Commentary

In My Opinion

Opinion and Responsibility

This will be my last opinion column as a member of the *Notices* editorial board. Although I will remain active in Society affairs, it is time to move on to other things. If you read my words again in the *Notices*, as you probably will, I will be wearing a different hat. I would like to take this opportunity to reflect on my experiences, from both sides of an editor's desk, with publishing opinion.

I began with the idea that the right of AMS members to publish letters on matters related to mathematics should be subject only to modest limits on length and frequency, and such basic constraints as required by libel laws and common decency. Longer articles and "Forum" submissions need more selectivity and higher standards of exposition. However, it is essential that the *Notices* publish contributions on all sides of an issue, including those that an editor may disagree with. This seems to suggest that editing of content be kept to a minimum. Although I still believe strongly in these principles, I soon realized that things are not always so simple.

Even in opinion pieces editors need to insist that quotations are accurate and that any data or factual material cited to substantiate an argument is correct. However, it is not always clear where to draw the line when an out-of-context quotation seems to distort someone's views. Should the editor demand revision or risk provoking a long sequence of clarifying letters? Care with data is even more important, since the *Notices* may subsequently be used as a secondary source of information.

Consider, for example, p. 768 of Damon Scott's "Counterpoint" in the August 1999 *Notices*. In response to Cora Sadosky's earlier assertion of "declining overall funding for basic research," Scott states, "Actually, the federal government has expanded expenditures for research, even with inflation taken into account." But the astute reader will notice that Sadosky said *basic* research, and the figures Scott gives for the *total* NSF budget include research in education, social science, special initiatives, etc. And the "total scientific research" figures he cites include the "war on cancer" and the space program. Were the editors right to leave it to readers to notice this distinction? Or should they have insisted that Scott obtain the data needed to distinguish between education, applied, and "basic" research?

My own first foray into "op ed" was not in the pages of the *Notices*, but via a 1986 letter in the *AWM Newsletter* about gender and science. The response was enormous and gratifying. However, although I had hoped to get more people involved in the gender aspect of what is now known as "science wars", many responses contained suggestions of things for *me* to do. There were panels and more articles, but the number of people actively involved remained small and the impact smaller still. More recently, my columns in the *Notices* on controversial matters involving teaching, student evaluations, AMS governance, and the like have attracted disappointingly few responses. We need more letters and thoughtful input on such issues.

For example, my previous (May 1999) column concluded by encouraging readers to make their views on "Featured Reviews" known to the AMS leadership. But I sometimes get the impression that readers are more likely to convey their views to me. For example, at a recent conference a colleague took the opportunity to express to me some concerns about a sensitive matter regarding a particular featured review. Fortunately, I was able to redirect him to an appropriate editor at *Mathematical Reviews*.

Since writing that column I have learned that some physics and chemistry organizations are making "featured articles" from their journals available electronically. Emulating the supermarket practice of "loss-leaders", the editors select a few articles which anyone can download, while most of the journal is available only to subscribers. The practical effect of this is that a few articles are advertised and distributed worldwide, while the rest are available only to subscribers who take the trouble to examine the table of contents. Is this fair? Will mathematics journals adopt this practice? Who will decide?

These are only a few of the many serious issues facing the mathematics community. Focusing on another arena, Douglas Ravenel's retrospective "Forum" on Rochester in the September 1999 *Notices* gives a graphic reminder of the consequences of ignoring university politics. Most of us would rather concentrate on mathematics than spend time on politics, whether at our institutions, in Washington, or within the AMS and similar organizations. Not everyone can, or should, do everything; people must choose issues and arenas in which they can be most effective. However, if we all ignore politics in favor of mathematics, then there will soon be far too few opportunities left to do mathematics.

—Mary Beth Ruskai
Associate Editor

Letters to the Editor

Dealing with Reduced Enrollments

In his letter in the June/July 1999 issue of the *Notices*, Kevin Charlwood notes how his department at Washburn University is dealing with the enrollment problems discussed in "The Sky Is Falling" (S. Garfunkel and G. Young, *Notices*, February 1998). The "Sky" article came up for discussion during a Special Session at the AMS Meeting on September 12–13, 1998, at DePaul University in Chicago. It also is a periodic topic of the Calculus Reform discussion group.

