Letters to the Editor

Shrieks and Shadows Over the *Notices*

Oh, for the old compact, pale little nonnonsense *Notices* of the late forties and the fifties that contained our abstracts and all sorts of exciting stuff! About the embellishments friend and colleagues have said it. My note concerns content.

The shadows are cast by a crowd of free-floating "this's" in Faris's attempt to explain a proof of Gödel's incompleteness theorem while reviewing Penrose's *Shadows of the Mind* in the February 1996 issue. The shrieks are mine, caused by, among other blunders, the omission of the crucial qualification "Meta-" in the citation of Kleene's classic "Introduction", calling Quantum Theory a "most peculiar subject", yet another appeal to a God that does not play dice, a pat on Penrose's shoulder for not worrying about nonstandard numbers (why should he—he probably understands how they come about), and, worst of all, a careless equivocation between soundness and consistency.

It is a sloppy review of a serious book, and I wonder what the *Notices*'s editorial policy is for such submissions.

Gödel's incompleteness theorem has become an "in" subject during the last couple of decades. Its methodological content is much less surprising than the fact that it can be articulated precisely enough to lend itself to a rigorous proof. The publication of yet another questionable sketch of it in a place accessible to a wide audience is irresponsible. The initiated may shrug it off as another garbled account of a great event in their field. But the innocent will either gobble it up and indiscriminately perpetuate the confusion, or they will be baffled and turned off by their justified failure to see how it all hangs together.

For the benefit of the latter I want to make a few comments. First, direct perception—or intuition—of the infinite sequence of natural numbers need not commit you to Platonic beliefs. Of course you are right; how can we talk about true but unprovable statements if we don't have some native concept of Mathematical Truth to base all this talk on? The fascinating questions are, how we find those truths, where we take our axioms from. Maybe Penrose does have something to say to these questions. Don't worry; formal Peano arithmetic (PA for short) has, in fact, been proved consistent, but, of course, by methods that transcend PA. And finally, the existence of nonstandard numbers is no disaster for the Foundations of Mathematics; it merely points out a weakness in the expressive power of so-called first-order formalisms.

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Mathematics in the Soviet Union in 1965

Recently we both had the opportunity of reading Paul Halmos's *Automathography*, an often penetrating and wonderful book that until now was not available in Moscow. This letter, however, is not a belated (the book was published in 1985) acknowledgment of our appreciation, but rather an attempt to clarify the underlying motivations of an episode that it describes. The episode is Professor Halmos's visit to Moscow in the spring of 1965 and, more specifically, his contacts with Mark Krein.

It is clear from the book that Prof. Halmos was looking forward to seeing M. G. Krein (for obvious mathematical reasons) and that the meeting itself was a disappointment: in the book it is described as "strange" and "strenuous" (pp. 313–314), and Krein himself is characterized later as "strange" (p. 387). Prof. Halmos is obviously puzzled by the fact that Krein refused to see him in Odessa, which he had planned to visit (he writes candidly about Krein's refusal, transmitted by an academic official, "I never figured that one out"). He was even more baffled by Krein's request to send some offprints to the U.S. and did not understand why Krein would not spend "the same number of rubles" to mail them from a Moscow post office himself. Concluding his description of a mathematical conversation he had with C. Foias and Krein, Prof. Halmos unexpectedly writes, "I felt guilty, but I am not sure why." Why indeed?

We should note that Prof. Halmos also had problems with several other mathematicians of Krein's generation not involved in Communist Party activities. He describes his contacts with Kolmogorov as "awkward" and the latter as "ill at ease", his meeting with Rokhlin as "difficult"; he cannot understand why Shilov "disappeared" after Halmos's lecture instead of meeting with him. In contrast to this,

he got along nicely with members of the Soviet official mathematics establishment, as well as with all the younger mathematicians (Anosov, Sinai, Kirillov). Why?

