Letters to the Editor

Just What You Wanted for Christmas

One of the ways by which the National Academy of Science (NAS), the National Academy of Engineering (NAE), and the Institute of Medicine (IM) help set public policy is through reports by the Committee on Science, Engineering and Public Policy (COSEPUP). COSEPUP is a joint committee of NAS, NAE, and IM and includes members of their councils. The COSEPUP report *Science, Technology and the Federal Government*[1] was produced under the auspices of the National Research Council and sets out national goals for science as well as a new framework for funding[2] based upon these goals.

As summarized on page 29 of the report, the goals are:

- The United States should be among the world leaders in all major areas of science.
- The United States should maintain clear leadership in some major areas of science.
- The comparative performance of U.S. research in a major field would be assessed by independent panels of experts from within and outside the field.
- The implementation of these goals for science requires more coherent federal budgetary procedures.

The first two goals distinguish between major areas of science where the United States should be *among the world leaders* and some major areas where the United States should *maintain clear leadership*. The report recommends that federal funding be guided by this distinction and sets forth criteria which call for clear leadership in a field.[3] Two of the criteria are:

- The field is demonstrably and tightly coupled to national objectives that can be met only if U.S. research performers are clear leaders.
- The field affects other areas of science disproportionately and therefore has a multiplicative effect on other scientific advances, especially those where clear leadership is the objective.

There is a case that mathematical research is not a major area of science in which the United States should maintain clear leadership. The argument is plainly stated by John Hopcroft,[4] a member of the National Science Board, which sets NSF policy.

> Pick the field of mathematics and say, "How would we decide what funding level is appropriate?" Well, the first thing you have to do is decide which of the two categories math falls into. Are we just going to be world-class in that area, or does it meet a condition so that we have to strive to be number one? Your first response might be: "Well, mathematics surely is an enabling science. All sciences depend on mathematics, and therefore we ought to be number one." But if you think a little bit more about it, if you look

[1] Science, Technology and the Federal Government: National Goals for a New Era, *by the Committee on Science, Engineering and Public Policy, chaired by Phillip Griffiths, National Academy Press, Washington, DC, 1993.*

[2] *Ibid, p. 24.*

[3] *Ibid, p. 20.*

[4] *John Hopcroft is dean of engineering at Cornell University. He spoke at the Fifth Academic Leadership Series at Robert Purcell Community Center, December 11, 1995. The quoted remarks are from his speech. A videotape of the Fifth Academic Leadership Series is available from Cornell University.*

> at Ph.D.-level mathematics today, you might say, "How many other fields of science really depend on that? Maybe in this area we only have to allocate enough funds to be world-class, not to be number one." And I believe that is what is indeed happening today.
>
> However, if you look at K–12 mathematics, you would discover that that meets two of the conditions on the list. It's certainly enabling. If we don't have our students coming out of high school with a strong background in mathematics, we're not going to have scientists of the future. And second, there's probably a clear economic need, because we need a trained work force, and so forth. That is the reason why the federal government gave NSF, a few years ago, 500 million dollars it didn't ask for for K–12 math and science education.

I feel that the mathematics research community has not adequately addressed *Science, Technology and the Federal Government* in the public arena.

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Computer Technology, the *Standards*, and Reform

I agree with Professor Wu's thoughtful "Forum" article criticizing aspects of the reform movement and in particular the NCTM *Standards (Curriculum and Evaluation Standards for School Mathematics*, NCTM, Reston, VA, 1989). The nation was ill served by the publication of these "Standards". While the *Standards* talk about problem solving (*Standards*, p. 4, no. 2), the *Standards* recommend that "decreased attention" be paid to "word problems by type", explicitly mentioning work (*Standards*, p. 127), but presumably targeting problems of motion, mixture, and investment as well.

