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.

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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 tractable 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
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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
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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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