The reasons are absolutely obvious to Russian mathematicians who worked in the Soviet Union at the time, but we feel that many of our Western colleagues would share Prof. Halmos's puzzlement. Let us explain. First, Mark Krein could not possibly invite Halmos to Odessa: at the time it was out of the question, even for high-ranking establishment scientists, to invite (officially or unofficially) Western guests to a provincial city. (We should add that Krein, who was Jewish and had always been unequivocally anti-Communist, was dismissed from Odessa University in the antiSemitic campaign of the late forties, worked in an obscure technical college, and was on bad terms with the local authorities.) Secondly, Krein obviously gave Halmos his offprints to mail *from the United States*, not from Moscow, because foreign correspondence of that kind was promptly conveyed to the KGB (which is probably what occurred after Prof. Halmos's unfortunate visit to the post office). Finally, although of course Kolmogorov, Rokhlin, and Shilov are no longer here to answer this question, one might ask if their uneasiness in meeting Halmos was not explained by the fact that they were simply scared of him as of a person who might inadvertently cause them serious problems with the authorities.

(This was not the case of the Soviet establishment mathematicians, who had probably been "cleared" for meeting Halmos in order to inform the "proper authorities" on their talks with him, nor was it the case of the younger mathematicians, who had begun their careers in the "Khrushchev spring" and whose courage and honesty had not been broken in the past by the shadow of Stalin's camps and had not yet been put to the test in the Brezhnev crackdown that was about to follow.)

To come back to M. G. Krein, he was not only a truly outstanding mathematician, one of the creators of modern functional analysis, but, for most of those who had the privilege of really knowing him, an extremely honest, kind, and cultivated man. We feel that for him the adjective "strange" is hardly appropriate. Incidentally, in Halmos's book this same opinion is expressed by Prof. D. Milman who, according to the author, describes several Russian mathematicians (including Kolomogorov and Gelfand) as "crazy" but resolutely denies any strangeness in Krein (p. 387).

The only thing really strange about Mark Grigorievich Krein is how he managed to achieve what he did in mathematics in the face of adversity and almost total isolation. He was one of the very few mathematicians of his generation who (although he was never an overt dissident) refused to participate in any of the Communist-inspired actions that his conscience deemed doubtful; he often resolutely stood up to the authorities; he was appropriately "rewarded": the nervous tick so vividly described by Prof. Halmos was the result of nerve-wracking and dangerous conflicts with the powers to be in Odessa and elsewhere.

We hope that this letter will not be misinterpreted. We are in no way being critical of Prof. Halmos, who (this should be stressed) has the good taste of not passing judgment on people he has no affinity for and, in the case of Krein, has enough sensitivity to feel that there was something wrong that he could not understand. This letter was intended to explain to our Western colleagues, who have not "benefited" from life in a totalitarian society, its underlying vissicitudes, often invisible to the outsider. And to say a few good words about a wonderful man who deserves many more.

(Ten years have passed since Halmos's book was published, and possibly an explanation of the Moscow episode similar to ours has appeared in print, in which case this letter is redundant. However, our experience teaches us how quickly people forget, in particular, the psychological hardships of life in a totalitarian society.)

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Using non-Ph.D. Mathematicians to Teach Mathematics

As I understand what happened at the University of Rochester (*Notices*, March 1996), the administration did two things:

1. eliminated the math Ph.D. program and
2. decided to hire adjunct faculty and use faculty from other departments in the future for teaching many math classes.

The main interest of the articles in the *Notices* was in the elimination of the Ph.D. program. I think we should be much more concerned with the second action. A strong argument can be made for eliminating many universities' (I don't think Rochester's is one of them) math Ph.D. programs. But the replacement of Ph.D. mathematicians with non-Ph.D. mathematicians for teaching many college-level math courses will severely damage higher education in math and greatly reduce the number of math researchers.

The quotes from the administration of the University of Rochester indicate that they believe that non-Ph.D. mathematicians (including "technology") will do a better job teaching service classes in math. I think we research mathematicians deserve the bulk of the responsibility for this disastrous belief. The statements from the University of Rochester about teaching are no more than what many math departments have been telling administrators for some time. We wanted to have more fun, spending most of our teaching time on higher-level courses, so we assured administrators that TAs and non-Ph.D. mathematicians could teach the service classes just as well as we could. Besides being wrong, this assertion is suicidal. The service classes are the bread-and-butter of a math department. We may have talked ourselves (to be more precise, future mathematicians, including our current Ph.D. students) out of a job.