The *Standards* speak of "removing the 'computational gate' to the study of high school mathematics" (*Standards*, p. 130). On the elementary level, skills in basic arithmetic are ridiculed as "'shopkeeper' arithmetic skills" (*Standards*, p. 3). In keeping with this view, the *Standards* would downplay (*Standards*, p. 21) or abolish (*Standards*, p. 8) the teaching of long division. Along with long division, "Long division without remainders" is also to be deemphasized (*Standards*, p. 21).

The *Standards* correctly observe that computer technology has great potential for classroom use. But at the same time, the *Standards* show a propensity for applications utilizing computer technology, while dispensing with important algebraic underpinnings (*Standards*, p. 148 and p. 150). The approach throughout the *Standards* is towards superficial exposure to mathematical topics, new instructional methods (which have their place), exploration (certainly welcome), increased emphasis on formulation and verification of conjectures (also welcome), together with minimal demands for mastery.

The approach is in keeping with the "call for liberating mathematics from the clutches of skill" made by Professor Gerald Rising in his article "Which Way Mathematics Education?" [*New York State Mathematics Teachers' Journal*, vol. 28, no. 1, Winter 1977–78, p. 16]. Professor Rising was a member of one of the "working groups" which assembled the *Standards* (*Standards*, p. iii and p. v). If anyone has any doubt that mathematics educators ran the show, I would encourage that person to read the comment on page 248 (near the *end* of the *Standards*) where we are told: "A mathematics-educator will bring a knowledge of mathematics and instruction" to the evaluation process.

While mathematics educators should have some role in the development of a curriculum for school mathematics, it is essential that mathematically well-trained high school teachers and several seasoned professors of pure and applied mathematics take an active role in the development of such a curriculum. There should be adequate testing and widespread consultation before any such curriculum receives endorsement from the MAA or AMS.

As a college teacher I dislike the lax preparation for serious college-level mathematics that the NCTM *Standard*s promote, and as a parent I am aghast that some of this nonsense has even infected the parochial school our youngest daughter attends.

I certainly do not oppose the intelligent use of technology in the mathematics classroom. But when the use of technology supplants the acquisition of the basic skills necessary to intelligently use that technology or to master serious mathematics, especially calculus, one runs the risk of creating a new learning disability: Computer-Assisted Mathematical Incompetence.

I believe the NCTM *Standards* have already significantly harmed our national scientific, engineering, and business infrastructure. And the well-financed assault of the reform movement would likely continue unabated were it not for outspoken math-

About the Cover

The figures depict side and top views of a foliation of $\mathbb{R}\mathbf{P}^3$ by circles. More precisely, several left cosets in $SO(3)$ of the isotropy group H of $(0,0,1)^{\mathrm{T}}$ are displayed. The fibers lie in the ball with center $(0,0,0)$ and radius π, with antipodal points on the boundary identified. (The colors on the fibers indicate distance from the origin; the dots indicate intersection with the plane $y = 0$.) Note that the fibration $\mathbf{S}^1 \to \mathbb{R}\mathbf{P}^3 \to \mathbf{S}^2$ depicted is double covered by the Hopf fibration $\mathbf{S}^1 \to \mathbf{S}^3 \to \mathbf{S}^2$. The detailed structure present in the figure (such as the overall z-axis symmetry, the fibers lying in planes, and the double cosets $H\backslash SO(3)/H$ comprising nested tori) is explored in detail in the article by Rick Kreminski in *Mathematica in Education and Research*, vol. 6, no. 1, TELOS/Springer-Verlag (WWW: `http://www.telospub.com`).

ematicians like Professors Wu and George Andrews. As with any movement, not everything in it is bad. There is perhaps a new enthusiasm for teaching in the reform movement and, when properly directed, enthusiasm is certainly good. But on balance the *Standards* are bad news for mathematics and disciplines which require serious mathematics.

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(The author wishes to thank his student, Brian Lombardo, for making some useful suggestions at the galley stage.)

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Researchers and Precollege Education

Susan Landau's otherwise excellent editorial (*Notices* **44** [February, 1997], p. 188) omitted one of the most important ways that mathematicians can be socially responsible—by working to improve precollege math education. More research mathematicians than ever before have become interested in contributing to society in this way. For example, K–12 education was a major theme at the recent "Symposium on the Future of Math Education at Research Universities", held in Berkeley at M.S.R.I. on December 5–6, 1996.

