Born in Budapest, Peter Lax came to America at age 15. During his entire mathematical career he has been connected with New York University. He received his B.A. there in 1947 and his Ph.D. in 1949, under the guidance of Kurt Friedrichs. He then joined the faculty and rose to serve for 8 years as director of the Courant Institute. Among his many honors are a Chauvenet Prize from the MAA and a term as president of the AMS. His research in applied mathematics has focused on hyperbolic systems of differential equations and computational methods. Jointly with Ralph Phillips he is the author of two books on scattering theory. He is a member of the National Academy of Sciences and a recipient of the National Medal of Science. He served on the National Science Board from 1980–1986 and was a recipient of the Wolf Prize in 1987. He also received the Norbert Wiener Prize of the American Mathematical Society and the Society of Industrial and Applied Mathematics.

The Flowering of Applied Mathematics in America

PETER D. LAX

Mathematicians are notoriously bad historians; they describe the development of an idea as it should logically have unfolded rather than as it actually did, by fits and starts, often false starts, and buffeted by forces outside of mathematics. In this sketchy account of applied mathematics in America, I shall describe the twists and turns as well as the thrusts.

Applied mathematics is alive and well in America today; just look at the 18 lectures chosen to describe the frontiers of research: one is on physiological modeling, another on fluid flow and combustion, yet another on computer science; a fourth is devoted to the formation of atoms within the framework of statistical mechanics. The subject of one lecture and the starting point of several others are physical theories; the conclusions reached are of interest to physicists and mathematicians alike.

It was not always so; for a few decades, in the late thirties, forties and early fifties, the predominant view in American mathematical circles was the same as Bourbaki's: mathematics is an autonomous abstract subject, with no need of any input from the real world, with its own criteria of depth and beauty, and with an internal compass for guiding further growth. Applications come later by accident; mathematical ideas filter down to the sciences and engineering.

Most of the creators of modern mathematics — certainly Gauss, Riemann, Poincaré, Hilbert, Hadamard, Birkhoff, Weyl, Wiener, v. Neumann, — would have regarded this view as utterly wrongheaded. Today we can safely say that the tide of purity has turned; most mathematicians are keenly aware that mathematics does not trickle down to the applications, but that mathematics and the sciences, mainly but by no means only physics, are equal partners, feeding ideas, concepts, problems and solutions to each other. Whereas in the not so distant past a mathematician asserting "applied mathematics is bad mathematics" or "the best applied mathematics is pure mathematics" could count on a measure of assent and applause, today a person making such statements would be regarded as ignorant.

How did this change come about? Several plausible reasons can be discerned. But first a bit of selective history.

The second world war, a watershed for our social institutions, concepts and thinking, has permanently changed the status of applied mathematics in America. That is not to say that there was no worthwhile applied mathematics in America before 1945; after all, already in the 19th century, Gibbs' contributions to statistical mechanics as well as to vector analysis and Fourier series, and Hill's studies of Hill's equation, had put America on the applied mathematical map. The leading American analysts in the twenties and thirties were G. D. Birkhoff, renowned worldwide for his work in dynamics, and Wiener, a pioneer in the study of physical processes driven by chance influences, such as Brownian motion and homogeneous chaos. The elusive goal of the ergodic theorem was assiduously pursued in the thirties. The early forties saw the birth of Shannon's theory of information, and Pitts and McCollough's theory of neural networks. Nevertheless, it is fair to say that applied mathematics before 1945 did not fare well in departments of mathematics; it was a marginal activity.

A shift from the margin to the center began after the war; the trickle of applied mathematics swelled to a river. A recent survey of the substance and outlook of applied mathematics has been rendered by Garrett Birkhoff. In the brief span of this talk it is possible only to indicate the broad areas of advance, and to select, somewhat arbitrarily, a number of highlights. If I fail to mention your favorite result in applied mathematics, that only underlines the *embarras de richesse* in this domain.

Partly because of the influential book by Courant and Friedrichs, *Supersonic Flow and Shock Waves*, fluid dynamics was one of the first fields to undergo a renaissance. The basic existence theorems of steady subsonic flow in two dimensions around fixed bodies were established by Bers and Shiffman in the early fifties; much excellent work has appeared since about flows with free boundaries. The problem of steady supersonic flow, and of one-dimensional time-dependent flow, turned out to be more difficult, because of the possible formation of shock waves; the only definitive existence theorem is Glimm's in 1966. Morawetz has studied smooth transonic flows and even those with shocks; there are some results, and many tantalizing open problems.