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Time to Panic

The inside cover of the *Hitchhiker's Guide to the Galaxy* is emblazoned with the words, "Don't Panic". Unfortunately, for undergraduate mathematics, it is time to panic. The recent situation at the University of Rochester is not an aberration, it is the wave of the future if we do not make some dramatic changes. In 1990 Gail Young and I detailed how enrollments in what were essentially upper division mathematics courses being offered in nonmath departments were outstripping mathematics enrollments. The response was silence. We are losing students. In 1970 11 percent of undergraduate enrollments were in courses above the level of calculus. In 1980 the number was 9 percent and in 1990—7 percent. If all the courses we teach are calculus or remedial, than maybe the replacement of full-time Ph.D. faculty by adjuncts makes sense (at least to budget-cutting deans and college presidents).

If we expect to deal with the underemployment of mathematicians, then we have a simple task—increase the number of students enrolled in our courses at all levels. Certainly, this is more than a question of economics. We all believe that students are better off taking more mathematics, or at the very least learning the mathematics they do take from mathematics faculty. We simply need to address this problem directly.

Those faculty at smaller than Rochester institutions are living with this everyday. Departments which used to teach a linear algebra course every semester, now wonder if they can afford to offer it once a year if only five students register. This is daily life for hundreds if not thousands of institutions. Admittedly, the economic effect has been masked by increased enrollments in entry-level courses which have long justified the size of present day math departments. But this is no longer the case.

The key here is to raise this issue to the top of the agenda of the mathematics community. Unless we encourage more students to learn more mathematics in mathematics departments, these will soon seem the good old days. Personally, I would like to see the community focus on the curriculum, because I believe that that is where much of the solution lies (and of course I work in curriculum development). But wherever we look for solutions, it is crucial that we recognize the seriousness of the problem. It is not enough to wring our hands over the unenlightened administration at Rochester. The problem is more serious and more endemic. It will require the courage and leadership of our professional societies and a great deal of hard work on all of our parts.

We must be honest. Our introductory courses have been designed by a variety of committees to serve a host of client departments (the very departments who are quite happy to teach advanced mathematical subjects to students we used to enroll and tell administrations what a terrible job we do in preparing students for their courses). We cannot bemoan the loss of students and at the same time give over to others the creation of our own curriculum. Students learn what mathematics is from us. They learn it in their first year of college. They decide, partially based on this experience whether to take additional math courses. What are we telling them? Do we know? What do they think?

It is time to ask and answer these questions. It is time to take responsibility for our course offerings and for our enrollments. It is past time. It's time to Panic.

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Book Review by Klingenberg

The *Bulletin of the American Mathematical Society* published a "review" by Wilhelm P. A. Klingenberg (vol. 33, No.1, January 1996, pp. 75–76) of our book, *Affine Differential Geometry* (Cambridge University Press, Cambridge, 1994). It is the purpose of this letter to refute the review.

The text of this review has 63 lines and is structured as follows:

1. He uses 26 lines in his cursory explanation of the induced connection ∇ and the affine metric h "to the uninitiated." In the next lines he states, "Actually, if h is nondegenerate, we may view it as a (possibly indefinite) Riemannian metric on M. The canonical Levi-Civita connection determined by this metric is the same as the one yielded by the above defined canonical ζ."

This statement is totally absurd, because the two connections coincide if and only if the hypersurface is a quadric—a classical result due to Maschke, Pick, and Berwald (Theorem 4.5, p. 53, of Nomizu-Sasaki).

2. In the next 18 lines he notes that a certain geometric interpretation is missing and continues, "Maybe the reason for this lack is that the authors believe that geometric arguments do not fit into what they call the structural point of view." Needless to say, this is a highly audacious statement. He also calls the title "What is affine differential geometry?" of a now 14-year-old lecture by one of the authors "grandiose".

Here the structural point of view is simply an adaptation of the idea of geometric structure that is prevalent in modern differential geometry. Does a given manifold admit a geometric structure of a certain type with some additional properties? Does the structure induce naturally the same type of structure on a submanifold? Study the relationships between the properties of these structures. For the classical affine differential geometry, the relevant geometric structure is an equiaffine structure, namely, a pair of a torsion-free affine connection and a volume element that is parallel relative to the connection; the standard model is an affine space $R| = A7n + 1 = E1$ with its usual flat affine connection and a usual determinant function. Thus the first question is: given a hypersurface in R^{n+1}, can we induce a natural equiaffine structure on it? In affine differential geometry, however, such a point of view was not widely adopted until rather recently. It has helped us to clarify the foundation of affine differential geometry and to generalize it to the geometry of affine immersions with interesting extensions of

classical theorems. It has also led to applications to higher codimensions, general ambiant spaces, and projective differential geometry. The readers may get a view of such developments in the introduction of the book (in particular, page xii).

