Commentary

In My Opinion

Hazardous Mathematical Platitudes

When we mathematicians say something is known, we are on the epistemological high ground. No other aspect of human knowing is as sure and as reproducible as a piece of valid mathematics. When we venture into nonmathematical issues, however, we have to live in the same world of rhetoric, argument, and contingent fact as the rest of the species. In fact, our mathematical training may put us at a disadvantage.

Consider, for example, how the following mathematical principles could be liabilities:

- "Truth is definitive": In mathematics if the proof is valid, the result must be accepted. Is this what leads us to believe that if only we could find the right argument, the proper reasoning chain, the logically compelling way to phrase our points, then the public would finally appreciate the value of our discipline and its practitioners? (And perhaps express its appreciation by better funding our research?) When he addressed the Society at its meeting in San Diego in January 1997, Congressman George Brown urged mathematicians, along with the rest of the scientific community, to make its case in political terms, by which I think he means to be compelling in ways other than just by the logical force of its positions. The success of the "Unified Statement on Research", endorsed by the presidents of over one hundred scientific societies and in which the AMS played a leadership role, is an example.

- "If it's not completely correct, it's all wrong": While there are correctable errors in proposed proofs, there's no such thing as a partly correct mathematical proof. Is that what leads us to try to apply the same principle to expressions of opinion? Errors and mistakes, including willful ones, can creep into written commentary, even when subjected to the careful editorial review of these *Notices*. Does a minor misstatement of fact invalidate the position taken by the author of an opinion piece? I'd say no, but letters to the editor received by the *Notices* suggest this is not a universally held view.

- "An example is not a proof": Of course it isn't, but sometimes it's the best that can be done. In areas where general principles are not universally applicable or where experimentation must be tempered by ethical considerations, like medicine or pedagogy, careful scrutiny of examples ("case studies") or collections of examples ("case series") is a standard research methodology. This is not to be confused with using narrative (including fiction) as a rhetorical device ("anecdotal evidence"). I wonder whether the slow diffusion of the results of education research through the mathematics community may be due in part to a prejudice against the case study method.

- "The mathematics is the only acceptable measure of the mathematician": Even if we occasionally fail to live up to our ideals, belief in a strict meritocracy seems to me central to the organization of mathematics. Most mathematicians seem to have a reasonable sense of how their mathematics rates and in their mathematical relationships defer or presume accordingly. Not unexpectedly, some mathematicians carry this forward into nonmathematical issues as well. It is possible for excellent mathematicians to produce poor commentary, and vice versa, as I learned during the few months I edited the *Notices* last year. And both those who presume that their mathematics entitles them to privileges and those who defer to others on mathematical bases regarding nonmathematical issues are at fault here.

- "Mathematicians are masters of logical thinking": Our tendency as mathematicians, whose view of ourselves as reasoners is central to our self-image, is sometimes to perceive attempts by holders of opinions opposed to our own to justify their positions as attempts to out-reason us, and hence as attacks on our professional identities. The advent of e-mail and the ease (trumping the etiquette) of cc'ing third parties mean that many more of us have looked in on nasty debates over silly points by otherwise sensible colleagues; I'd attribute some of this to that tendency.

So with mathematical truisms exposed as weaknesses in the nonmathematical world, what would I have us do? Have no opinions, or, if we do, keep them to ourselves? Of course not. But I'd like us to keep in mind regarding commentary, especially commentary that finds its way into print, for instance in the *Notices*, or in criticism thereof, that mathematical habits of thought are not always what is needed or wanted. Mathematicians, in my opinion, can be as persuasive, and collegial, as anyone else.

—Andy Magid
Associate Editor

Letters

Origin of the KdV Equation

As Richard Palais intimated in his fine article on "The symmetries of solitons" (*Bulletin*, October 1997), any story about such a wide-ranging subject must select and simplify. Perhaps, though, readers may find these few historical clarifications concerning the early controversy over the solitary wave and the origin of the Korteweg-de Vries equation to be of interest.