Perhaps the most exciting new development is computational fluid dynamics, the construction by elaborate numerical calculations of approximations to flow fields. The purpose is two-fold: first to provide engineers with accurate performance characteristics of devices that are in contact with moving fluids, such as pipe systems, aerodynamic shapes, turbines, etc., for purposes of design or control. This approach has been used in more and more complicated situations: combustive flows, magneto-hydrodynamics, etc. The second purpose for doing fluid dynamical calculations is to give theoreticians clues about the possible behavior of fluids, to jog their imagination, in short: to experiment. Such clues have been used to study the complete or partial breakdown of solutions of the Navier–Stokes and Euler equations, and for many other investigations.

The recent spectacular advances in computational fluid dynamics were made possible by increased machine speed, larger memories, and better software, but even more by the invention of clever new numerical methods and algorithms, such as Chorin's use of discretized vorticity, and the fast Fourier Transform of Cooley and Tukey.

Of course, pure mathematicians, too, perform numerical experimentations; that is how Gauss was led to surmise the prime number theorem. He would have loved the computing facilities available today to number theorists, students of dynamical systems, etc. Reliance on fancy computing today creates a strong bond between the pure and the applied.

Equally great advances have been made in other branches of mathematical physics. In the early fifties Kato succeeded in proving that the Schroedinger operator for the helium atom (and other heavier atoms) is selfadjoint. In the midfifties, he and Rosenblum proved the existence of the scattering operator for a pair of operators that differ by an operator of trace class.

Keller's work on diffraction of waves was also begun in the fifties. Using geometrical optics Keller and his coworkers were able to derive mathematically a large number of diffraction patterns; some of these were proved rigorously only much later by Melrose and Taylor, by means of specially designed microlocal operators.

The classical field of dynamics received a jolt in the early sixties, when Moser showed the existence of infinitely many closed curves invariant under area preserving maps of annuli; this shows that such mappings — which include many of physical significance — are not ergodic.

Starting in the fifties there were impressive advances in solving some of the basic problems of statistical mechanics — existence of thermodynamical limits, phase transition, stability of matter. Much of this work was done by physicists, many of whom deserve the title of honorary mathematician. It was an honorary mathematician, Mitchell Feigenbaum, who discovered the doubling of stable periods of selfmappings of intervals as the mapping is deformed, and the universal character of the transfer of stability, a highly unexpected result.

Even more unexpected was Kruskal's discovery of solitons, their curious interaction with each other, and their relation to the existence of infinitely many conserved quantities, and the complete integrability of systems with soliton-like structures. It is astonishing that there are so many completely integrable systems — KdV, sine-Gordon, nonlinear Schroedinger, Toda, etc. — unrecognized as such in the classical days of Hamiltonian mechanics. It is doubly astonishing that they all have a measure of physical significance. That one of them, the Kadomtsev–Petviashvili equation, arising in the study of water waves, has led Dubrovin, Arbarello, DeConcini, and Shiota to a solution of Shottky's classical problem of characterizing Riemann matrices in the theory of Riemann surfaces is truly mindboggling.

Another example of mathematical physics lending a hand to pure mathematics is Faddeev and Pavlov's use of the notions of scattering theory to study automorphic functions.

A great achievement of the last fifteen years is computerized tomography, a lovely combination of inversion of an integral transform, harmonic analysis, and construction of fast and effective algorithms.

The postwar period saw the rise to prominence of the theories of probability and of partial differential equations. Each field stands on two legs, one firmly planted on applications, the other in purely mathematical considerations. Before the war, they were regarded as specialties; today they play a central role in large parts of mathematics.

There also arose entirely new fields of applications, such as the theory of games, control theory, operations research, linear programming, dynamic programming, integer programming, etc. The general aim of these disciplines is to optimize; therefore they have much in common with the calculus of variations. But there are substantial differences as well: these modern theories of optimization often deal with discrete rather than continuous models, and their targeted applications are novel, usually some aspect of economics, business or finance. Equally novel are the algorithms used to achieve the desired optimum in the shortest possible time. A strikingly effective algorithm — simulated annealing — has been borrowed by Kirkpatrick from metallurgy and statistical physics. Annealing is a process applied to amorphous material, glasses of various kind, where the energy of configurations has many minima. The absolute minimum occurs in a highly ordered state, called crystalline; if an amorphous material is cooled very rapidly, it solidifies into a highly disordered state corresponding to a local minimum far from absolute minimum. If the material is cooled slowly, it settles into the crystalline state.