3. In the next 7 lines the reviewer continues to attack the structural point of view by saying, "it is often not much more than the use of a modern terminology." He also claims that the concept "affine immersion" is nothing but the "rigging" of a submanifold in an affine space, as used already by Schouten in his book.

This explanation by the reviewer is not correct. Chapter V, Section 3, of Schouten's book contains computations leading to the choice of the affine normal. But he does not really discuss the concept of an affine immersion. Here it was Elie Cartan who had geometric ideas that were ahead of his time. In the rarely mentioned C.R. note in 1924 ([Carl] in the reference of our book) he points out that the induced connection on a nondegenerate surface is independent of the determinant function we pick in the ambiant affine space $R|3$. He also posed a general question whether a 2-manifold with an affine connection can be realized, that is, can be affinely immersed in $R|3$. Also another idea of Cartan, vaguely expressed in [Carl] and later supplemented by Norden (Appendix of [Schr]), is now stated as Theorem 3.2 (p. 159) in Nomizu-Sasaki. A precise formulation of this result would have been impossible were it not for the notion of an affine immersion.

4. In the next 7 lines the reviewer says, "Even more lamentable is the lack of credit given to L. Berwald when the authors introduce the concept of a 'Blaschke structure'. It should be called the 'Blaschke-Berwald structure'…." Then the reviewer goes on, "He was a Jew and therefore lost his job in Prague under German occupation. In 1941, he died under miserable circumstances in the ghetto of the city of Lodz." Does anyone understand why he is injecting this personal history of Berwald?

As for credit to Berwald in our book, see the introduction, pages 27, 53, and 169, and recall the reference in item (1).

5. At this point the reviewer gives a one-line description of the content of the book: simply, "The main body of the book under review is devoted to hypersurfaces."
6. In the final 11 lines the reviewer has yet to complete his personal agenda. He first puts down "a new wave of papers devoted to affine differential geometry" by saying, "After all, there is no limit to the number of questions one may pose. But it also is obvious that the field is isolated from the main stream of mathematics."

What arrogance! To say the least, the new approach to the geometry of affine immersions has brought the classical theory much closer to Riemannian geometry, to projective differential geometry, to other areas of differential geometry, and to mathematical statistics (see Introduction, p. xii, and [Am1]).

7. The reviewer then finds that "There is no hard analysis involved, except for a few papers by Calabi, Terng, and Yau. But their proofs fall outside the scope of this monograph."

To be accurate, why isn't Chern quoted with Terng (our reference [ChT]) and Cheng with Yau (our reference [CY1])? If the reviewer has the results in [CY1] in mind, why not give credit to the work in [Gi], [S1], and, most crucially, to the work by An-Ming Li, [L5, L7, and L8], which gave the final solution to Calabi's conjecture on locally strictly convex hyperbolic affine hyperspheres and related completeness questions. Our book gives a summary of these results in Note 7 (p. 217). The monograph [LSZ] *Global affine differential geometry of hypersurfaces*, by An-Min Li, Udo Simon, and Guosong Zhao (de Gruyter: Berlin and New York, 1993), contains the proofs in Chapter 2. No review mentioning Calabi's conjecture would have been complete without quoting this book. In discussing literature in affine differential geometry, we should also mention P. A. Schirokow and A. P. Schirokow, *Affine differentialgeometrie* (Teubner: Leipzig, 1962), for its wealth of information.

8. The reviewer ends his polemic with this parting shot: "A look at the type of journals in which the papers quoted in the book have appeared also bespeaks the quality of the results presented."

A quick check on our references will show that several papers appeared in *Proc. Amer. Math. Soc.* or *Trans. Amer. Math. Soc.*, some others in *Tôhoku Math. J.* or *Results in Math.*, and many in *Math. Ann.* or *Math. Zeits.*, and so on. So how do these journals rank according to the rating system of the reviewer, who was, or still is, on the Editorial Committee of some of the journals?

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P.S. The writers urge the American Mathematical Society to take measures to prevent any further appearance of this kind of book review in the *Bulletin* by insisting that the Editorial Board for Book Reviews, and not any one person, be made responsible for ensuring high standards of quality and accuracy in all published reviews.

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