An examination of the 1995 CBMS Survey (published in September 1997 by the MAA), on which the Garfunkel/Young article is based, provides some interesting information:

(1) Enrollments in mainstream calculus I & II—the standard first-year calculus course—fell 4.5% from 1990 to 1995.

(2) Enrollments in second-year calculus-related courses (mainstream calculus III & IV, linear algebra, differential equations, etc.) fell by 22.1% over the same period.

(3) The percentage of enrollments in upper division courses when compared to enrollments in the second-year courses was 61% in 1990 and 63.2% in 1995.

(4) (a) Enrollments in nonmainstream calculus I & II—the standard nonmajors' calculus course—fell by 31.9% from 1990 to 1995.

(b) Enrollments in elementary statistics—no calculus prerequisite—rose by 32.2% over the same period.

(c) The net loss in enrollments from (a) and (b) is some 24,000 students.

The decrease in (1) by itself is not that significant; the real drop in "calculus" enrollments comes from (2) and (4a). The drop of 22.1% in the second-year courses is of particular concern, since, as (3) indicates, once we get students into second-year courses we seem to maintain a consistent progression into our upper division courses. The changes noted in (4) may indicate that many students in nontechnical majors are not all that interested in calculus and are looking for alternatives. We also must deal with things like the reduced mathematics requirements in new degree programs such as B.A. degrees in chemistry and environmental science tracks in biology. And then there is the new orientation of the first four actuarial examinations, which will take effect in year 2000.

We need to work on solutions to these and other related problems. Even though these difficulties are national in scope, the solutions probably will be local in nature since, to my knowledge, our professional societies have expressed little interest in dealing with these enrollment problems. Communication is important and we should use any and all forums available to us to let one another know what works and what does not.

—Richard J. Maher
Loyola University of Chicago

(Received July 16, 1999)

Discovery and Proof—Right from the Start

In the October issue of the *Notices*, Arnold Ross ("Vitality of Some Very Old Ideas") and Krzysztof Jarosz ("Teach Calculus for Full Understanding of Concepts") both argue for teaching mathematics that is closer to mathematicians' own understanding of the subject, and away from the increasingly exclusive focus on mere computation and application. Jarosz states, "In many colleges a mathematics student is expected to take three calculus classes, then a course in logic and proofs, another advanced calculus course, and finally a real analysis course. But if the students were supposed to really understand the material in the calculus sequence, then why do we need all the rest?" In my calculus courses, I spend a day or so explaining to students that they need not be intimidated by proof and introducing a method whereby they can find their own proofs to theorems in their text. The method is further developed in a text supplement *Outlining Proofs in Calculus*—available free at `http://www.umemat.maine.edu/faculty/wohlgemuth/index.html`.

Interested students work through the supplement on their own (with help from me as needed) and carry on as far as they want. This is far short of Jarosz's suggestion that we "arrange the undergraduate curriculum around courses in logic and proofs, and ...cover only as much calculus as we can cover with full understanding of the concepts discussed." But it's a step in the right direction—also a step that brings about no student complaints, and is therefore acceptable to the administration.

The system in *Outlining Proofs in Calculus* is an abbreviation of a version developed to help math majors who had difficulties understanding what a proof is and how to go about finding their own proofs. Continued efforts to clarify things forced me to be explicit, and led to a system where even the logic used in proofs was explicit. It looked a lot like formal logic, but was devised solely to help students who did not understand—without any reference to works in logic. Now students using the system start out with the formal and explicit, and become informal only after the foundation is secure—just the reverse of the process that led me to the system.

The resulting system was so simple and straightforward that I became convinced that it ought to be taught, not just to math majors in college, but to school students. I decided that it would be a good approach to teaching prospective teachers. The elementary ideas about sets and subsets and the set operations of union, intersection, and difference are now generally introduced in junior high school. These set operations and relations so closely follow the logic used in elementary mathematical arguments, that students using the system are naturally prepared to prove any (true) conjectures they might discover about them. It is an easy entry into the world of discovery/deductive mathematics—and things can build gradually in complexity.

In a course for prospective elementary and junior high teachers (nonspecialists in mathematics), I broke the class into groups. A group of students played the role of editors. The other groups submitted the results of their investigations to the editors. The students used Venn diagrams (examples of sets) to make conjectures about set intersection, union, and difference. A systematic use of the diagrams led them to *discover* DeMorgan's laws, the distributivity of intersection over union and vice versa, as well as many (almost all useless in themselves) set identities. They submitted to the editors their conjectures along with narrative proofs *based on the definitions* of intersection, union, and difference (not the pictures). The editors finally compiled the corrected results. The proofs were the students' own. They were readable, based on fundamental ideas, and with a level of rigor that would be acceptable in any graduate mathematics course.