There is a large class of combinatorial optimization problems — of which the traveling salesman problem is typical — which resemble amorphous materials in the sense that the objective function has a superabundance of minima. In such cases any descent method is likely to steer the configuration to a local minimum that is far from the absolute minimum. Simulated annealing operates with a sequence of temperatures T getting smaller and smaller; for each temperature there is a corresponding Gibbs distribution, where the probability of the jth state is

$$\frac{e^{-E_j/T}}{Z},$$

where E_j is the energy of the jth state and Z defined by

$$Z = \sum e^{-E_j/T}.$$

The Metropolis algorithm is used to construct a sequence of states in equilibrium with the Gibbs distribution, as follows: the configuration is changed, according to a chosen recipe. If the new configuration has lower energy, accept the change; if the change in energy ΔE is positive, accept the change with probability $e^{-\Delta E/T}$; this part of the algorithm is implemented by a Monte Carlo method, employing a random sequence. After this algorithm has run for a certain time, the temperature is lowered and the algorithm sequence repeated. This procedure has been remarkably effective for finding excellent approximations to minima in a number of combinatorial optimization problems.

Computer science has been the source of much novel mathematics. It has focused attention on algorithms and has come up with many astonishingly efficient ones, such as the fast Fourier transform, fast matrix multiplication, the simplex method, and many more, described in Knuth's magnum opus. More recent are Karmarkar's algorithm, and Greengard and Rokhlin's method for the fast evaluation of potentials. It is often difficult to estimate the efficiency of an algorithm, especially if it works better in the typical case than in the worst case; see Smale's penetrating study of the simplex method.

An important problem is to design networks that perform efficiently sorting, parallel processing, and other such tasks. Such graphs, called expanders and concentrators, have the same number of edges issuing from each vertex and have good connectivity properties; the task is to construct concentrator graphs with as small a number of edges as possible. Sarnak and his coworkers Lubotzky and Phillips have explicitly constructed a family of expanders with nearly minimal number of edges, which they call Ramanujan graphs, since the proof that they have the desired property depends crucially on a conjecture of Ramanujan concerning the representation of numbers as linear combinations of four squares, as well as on delicate harmonic analysis on groups.

Computational complexity deals with the limits of cleverness, i.e., what are the fastest possible algorithms for evaluating a class of functions? There are few answers as yet to such deep questions; see, for example, Winograd's study of multiplication.

Perhaps the most fascinating area of computer science is artificial intelligence, with its implied threat to put out of business both pure and applied mathematicians of the human kind. But I am bothered by the widespread habit of some parts of the AI community to set their goals preposterously high, and to exaggerate past achievements. For instance, in his Gibbs lecture delivered in 1984, Herbert Simon described a computer program named BACON, designed to extract scientific laws from experimental data, without the benefit of theory, by a kind of curve fitting. He claimed three successes for BACON, the first the derivation of Kepler's third law of planetary motion, which Simon states as:

$$P = KD^{3/2}$$

where P is the period of revolution of the planet, D its distance from the sun, and K a constant that has the same value for all planets. This formulation is meaningful only for planets whose distance from the sun is constant, i.e., whose orbit is circular. Kepler's law on the other hand concerns *elliptic* orbits; he sets D equal to the arithmetic average of the closest and farthest distance of the planet from the sun:

$$P = K\left(\frac{D_1 + D_2}{2}\right)^{3/2}$$

That is, Kepler has found an expression for the period of any planet as function of the *two* parameters characterizing the planet's elliptic orbit. This is worlds away from finding the period of planets with circular orbits. If a mathematician proved a theorem in the spherically symmetric case, he wouldn't dream of claiming the general case; computer scientists must hold themselves to the same standard of precision. There are, to be sure, more profound objections to Simon's paradigm for AI — for example, those voiced by Grabiner and by Edelman.

The most curious — and controversial — of the new applied branches is catastrophe theory, the brainchild of the great mathematician René Thom. A sympathetic presentation of the epistemology of this theory is given in Ekeland's charming new popular book, *Mathematics and the Unexpected*, and in the treatise of Poston and Stewart. More jaundiced views are expressed, in deepening shades of yellow, by Arnold, Guckenheimer, and Sussmann.

