspective of what the most exciting and promising directions of research are. My public lecture in Beijing on June 20 [2006] was addressed to the mathematics community and a large group of string theorists. In that talk I focused on the achievements of Hamilton and Perelman. Since Cao and Zhu managed to put together in writing the details of the deep ideas of Hamilton and Perelman, I praised them as well, hoping this would encourage their fellow mathematicians in China.

Over the years I have inherited from my teacher S. S. Chern the strong belief that it is the duty of any mature mathematician to train the next generation. Since he and I both come from China and there are many talented young Chinese mathematicians who are not exposed to modern mathematics, we have spent a lot of time helping mathematicians and students in China. We devoted a lot of time discussing the challenges and working together to address them. Thanks to his leadership, there are now many outstanding Chinese-trained mathematicians in Western universities. Over the last twenty years, Chern and I have also been trying to develop mathematics within Chinese universities. Because of the Cultural Revolution, the recovery has been slow. But thanks also to the help of many friends from the West, the situation is improving.

There have been uninformed reports on how the Cao-Zhu paper was handled by the *Asian Journal of Mathematics*, as well as on the joint work by Lian, Liu, and me on the mirror symmetry conjecture. Regarding the former, rumor has it that the normal peer review process had been tossed out the window. On the contrary, it took the journal several months to go through the established process until the paper was accepted for publication. After receiving the submission in December 2005, I asked, without success, several leading experts on geometric flows, including Perelman, if they would referee the paper. Under the circumstances, I myself took on the referee's task. After attending more than sixty hours of lectures by Zhu and numerous hours of studying the paper on my own, I convinced myself that the paper was correct. This was in April 2006, and only then did I recommend the publication of the paper to the whole editorial board. The paper was then accepted according to the standard editorial procedure of the journal, by which acceptance was automatic unless an objection was voiced within a few days of the chief editor's recommendation.

I must add that this procedure of the *Asian Journal of Mathematics* of requiring consent from the whole editorial board is more stringent than several leading mathematical journals, where the chief editor would consult only a few members closest to the subject of the submitted paper. It is also a common practice for editors to expedite the reviewing process for important solicited papers.

Regarding my joint work with Lian and Liu, some reports have been particularly incomplete, biased, and unfair. This is not the place for me to respond to these false allegations, but I urge anyone else interested to look up the responses of Lian and Liu (B. Lian and K. Liu, On the Mirror Conjecture, `http://www.doctoryau.com`), which contain an account of the mathematics and history surrounding this conjecture.

In an age of instantaneous communication, the solution of a great problem like the Poincaré conjecture would inevitably draw attention from the media, with some reports more meritorious than others. Regardless of what has been said, however, what we in the mathematics community can cherish is the good fortune of having borne witness to this historic achievement of Hamilton and Perelman.

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## Letters of Recommendation Terminology

From the chair of a hiring committee to everyone who writes letters for job candidates: Recommending someone "in the strongest possible terms" has become a cliché. I urge you in the strongest possible terms to find other ways of expressing your enthusiasm.

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### Submitting Letters to the Editor

The *Notices* invites readers to submit letters and opinion pieces on topics related to mathematics. Electronic submissions are preferred (`notices-letters@ams.org`); see the masthead for postal mail addresses. Opinion pieces are usually one printed page in length (about 800 words). Letters are normally less than one page long, and shorter letters are preferred.

### Identifications

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