In order to contribute to improving math education in the broad sense, one needs to understand what goes on in a typical classroom. This means visiting and observing average public school classes. Work in elite schools and in special programs for selected youngsters may be helpful in producing the next generation of research mathematicians, but it has nothing at all to do with math education for the masses.

As always when embarking on something new, it is wise to approach work in the schools with a certain amount of caution and humility. One should avoid messianic delusions. The reasons for the poor state of math education in the U.S. are complex, multifaceted, and deep-rooted. There is no quick fix, and the view that any of us can singlehandedly turn things around is naive and unproductive.

My own work with the schools has been on a very small scale. I teach a course for math education majors that includes weekly visits to an inner-city middle school, where my students and I present nontraditional enrichment topics in geometry, statistics, and discrete math to four sixth-grade classes. I do not know whether or not we are having a measurable impact. But what I do know is that my university students and I find these visits to be tremendously stimulating and enlightening.

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Mathematical Research and Education

While generally agreeing with Sherman Stein's letter in the March 1997 issue about the need for mathematics departments to involve themselves seriously in the preparation of teachers, I would like to make a few comments.

Too many of us believe that, as Stein writes, "Someone who wants to do a good job in mathematics education will not have the time or energy to continue mathematics research." This is demonstrably false, and it is not in the interest of our community to continue believing it. While some institutional accommodations may have to be made—a lighter service load or an occasional course reduction—this is no big deal. The real problem is not to sustain both education and research, but to either find money for projects or to invent reasonable projects that do not need extra funding.

It is not true that *Mathematics Teacher* has had no articles on teacher development. The "Projects" section has had frequent articles on teacher development as well as on curriculum and materials development projects.[1]

[1] *In particular, there have been articles on projects involving research mathematicians.*

And while many of us might devoutly wish that better professional development had preceded curriculum reform, the way in which professional development is funded (see above paragraph) is the real problem here: simply put, there isn't nearly enough money to do what needs to be done, with or without curriculum reform.

Finally, the NCTM *Standards* are being revised, possibly extensively. This is a three-year process, just begun this academic year, in which all of the professional societies, including the AMS, have considerable input through advisory committees.

Judy Roitman
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Science Wars or Wars of Derision?

The October issue of the *Notices* having recently made it to France, along with news of the "Sokal affair", I was surprised to learn that the AMS has decided to open its pages to what some call the "Science Wars". Evans Harrell's enthusiastic "report" in the October issue on the book *Higher Superstition* (*HS*, in what follows), concludes by assuring us that the book's authors, Paul R. Gross and Norman Levitt, "have done us all a great favor." But Harrell is short on actual quotations from *HS*. *Notices* readers who assume Harrell's civility and amused tolerance reflects the tone of the Gross-Levitt book may be surprised to find in the latter such lines as

> …sexist discrimination, while certainly not vanished into history, is largely vestigial in the universities; that the only widespread, *obvious* discrimination today is against white males. (*HS*, p. 110).
>
> Among all the pressing possibilities for a searching-out of error in important and problematic sci-

> ence … the most examined case of distorting male contextual values is that perennial feminist whipping boy, biological and behavioral differences between the sexes. (*HS*, p. 145)

> If…the humanities department of MIT … were to walk out in a huff, the scientific faculty could, at need and with enough released time, patch together a humanities curriculum, to be taught by the scientists themselves … on the whole it would be, we imagine, no worse than operative. (*HS*, p. 243)

I, for one, find such talk offensive, but some might object that the lines quoted above are incidental to the main argument of the book and should not be taken out of context. The easy retort is that the Gross-Levitt attack on "Science Studies" also operates largely by collecting outrageous quotations, often accompanied by comments that distort their authors' intentions. Several examples are discussed in Roger Hart's review of *HS* in the volume *Science Wars*, an expanded version of the issue of *Social Text* examined by Michael Sullivan in the October issue. Sullivan's otherwise thoughtful article makes the same mistake by reducing Sarah Franklin's contribution to *Social Text* to her (admittedly ill-considered) comparison of Gross and Levitt to Operation Rescue. Scientists willing to make the effort to read past the conventions of an alien discipline might find Franklin's article illuminating, and even convincing, as an expression of what Science Studies might mean to its practitioners.