The aspects of John Scott Russell's 1844 report concerning his "Wave of Translation" certainly generated mathematical controversy, but for reasons slightly different from those suggested; R. K. Bullough's article in the collection *Solitons* (M. Lakshmanan, ed., Springer, 1988) discusses the situation in depth. On the one hand, it was well known that d'Alembert's solution of the wave equation gave traveling-wave solutions with arbitrary waveform. So it seemed to Airy in 1845 that no particular wave shape should be exceptional in the sense that Russell claimed. On the other hand, Airy also developed a theory that takes nonlinear effects into account (especially the variable depth of water under a wave), which showed that any single-wave solution must steepen. Stokes argued in 1847 that a permanent solitary wave was impossible for a different reason. He computed that nonperiodic traveling waves were impossible if one took account of dispersion. (The effect of dispersion on water waves is pronounced: short waves travel much slower than long waves, as is readily observed.)

It was nearly thirty years before Boussinesq in 1872, and then Rayleigh in 1876, resolved the issue by balancing the effects of dispersion and nonlinearity to produce a solitary wave of permanent form with a particular shape. (It is probably fair to say, though, that it is not well understood even today why this delicate balance should produce a *stable* waveform.)

Boussinesq wrote four works which contain treatments of the problem of the solitary wave. (For bibliographic details, see the historical article by J. W. Miles, *J. Fluid Mech.* 106 (1981), 131–147.) These works can hardly be said to be models of clear exposition or consistency. But while tracing the origins of various "Boussinesq equations", I noticed that the *KdV equation* appears in a footnote on page 360 of Boussinesq's massive 680-page memoir, *Essai sur la théorie des eaux courantes*, which was presented to the French Academy in 1872 and finally appeared in 1877. More careful consideration revealed that Boussinesq based his description of the solitary wave, and his explanation for its stability, on a pair of equations exactly equivalent to the KdV equation, written in his notation as

$$\frac{dh}{dt} + \frac{d.h\omega}{dx} = 0,$$
$$\frac{\omega}{\sqrt{gH}} = 1 + \frac{3h}{4H} + \frac{H^2}{6h}\frac{d^2h}{dx^2}.$$

Here h represents the elevation of the wave, H is constant and represents the depth of the fluid at infinity, and g is the gravitational constant. These are equations (5a) and (7a) of Boussinesq's second 1871 *Comptes Rendus* article, equations (29) and (34) of his 1872 article in *J. Math. Pures Appl.*, and equations (283) and (291) of the memoir. The solitary waves obtained by Boussinesq in these works were exactly traveling-wave solutions of this pair of equations, obtained by requiring that ω be constant.

In addition, Boussinesq's rationale for the stability of solitary waves has had a direct influence on modern developments. T. B. Benjamin credited Boussinesq for the idea that a certain conserved functional, called the "moment of instability" by Boussinesq, is relevant for understanding the stability of solitary waves. This functional is now known as a Hamiltonian energy for the KdV equation. One hundred years after Boussinesq introduced this quantity, Benjamin and Bona used it as a Lyapunov functional to construct a rigorous proof of orbital stability for KdV solitons. Boussinesq's argument that the moment of instability is constant in time rests exactly on the pair of equations above.

It is not clear why Korteweg and de Vries thought the permanence of the solitary wave still controversial in 1895, but perhaps they were not aware of three of Boussinesq's works on the subject, since they refer only to his first 1871 *Comptes Rendus* article, which sketches a different, less satisfactory treatment of the problem.

Miles's article mentioned above appears to be the only modern source (among several historical papers) to properly appreciate Boussinesq's work in this respect, and indeed it contains quite a thorough account, except that it does not mention the 1877 footnote. Maybe for this reason Miles came up shy of pressing Boussinesq's priority in deriving the KdV equation.

Robert Pego
University of Maryland, College Park

(Received December 1, 1997)

Use Convergence to Teach Continuity

In reaction to the January 1998 issue of these *Notices*, page 6, I would like to submit the following.

There is hardly a better way to explain, to define, or to teach the notion of continuity (especially in calculus) other than by saying that "A function f is continuous at c if and only if whenever a sequence $c(1), c(2), c(3), \ldots$ converges to c, then the corresponding sequence $f(c(1)), f(c(2)), f(c(3)), \ldots$ converges to $f(c)$."

Notice also that the notion of "convergence" is much more intuitive than any of the statements such as "the closer x gets to ...", etc.

Most important is that the above sequential definition of continuity is equivalent to the universally accepted "epsilon, delta" definition of continuity. The equivalence uses a very mild version of the Axiom of Choice AC (for which one need not feel apprehensive).