Even at this very elementary level—and with students who had showed little or no prior interest in mathematics, many students showed the same enthusiasm for discovery and proof reported in the Ross Young Scholars Program. One student wrote, "This was a wonderful class, mostly because of the instructor's unusual approach. He got me interested in a subject I used to hate!"

A monograph, *The Basic Ideas of Mathematics, Volume 1: Introduction to Logical Reasoning—with a System for Discovering Proof Outlines*, can be downloaded (free) from the Web address above. It has been used as a text with prospective teachers and in a lab-type course where math majors read the material on their own and submit solutions to the exercises for correction in the class/lab.

—Andrew Wohlgemuth
University of Maine

(Received July 23, 1999)

An Army of Mathematicians

For all the jokes that mathematicians make about the military, there is one concept that the military takes for granted which seems to be far beyond the grasp of even the best mathematicians. That is, there is no army without the foot soldiers. That without the troops, there would be no need for generals. That if the only people who were allowed to stay in the army were Medal of Honor winners, than the medal itself would become meaningless.

Yes, the officers go to their elite schools to receive an elite education. Yes, they use their brilliance to formulate strategy and claim credit for the victory. But they understand that once all the preparations are done and the important day has arrived, victory or defeat will take place in the trenches. Therefore, it is extremely important for them to take an interest in the training of the troops. If a soldier is not performing up to standard, they don't just throw him/her out of the army. First, they make every effort to help them improve. And when things look bad on the battlefield, they come forward risking their own lives, to help inspire the troops to victory.

Mathematics is staring in the face its worst defeat in history. Lack of research funding, good positions, etc., are driving the future Medal of Honor winners out of the field. The troops are beginning to lose their morale. Their belief that victory will ultimately be theirs is waning. *Where are the generals?* It is time for them to take a risk. To come out of their Institutes for Advanced Strategy and provide leadership. It is time for them to band together, go to Congress, use their power and prestige to save the army. For to do nothing is to say that the army isn't worth saving. And one day, someone will notice that there is no longer an army and begin to question why we still have generals.

—Peter Casazza
University of Missouri

(Received July 29, 1999)

The Role of Statistics in the Census

R. Schlafly (Letter to the Editor, *Notices*, August 1999) inappropriately likens the census to an election. Voting paradoxes familiar to the mathematical community show that elections do not measure something that can be well-defined. Different apparently reasonable voting procedures can produce significantly different outcomes. It is not at all clear that there is analogous difficulty in ascertaining, for example, the number of people who live in a given geographical area.

The existence of biased methods of estimation does not imply that all estimates are biased or that no method is less biased than others. It is entirely appropriate for statisticians to comment on the scientific merits of various techniques of estimating populations for census purposes. If people cannot refute the analysis that shows their preferred method of counting is statistically inferior, then they should leave the statisticians alone and argue the merits of their case in an intellectually honest manner.

—Daniel Frohardt
Wayne State University

(Received July 29, 1999)

Pressure To Study Calculus in High School

Want to know why students are arriving at the doorsteps of college and university mathematics departments so poorly prepared? Partly because of the almost irresistible pressure on them to rush through the material that they need to be prepared, and to study it before they are mature enough to understand it.

Sure there are kids for whom acceleration is an appropriate and joyful experience. These are the kids that the AP program was originally designed for. I teach in a selective independent school and between 25 percent and 35 percent of our kids can reasonably expect to take calculus before graduating. Most of the rest of the students graduate with four years of algebra (during grades 8–12) that includes rigorous work with proof, limits, vectors, matrices, data analysis, and a variety of three-dimensional coordinate systems. They graduate with an intuition not only about traditional precalculus topics, but about prelinear algebra topics and discrete math topics as well, because they have had the luxury of spending time looking at these topics from a variety of viewpoints and getting their hands dirty playing with them.

However, we may not be able to continue this for much longer in the face of the pressure from college admissions people to get the kids to calculus in their senior year and the overwhelming anxiety this arouses in some parents who want their kids to accelerate when they are not ready to do so.