It might also be pointed out that the "feminist whipping boy" of biological determination of behavioral differences between the sexes, apart from its historical importance in the contemporary critique of science, is immediately relevant to *HS*, given the citation of sociobiologist E. O. Wilson in the acknowledgments and his endorsement on the dust jacket. It is also a prime example of what goes wrong when biologists pretend "to isolate facts from values" in research bearing on human behavior. Two recent articles by mathematician Catherine Goldstein[1] persuasively dissect the ideological presuppositions, not to mention the ignorance of mathematics, that lie behind the endlessly quoted studies purportedly demonstrating male superiority in mathematics.

Goldstein, being French, is presumably vulnerable to Camille Paglia's critique, quoted with approval by Gross and Levitt as "exquisitely formulated": "French rot! Gibberish" (quoted in *HS*, p. 286). This charge cannot be leveled against Mary Beth Ruskai, who has used AMS data to show that claims of "discrimination…against white males" are simply false. Ruskai—who, by the way, has publicly expressed concern about many aspects of Science Studies—has also documented the pervasiveness of the attitude underlying the Gross-Levitt "white males" quote, though in academic circles it is rarely expressed in so crude a form.

Despite its bluster and bad manners, some might claim that the Gross-Levitt book has "done us all a great favor" by asking cultural constructivist critics of science the following question: what criteria do they propose to distinguish "reliable knowledge from superstition" (*HS*, p. 45) (or prejudice, or nonsense)? This is a really important question for Gross and Levitt (and for over a hundred generations of western philosophers, but never mind), and they argue that contemporary science, if it has no foolproof criterion, at least has a reliable methodology. But in their hands this methodology turns out not to be the refined instrument they imagine. For example, I have no doubt that they would pounce on the following quotations, if they were to find them in a book by a literary critic:

> In a way, mathematics is a novel about Nature and Humankind. One cannot tell precisely what mathematics teaches us, in much the same way as one cannot tell what exactly we are taught by *War and Peace*. The teaching itself is submerged in the act of rethinking this teaching.

> Having no external object of study, being based on a consensus of a restricted circle of devotees, … mathematics could not develop without the permanent control of rigid rules of [the] game.

However, these thoughts were expressed by one of us, and a particularly distinguished one at that, Yu. I. Manin, in his 1990 ICM address, entitled "Mathematics as Metaphor". Having made contributions of undeniable importance to mathematics, Manin is surely entitled to speculate as to their meaning. But even "with enough released time" I can't see how Gross and Levitt would distinguish Manin's metaphors from post-modern prattle on the basis of "no-nonsense logical positivism … with Popperian addenda" (*HS*, p. 86) alone.

I choose Manin's "Mathematics as Metaphor" as an example because, in one of their more insufferable passages, Gross and Levitt "testify to the *uselessness* of metaphor in mathematical invention…" (*HS*, p. 116) with an air of smug authority. So much for Manin; so much for linguist George Lakoff; so much for "knots", "flows", "surgery", and even "maps". No more "killing homotopy groups". There would be no reason to insist on such a petty point except that it illustrates the combination of arrogance and careless thinking that is a hallmark of *HS* and is precisely analogous to what Gross and Levitt reject in their targets.