In this connection I would like to note that AC is one of the extremely natural axioms of the standard ZFC set theory. AC, besides being consistent with ZF, is also almost inextricably related to the other axioms of ZF. For more than seventy years before

1963 mathematicians could not construct a model for standard ZF set theory where AC was not stubbornly present and hence valid. No matter how hard they tried, they could not expunge AC from any standard model of ZF, so intimately is AC bound to the other axioms of ZF. The main reason is, of course, that "well ordering things" is almost a way of life in mathematics. Set-theoretical models are usually created as well-ordered sequences of shelves, and on each shelf objects are placed in a well-ordered sequence. Thus, well ordering is mostly built in (in a natural way) in any standard set-theoretical model, and AC was inevitably valid in all of them up until 1963. It was P. J.Cohen's genius which finally in 1963 created a standard model for ZF, extricating AC from it (same thing can be said in connection with the Continuum Hypothesis CH).

Alexander Abian
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(Received December 10, 1997)

Beal Conjecture and Prize

I am writing to update the announcement in the December 1997 issue of the *Notices* of the Beal Conjecture and Prize. Let me report first that it has now come to my attention that the conjecture is stated and discussed in van der Poorten's recent book *Notes on Fermat's Last Theorem*. The problem is also discussed in a March 1997 lecture and paper of Darmon, available from him as a PostScript file. In view of this, I realize now that I should have included more of the story of how and when Beal arrived at his conjecture. Let me also state that although my purpose in writing the original notice was not to give a comprehensive survey of the ideas surrounding the problem (which is beyond me) but simply to report on Beal's conjecture and prize; any essential omissions or oversights in the article are my own responsibility. Thus, I am writing to provide some background about the genesis of Beal's conjecture, to report a simplification of the prize, and to announce a Web site about the prize.

In the summer of 1993 Beal, inspired by hearing about Wiles's stunning achievement, began thinking about Fermat's Last Theorem. From his viewpoint he discovered that there seemed to be a more general relationship at work, which he formulated as his conjecture. Beal mulled over the problem himself. During August 1993 Beal hired an independent contractor, James Wilhelmi, to conduct computer searches for counterexamples. The bank's computers were turned over to this search at night and on weekends. With no counterexamples in sight, Beal became even more convinced that his conjecture was indeed correct. Over the next several months, in his spare time, he tried to prove it. During the summer and fall of 1994 Beal wrote to perhaps fifteen or twenty mathematicians and journals informing them of his conjecture. Some of his choices were very good, whereas others could be expected to be nonresponsive.

Harold Edwards responded in September 1994. He suspected there might be counterexamples and suggested that Beal have someone do a simple computer study which would perhaps reveal them. Beal had also written to Earl Taft as editor of *Communications in Algebra* about his conjecture. Taft had sent it to someone (an anonymous expert) who said they had never heard of the problem, mentioned its relation to the ABC conjecture, and also thought there might be counterexamples.

In the fall of 1995 Beal came to North Texas as a guest of the administration and soon began meeting with some of us here to discuss mathematics. He told us about his conjecture. I thought it seemed interesting, and eventually he proposed to offer a prize for its solution. This culminated with the announcement in the *Notices.*

Since the prize was announced in the *Notices*, Beal has simplified the prize at a fixed \$50,000. Thus, the prize beginning December 1, 1997, is \$50,000 for either a counterexample or a proof. In the case of a proof, the prize will be awarded when the paper has been accepted in a (reputable) standard mathematics journal and also, in the eyes of the committee, when the proof has been accepted as correct by the mathematics community.

Inquiries about the details of the prize may be sent to me via e-mail: `mauldin@dynamics.math.unt.edu` or by regular mail. There is also a Web site: `http://www.math.unt.edu/~mauldin/beal.html`.

R. Daniel Mauldin
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(Received December 11, 1997)

Mathematics Communication in the 21st Century

The last two letters to the editor in the January *Notices* are disturbing. I hate to think of the AMS stepping across the millennium threshold worrying about "typists" and "overlays".