College admissions officers look at AP Calculus enrollments (enrollments, not AP test scores—decisions on admission are made well before kids ever take the exam) and see a nifty flag to help them identify "the kids we want". The message they communicate to high school college guidance counselors is "we are most interested in looking at kids who have taken AP Calculus". (They *do* say this. Ask any college guidance counselor.) It has taken about a nanosecond for parents and school administrations to get this message and another nanosecond for the pressure to mount to accelerate kids—ready or not, mature enough or not, well-prepared enough or not—as quickly through algebra as humanly possible.

If college and university math educators around the country really want to do something about the sorry state of the average freshman's math preparation, they ought to talk to their admissions officers and get them to stop using enrollment in AP Calculus as a convenient flag. They ought to convince them that having taken calculus in high school is not necessarily a virtue. They ought to convince them as forcefully as possible to communicate enthusiasm about schools that challenge their students with strong *algebra* programs. For too long, admissions officers have spoken as the self-appointed mouthpieces of mathematics departments and their messages have not done the state of math education any favors.

—*Joan Reinthaler*
Sidwell Friends School
Washington, DC

(Received August 16, 1999)

Irving Segal and Nonlinear Hyperbolic PDEs

I enjoyed the reminiscences about Irving Segal in the June/July issue of the *Notices*, but there was one circle of ideas that was almost entirely omitted. These ideas need to be pointed out because through them he had a great influence on the subsequent developments in nonlinear hyperbolic partial differential equations.

As early as 1951 he wrote a book review of Laurent Schwartz's *Théorie des Distributions* where he opined that this theory would have its greatest significance in hyperbolic PDEs. In the late 1950s and early 1960s, as part of his program in quantum field theory, he started talking up the idea of developing the theory of nonlinear hyperbolic PDEs which (aside from shock waves) hardly existed at that time. "Talking up" was an active process for him as he would buttonhole everyone in sight with his ideas about his latest passion. It was usually somehow related to quantum theory. I was lucky enough to become his student around that time, and he suggested that I look into the scattering and quantization of nonlinear waves. It was not at all clear how to even formulate mathematically what scattering meant. However Segal had the ability to formulate concepts that no one else even knew existed; that was another of his characteristics. As for scattering, he was unrealistically optimistic, or so I thought at the time. It turned out that his original wild conjectures were in fact true but it took many people's contributions over twenty years to find out!

In 1961 Konrad Jörgens's paper on nonlinear wave equations came out. In his seminar at MIT, Segal brought out how the basic concept of energy conservation could be used to greatly generalize that paper. He waited until he found a general formulation that really satisfied him. This led to two very important papers. In one of them he invented and developed the concept of a semigroup of nonlinear operators, including the definitive formulation of the concept of blow up in finite time of a solution in a Banach space. In the other paper he used weak convergence to construct global weak solutions of nonlinear wave equations with positive energy. In a third paper later in the 1960s he showed how to construct global solutions in significantly more general situations provided the Cauchy data was sufficiently small. Much later, this work of Segal was generalized by Fritz John, Sergiu Klainerman, Jalal Shatah, and me to handle more difficult nonlinear terms.

Around 1970 he suspected that if there existed L^p estimates for linear wave equations, they could prove useful to the nonlinear theory. His suggestion led to the first of Robert Strichartz's papers on the subject. Then in 1976 he wrote a seminal paper proving L^6 estimates in both time and space for the one-dimensional case. He suggested the possibility of generalizing these estimates to more dimensions. In 1977 Strichartz succeeded in doing so by making use of interpolation and duality techniques. True to form, these estimates have turned out to be much more central to the theory of nonlinear waves than anyone originally suspected. This uncanny ability again illustrates Segal's mathematical personality: he made a suggestion that seems at first just a bit off base and it ultimately led to a great success. Nevertheless, he was free with his ideas and always exceedingly generous to young mathematicians, and he was happy to see them get the full credit for their successes without claiming any credit for himself.

Segal's later work in PDEs was a reflection of his growing interest in cosmology and the conformal group and led to papers on the Yang-Mills equations and the Goursat problem, using the method of conformal compactification. He was always centrally interested in what he called "fundamental physics", which for him mainly included quantum theory, quantum field theory, and cosmology. To him nonlinear wave equations were a key ingredient in the mathematical construction of such physical theories. His ideas have become a crucial part of the fundamental theory of nonlinear waves.