Catastrophe theory already has some solid achievements to its credit, and I believe that more are to come. The hostility to the subject is a reaction to attempts to oversell it, like snake-oil medicine, good for whatever ails you. The applications touted by the popularizers were often flaky, and their novelty exaggerated. For instance, Zeeman, in his *Scientific American* article in 1976 wrote:

> For 300 years the preeminent method in building such models has been the differential calculus invented by Newton and Leibnitz. Nevertheless, as a descriptive language, differential equations have an inherent limitation, they can describe only those phenomena where change is smooth and continuous. In mathematical terms, the solutions to a differential equation must be functions that are differentiable. A mathematical method for dealing with discontinuous and divergent phenomena has only recently been developed.

This is strange talk in the age of the theory of distributions! Besides, the theory of discontinuous solutions is much older than the American Mathematical Society. The basic laws of shock waves, which are discontinuous solutions of nonlinear partial differential equations, were set down by Riemann 130 years ago.

Having described some of the achievements of applied mathematics, I would like to discuss briefly its methods. Some of them are organic parts of pure mathematics: rigorous proofs of precisely stated theorems. But for the greatest part the applied mathematician must rely on other weapons: special solutions, asymptotic description, simplified equations, experimentation both in the laboratory and on the computer. Out of these emerges a physical intuition which serves as a guide to research. Since different people have different intuitions, there is a great deal of controversy among applied mathematicians; it is a pity that these debates so often become acrimonious, shedding more heat than light.

We come back now to the question: What were the causes of the flowering of applied mathematics in America after World War II? Perhaps the most important factor was the war itself, which demonstrated for all the crucial importance of science and technology for such projects as radar, the proximity fuse, code breaking, submarine hunting, and the atomic bomb. Mathematicians, working along with physicists, chemists and engineers, made substantial — in some cases decisive contributions; without these developments, the United States might have lost the war. Those responsible for science policy after the war remembered this lesson well and applied it farsightedly and with imagination. They realized that applied science is a basic ingredient of technology, that applied mathematics is an essential component of applied science, and that all parts of mathematics, the pure and the applied, form an organic whole. Consequently, the U. S. Government started a vigorous program to support mathematics, for project-oriented work at government laboratories, for research at universities. A wide variety of subjects were encouraged, for a wide variety of reasons. Some, like numerical linear algebra, or the propagation of electromagnetic waves, were supported for use in immediate applications; others, like the theory of partial differential equations and statistics, because they were underdeveloped compared to their importance; others simply because they were part of the fabric of mathematics. The first agency to systematically support science and mathematics was the Office of Naval Research; it was followed somewhat later by similar offices of the Air Force and the Army, and the Department of Energy in its previous incarnation as Atomic Energy Commission. In addition to supporting a string of mathematicians whose names read like a Who's Who, these agencies were instrumental in the establishment of the School of Probability at Cornell, the School of Applied Analysis and Statistics at Stanford, and the Courant Institute at New York University. The National Science Foundation, coming somewhat later, took over many of the outlooks of its predecessors, as well as forming its own philosophy and point of view. Support of mathematics by the DOD continues to this day, supplementing and complementing support by the NSF and other agencies.

In view of the distinguished past and present success of this research program, it came as an utter surprise that a group within the AMS proposed to reduce support for mathematics by the DOD. Many who were supported by the DOD were deeply offended by the suggestion that they were accepting money from a tainted source, and that the support should have been given to worthier recipients.

The program to build up mathematics in general, and applied mathematics in particular could not have succeeded as well as it did without leadership in the mathematical community. Leadership in applied mathematics was largely provided by a remarkable group of immigrants, mostly refugees from Europe, such as Courant, Feller, Friedrichs, John, Kac, Kato, v. Karman, v. Mises, v. Neumann, Neyman, Prager, Schiffer, Synge, Ulam, Wald, Weyl and others. This group brought to these shores outlooks and styles that were radically different from the purity then prevailing, in particular a greater affinity for applications of mathematics to physics and engineering. Many of the newcomers were in their prime, and were able to put forward their ideas with vigor and confidence.

v. Neumann was a key figure among this illustrious coterie. There is hardly an area of applications that doesn't bear his stamp. In a prophetic speech in Montreal in 1945, when electronic computers were merely figments of his imagination, he declared that "many branches of both pure and applied mathematics are in great need of computing instruments to break the present stalemate created by the failure of the purely analytical approach to nonlinear problems." v. Neumann was a key figure in the American Mathematical

Society; his tragic, premature death has deprived applied mathematics and computer science of a natural leader, a spokesman, and a bridge to other sciences.