The underlying issue in both letters is the communication of mathematics. The questions we need to address are:

1. What sort of electronic translation services should a mathematics department provide? The Mathematical Markup Language (MathML) standards are nearing completion, as are various automated translation programs. Mathematica, for example, can import and export to a variety of print and electronic formats. To what extent could the AMS help by setting up a Web site that would automatically translate, say, $\mathcal{A}_{\mathcal{M}}\mathcal{S}$-LaTeX to MathML? Should department librarians be expected to purchase scanning software that will translate archival printed documents into MathML, Mathematica, or other systems of choice?

2. What sort of electronic communication systems should the AMS provide at meetings? Wireless communication systems are becoming standard features on notebook computers. AMS meetings could include computer servers with public directories on which conference attendees could post electronic documents. Should we also expect meeting rooms with projection systems connected to the Web?

I'll accept that there is a certain amount of "audiovisual tradecraft" associated with giving a good presentation. The University of Malta has an excellent Web page on details of using the overhead projector. The URL is `http://www.ilands.com/education2000/08.htm`.

David Fowler
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(Received December 12, 1997)

Objects to Universities Run as Businesses

It is common nowadays for university administrators to describe their university as a "business", whose "customers" or "clients" are the students. I have at least four objections to make to this outlook.

First, we should make it clear what it is we are selling. If we are selling degrees, we could certainly streamline the operation. No faculty or classes would be needed; students could mail in four years of tuition, and administrators could mail them whatever degree they have paid for.

I hope it is not necessary to explain why selling degrees or grades is reprehensible. Yet we may not be far from this vision of the university; an article on grade inflation in the July 25, 1997, *Chronicle of Higher Education* quoted a recent University of Chicago Ph.D. as saying that professors would raise the grade he had recommended (as a teaching assistant) because "Hey, they're paying $125,000; we ought to give them a good grade."

Second, with the exception of those few truly private—in the sense of receiving no state or federal aid—universities, our customers are not just our students but all of American society. Especially in math, giving the majority of students what they want—an effortless high grade and the illusion of learning—is disastrous in terms of giving society what it needs. What society needs from us more than anything is the identification of people incompetent to hold positions of intellectual responsibility. There is nothing more dangerous to society than ignorant people who believe they are knowledegable and have been falsely identified as being competent; it's like being driven in a bus into a chasm because the driver believes there is a bridge.

What our real customers, the public, want from math departments is teaching, especially the service classes. The extent to which administrators are willing to serve these customers can be measured by the salaries they pay part-time instructors and teaching assistants, whom they increasingly rely on to teach service classes.

Third, if universities really are going to be capitalists first, to avoid the excesses of laissez-faire capitalism, we need the equivalent of the Food and Drug Administration. In particular, we need some explicit truth-in-advertising laws. I know of a master's degree in math that requires no thesis or qualifying exam and may be acquired entirely by taking undergraduate classes, including ones equivalent to sophomore-level classes for engineering majors. Society is paying for a T-bone steak and getting hamburger.

Fourth, I don't believe university adminstrators really want to behave like businesspeople in any constructive way. In business the phrase "top-heavy with administration" is pejorative. The ideal university as apparently envisioned by the average university administrator is top-heavy, bottom-heavy, and middle-heavy with administration. The term "service", which almost invariably means administration, occupies the same moral position in universities that "charity" does in the outside world: ten minutes of "service" per week is morally superior to any amount or quality of teaching and research. If Jaime Escalante were performing his miracles at an average American university, the response of administrators would most likely be "Yes, but what committees have you served on recently?"

We are not a business and we shouldn't be. We are being trusted with a good deal of money and authority, with very little specific accountability: faculty must show up to give some kind of lecture a few hours each week, and administrators must optimize managerial parameters while utilizing careful scrutiny of matters that have come to their attention. It would be a betrayal to twist that trust into profit making. If we really want to be a business and be honest about it, we should renounce all government aid and submit to persistent governmental inspections and evaluations, beginning with proficiency exams for all our degrees. I hope we are not irresponsible enough to make this necessary.

Ralph deLaubenfels
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(Received December 12, 1997)

About the Cover

This month's cover is a joint effort with Robin Wilson, who writes the "Stamp Corner" for the *Mathematics Intelligencer*. The mathematicians portrayed in this collage of stamps are (clockwise from upper right corner) LaGrange, Pascal, Kovalevskaya, al-Khuwarizmi, Dedekind, Newton, Cauchy, Ramanujan, and Bolzano.