—*Walter A. Strauss*
Brown University

(Received August 17, 1999)

More about Visualization

I am writing to give further context to Palais's article in the June/July *Notices* recounting his travels in the world of visualization. First of all I want to draw attention to the work of the pioneers of 3D math visualization: Nelson Max, Tom Banchoff, George Francis, and David Hoffman. In particular, Francis's courses on math visualization for undergrads and grads at the University of Illinois are exemplars of the genre.

In his article, Palais enumerated a number of useful programs doing 3D graphics in mathematics: some were small, specialized programs, while others were large, important, multifaceted software packages for research and exploration. Among the latter I want to bring special mention to Jeff Weeks's SnapPea and Ken Brakke's Evolver. Weeks became a MacArthur Fellow this year in recognition of his creation and implementation of SnapPea, which computes and visualizes hyperbolic 3-manifolds. Brakke's Evolver computes (and visualizes in 3D) minimal surfaces and other solutions to geometrical optimization problems; this is especially useful since most such surfaces do not have explicit parameterizations like the minimal surfaces Palais mentions and displays on his Web site.

The bottom line is that 3D graphics is extremely time-consuming, unless there is a program that does just what one wants. In higher dimensions visualization is, with some notable exceptions, even more problematical. It is important that graphics projects have substance at least commensurable with the time required to implement them. And then one might like to use the pictures to tell the world about the subject; this too is difficult and rarely done well. It is true that in research, objects are often just as well seen by our "mind's eye", helped by a few apt diagrams. Yet there are more and more cases where, as for the Costa surface, progress in research depends on the results of visualization experiments.

The number of research mathematicians able to do original and professional grade 3D graphics remains infinitesimal. With very few exceptions, it is not an activity supported by local math departments. Konrad Polthier and John Sullivan are perhaps the most prominent practitioners among the current generation.

In contrast, there is an immense amount of 3D visualization being done in related sciences, especially chemistry, engineering, and physics, including fluid dynamics (not to speak of the entertainment industry). Mathematics too has much to gain by expanding large-scale software development which appropriately incorporates and adapts 3D graphics tools. Such activities might be further advanced if the AMS were to offer prizes for the best work.

—Albert Marden
University of Minnesota

(Received August 23, 1999)

Exactly Solvable Models in Statistical Mechanics

I read with great interest the recent article by David Bressoud and James Propp (*Notices*, June/July 1999) on the alternating sign matrix conjecture. In recent years, it has been found that results of exactly solvable models in statistical mechanics often shed new light on many classical problems in mathematics. These include the role played by the Temperley-Lieb algebra (which arose in their study of the Potts model); in the Jones polynomial in knot theory; in new Rogers-Ramanujan type identities discovered by George Andrews, Rodney Baxter, and Peter Forrester through their solution of a Solid-on-Solid (SOS) model; and in the intriguing connection of the infinite-state Potts model with the intractable problem of multidimensional restricted partitions of an integer observed by F. Y. Wu. The article by Bressoud and Propp offers another fine example of this.

In their article Bressoud and Propp presented an excellent and eloquent review of the history of the proof of the alternating sign matrix conjecture. A key step in Greg Kuperberg's proof, as recounted by Bressoud and Propp, is the use of the 1982 Korepin-Izergin solution of a six-vertex model in statistical mechanics. However, because the article concentrates on the relation of the Korepin-Izergin solution to Kuperberg's proof, it may leave readers with the impression that there was very little work done on the six-vertex model prior to the work of Vladimir Korepin and Anatoli Izergin in the 1980s. This impression cannot be further from the truth! In fact, there is a very rich history of the six-vertex model dating back to 1967, a history which also parallels the development of the "modern" statistical mechanics. The purpose of this letter is to point this out and provide readers with a brief account of these early and very exciting developments.

The field of exactly solved models in statistical mechanics began in 1944 when Lars Onsager solved the Ising model in two dimensions. The field remained essentially dormant after that for more than twenty years until 1967 when Elliott Lieb published his seminal solution of the square ice problem, a version of the simplest six-vertex model. It turned out that this result was a major turning point in statistical mechanics and quickly led to a succession of solutions of other six-vertex models by Lieb, and by Bill Sutherland and C. N. Yang, solutions which exhibit new types of phase transitions. This also led to another major step forward by Baxter in 1970 when he announced the solution of the eight-vertex model. In his book *Scope and History of Commutative and Noncommutative Harmonic Analysis*, published by the AMS in 1992 (see especially pp. 356–360), George Mackey gives a very nice discussion of the history of this subject in which he clarifies the contributions by Lieb and others, as well as providing insightful connections to other areas of mathematics.