It is impossible to exaggerate the extent to which modern applied mathematics has been shaped and fueled by the general availability of fast computers with large memories. Their impact on mathematics, both applied and pure, is comparable to the role of telescopes in astronomy and microscopes in biology; it is a subject fit for another lecture; here I have time only for a few observations:

In the bad old days, when numerical work was limited to a few hundred, or few thousand, arithmetic operations, the task of the applied mathematicians called for drastic simplification, even mutilation, of their problems, to fit the available arithmetic capabilities; they had to cut every corner, exploit every accidental symmetry. Such expediency did not appeal to the mathematical mind, and probably had a great deal to do with the unpopularity of applied mathematics in the days before computers. Today we can render unto the computer what is the computer's, and unto analysis what is analysis', we can think in terms of general principles, and appraise methods in terms of how they work asymptotically for large n, rather than for $n = 8, 9, 10$!

There are many kinds of calculations carried out today, for many different purposes; the confidence we can place in them varies from case to case. Some of the most striking model truly chaotic phenomena such as multiphase flow, turbulent combustion, instability of interfaces, etc. Such calculations use discrete analogues of physical processes, and are very often fine tuned to resemble experimental results. For me, there is something unsatisfactory when a computational scheme usurps the place of a theory that ought to be independent of the parameters entering the discretization.

The applied point of view is essential for the much needed reform of the undergraduate curriculum, especially its sorest spot, calculus. The teaching of calculus has been in the doldrums ever since research mathematicians have given up responsibility for undergraduate courses. There were some notable exceptions, such as Birkhoff and Mac Lane's "Modern Algebra", and Kemeny, Snell, and Thompson's "Finite Mathematics", but calculus, in spite of some good efforts that did not catch on, has remained a wasteland. Consequently the standard calculus course today bears no resemblance to the way mathematicians use and think about calculus. Happily, dissatisfaction with the traditional calculus is nearly universal today; there are very few doubting Thomases. This welcome crisis was brought on by the widespread availability of powerful pocket calculators that can integrate functions, find their maxima, minima, and zeros, and solve differential equations with the greatest of ease, exposing the foolishness of devoting the bulk of the calculus course to antiquated techniques that perform these tasks much more poorly or not at all. We now have the opportunity to sweep clean all the cobwebs and dead material that clutters up calculus. We have to think carefully what we put in its place; I strongly believe that calculus is the natural vehicle for introducing applications, and that it is applications that give proper shape to calculus, showing how and to what end calculus is used. UMAP is an excellent source of such applications.

No doubt computing will play a large role in undergraduate education; just what will take a great deal of experimentation to decide. The brightest promise of computing is that it enables students to take a more active part in their education than ever before.

I would like to direct a comment at enthusiasts for discrete mathematics, a subject of great beauty and depth, which has gained enormous importance for applications because of the availability of computers. But it is mistaken to think that discrete mathematics should compete with or even replace calculus-based applied mathematics in the elementary undergraduate curriculum; this would disregard the explosive growth, thanks to computing, in our ability to bring calculus-based mathematics to bear on applications.

How can one resist the temptation to make guesses about the directions of future research? I am on the safest ground in surmising that computing will play an even bigger role in the next century than today. Mathematical modelers will explore their subjects in the manner of experimentalists. We shall enjoy routinely graphic display capabilities that would dazzle us today. We shall learn to use computations as an ingredient of a rigorous proof, a road already taken by Fefferman and Lanford. I am confident that fluid dynamics will be regarded as a central discipline, and that the elusive goal of understanding turbulence will be vigorously pursued. We will no doubt try to digest the large amount of chaos generated lately, by trying to extract information concerning average behavior. This can sometimes be done in completely integrable cases; the KAM theory gives reason to believe that such results have relevance for systems not too far from integrable ones.

I heartily recommend to all young mathematicians to try their skill in some branch of applied mathematics. It is a gold mine of deep problems, whose solutions await conceptual as well as technical breakthroughs. It displays an enormous variety, to suit every style; it gives mathematicians a chance to be part of the larger scientific and technological enterprise. Good hunting!

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