I don't want to leave the impression that I am particularly impressed by work in the currently fashionable field of Science Studies, much of which deserves criticism and some of which seems to court ridicule. Like Michael

[1] Une creativité specifique des femmes en mathematiques? *in Sextant (Revue du Groupe Interdisciplinaire d'Études sur les Femmes), vol. 2, 1994.*

On ne nait pas mathematicien, *in Le Sexe des Sciences, Editions Autrement, Serie Sciences en Societé, no. 6 (date not indicated).*

Sullivan, I see an urgent need to "open lines of communication and narrow the cultural gaps in the academe." When I first heard about the Gross-Levitt book, I was hoping it could serve that purpose. But all Gross and Levitt proved is that misreading the work of other disciplines is as much a problem for scientists as for nonscientists. *HS* is in many ways the mirror image of the caricatures of science created by the more outlandish of contemporary science critics (who account for only a small minority of the contributors to the notorious issue of *Social Text*, by the way, though you wouldn't know that from the media coverage). No doubt the book would have been more serious and constructive had it been written by Michael Sullivan and Evans Harrell, to judge by their articles in the *Notices*. But a serious and constructive book would not have found a publisher in the current intellectual climate, where, as a critic wrote recently in *The Nation*, "Derision, more often than not, is the currency of exchange in culture-war literature; neither side listens to the other." *Higher Superstition* is no exception.

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Mathematicians and Social Responsibility

Susan Landau's editorial (*Notices* **44** [February, 1997], p. 188) outlines four obligations (not claimed to be exhaustive) that mathematicians have in order to meet their social responsibilities. I object to three of them: (1) "responding to *The Bell Curve* as mathematicians, and unraveling the arguments behind the statistical claims in the book." The book is described (in a footnote) as arguing that "racial differences in IQ measurement are determined largely by genetics." On the contrary, the authors (Charles Murray and Richard Herrnstein) take great pains to avoid giving that impression. For example, on p. 270 they write: "The debate about whether and how much genes and environment have to do with ethnic differences remains unresolved." And again, on p. 311:

> If the reader is now convinced that either the genetic or environmental explanation has won out to the exclusion of the other, we have not done a sufficiently good job of presenting one side or the other. It seems highly likely to us that both genes and environment have something to do with racial differences. What might the mix be? We are resolutely agnostic on that issue; as far as we can determine, the evidence does not yet justify an estimate.

Moreover, the *Wall Street Journal* published a substantial statement ("Mainstream Science on Intelligence", December 13, 1984, p. A18), signed by fifty-two professors characterized as "experts in intelligence and allied fields," consistent with the claims that *The Bell Curve* made. By all means, let us separate the strands of these claims (one meaning of "unravel") for further examination. But when one unravels a braid or a mystery, it ceases to exist; it has been "deconstructed". Under this interpretation, there are postmodernist connotations that mathematicians might not want to be associated with. The claims might be unpleasant, but we should not be obligated to nullify them prior to examining their validity.

(2) "preparing the biology students for the work they will actually do." But that is the obligation primarily of the biology teachers. Teachers of physics, economics, and so forth have similar obligations. I fear that incorporating realistic applications from a wide variety of other disciplines into our mainstream calculus courses will so bog us down in highly specialized modeling tasks and cumbersome algebraic and arithmetic calculations that we will have insufficient time to teach the mathematics properly. Our obligation as mathematicians is not to teach every subject but our own; instead we should give all our students the mathematical tools they will need for their various disciplines, with enough application examples to suggest the power and use of these tools, and try to reveal some of the beauty of our subject along the way.

(3) "providing programs, as Uri Triesman [sic] and others have done, that enable members of underrepresented groups to succeed in mathematics, and in science." My understanding is that these programs have done wonders for selected African-American, Hispanic, and rural white students. But I am concerned about the exclusion that is a central feature of these programs. I prefer to make our most effective pedagogical strategies available to all our students. If limited resources force us to be selective, then I would prefer that the selection be made using individually ascertained characteristics (such as being at risk) rather than group membership. Surely some members of the currently included groups are not at risk, while some members of the currently excluded groups are.

The danger is that social responsibility seems easily to slide into social engineering. If we focus so strongly on groups, then we lose sight of individuals. Murray and Herrnstein take great pains to avoid doing this (see pp. 312–314 of *The Bell Curve*); shouldn't we all?

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