Given this extensive work, one might wonder why the article describes Kuperberg as waiting for a 1993 book by Korepin, Nikolai Bogoliubov, and Izergin before he was able to exploit their work. However, the Korepin-Izergin solution is that of a six-vertex model under a "domain wall" boundary condition (described in the article) introduced by Korepin in 1982 for the purpose of analyzing the uniqueness of the Bethe ansatz solution, while the versions solved by Lieb and others are models with the more commonly used periodic boundary condition. It turns out that internal vertex configurations of the six-vertex model under the domain-wall boundary condition describe alternating sign matrices in a very natural and unique way. It is this correspondence which equates the counting of distinct alternating sign matrices to the evaluation of the state sum of the six-vertex model. The determinantal form of the state sum was eventually published by Izergin, David Coker, and Korepin in 1992 (as well as in the 1993 book), so it perhaps should be referred to as the Izergin-Coker-Korepin solution.

—F. Y. Wu
Northeastern University

(Received August 25, 1999)

Comments on Howe's Review of Ma's Book

Roger Howe has written a thoughtful and stimulating review of Liping Ma's *Knowing and Teaching Elementary Mathematics* (*Notices*, September 1999). His main conclusions are that many of our teachers have insufficient understanding of mathematics and that upgrading their understanding should be a high priority. No mathematics educator I know would argue with either of these conclusions.

However, I found it strange that Howe seems unaware of what the mathematics education community is doing. He writes, for instance, about "how little this intuition [that mathematical knowledge of teachers plays a vital role in mathematics learning] seems to affect the agenda in mathematics education reform."

To whose agenda is Howe referring? The examples of good teaching to which Howe refers are precisely the type of thing that many mathematics education reformers are striving for, and we have been working hard to help teachers to teach with this level of insight. For instance, in the Interactive Mathematics Program (IMP), which I codirect, teachers participate in summer and after-school workshops in which they examine in detail the mathematics they are teaching. Many report that they finally understand the mathematics they studied in college. Moreover, we strongly encourage districts to provide IMP teachers with time on an ongoing basis, during the school day, to talk to each other about the mathematics in each unit and ideas to help their students learn the mathematics. (During the field-test phase of the program, we required districts to provide this time. Now we can only exert moral pressure, but many districts do continue to provide it.)

But improving the preparedness of teachers and changing the culture of the mathematics classroom are very complex tasks, and they must be approached on several levels simultaneously. In particular, it will be hard to motivate teachers to develop what Ma calls "PUFM" (profound understanding of fundamental mathematics) if we continue to have them teach a procedure-based curriculum, and if they and their students are evaluated using procedure-based tests that do not reward understanding. Teachers, like their students, will learn mathematics best if they see a purpose for the learning. For them to care about understanding mathematics, we must offer them a curriculum that emphasizes understanding. Moreover, in the long run, students who learn by way of a curriculum that emphasizes understanding will be in a better position to become teachers with PUFM.

In a separate matter, I disagree with Howe's statement that the traditional curriculum was a "major success" because it "allowed millions of people to be taught reliable procedures for finding correct answers to important problems, without either the teachers or the students having to understand why the procedures worked." There were also millions of people who did not learn those procedures, including many who dropped out of mathematics education at an early stage because they were not learning anything.

The second part of Howe's statement is equally significant. He acknowledges that during the reign of the "traditional curriculum", few people understood what they were doing. Many mathematics educators believe that this is the cause of the lack of PUFM in many of our teachers today, and it is the reason why they are working to change the way mathematics is taught.

I urge Howe and others with similar views to take another look at what IMP and similar programs are doing. I believe that Howe and others with similar views agree with our aims, and that once they understand what we are trying to do, they might also agree with our methods. Then we can work together on these issues, rather than waste time casting aspersions on each other.

—Dan Fendel
San Francisco State University

(Received September 14, 1999)

J. W. Alexander

I am preparing a biographical memoir of the Princeton topologist J. W. Alexander (1888–1971) and would be grateful for information about his life and work not already on public record.

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