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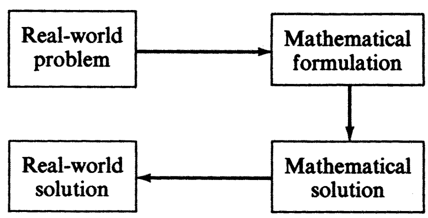
THE ROLE OF APPLICATIONS IN PURE MATHEMATICS

DOROTHY L. BERNSTEIN

In recent years, the MAA and other mathematical groups have paid considerable attention to applied mathematics and the use of mathematics in natural and social sciences and in government and industry. A glance at the programs of the national and sectional meetings of the past few years will show a generous sprinkling of hour-long talks and panels dealing with applied topics. The usual diagram that is supposed to represent the role of mathematics in applications goes something like this:



Of course, one might proceed by experiment or observation directly from the real-world problem to the real-world solution; but this may be impossible or prohibitively expensive, and so the fruitful detour through mathematical models is taken. I have no quarrel with this general description, although I point out that it says nothing about who formulates the mathematical problem, who solves it, and who interprets the solution in real-world terms. I am concerned with the impression it leaves, either expressed or implied, that this is all irrelevant to the development of pure mathematics. Mathematicians stand over here, inventing axiom systems and abstract spaces and proving theorems about them, which are available for use when needed in the above scheme. Physicists and biologists and economists and all the others are over there, proposing their problems and then gratefully receiving the solutions.*

Dorothy L. Bernstein was an undergraduate at the University of Wisconsin in Madison, where she began her graduate work; she continued her graduate study at Brown University, writing her dissertation on "The Double Laplace Integral" under Professor Jacob Tamarkin, and receiving her Ph.D. in 1939. Since September 1937, she has been teaching full-time in colleges and universities, except for sabbatical leaves for research. She has taught at Mount Holyoke College, the University of Wisconsin, the University of California (Berkeley), the University of Rochester, and Goucher College. Her sabbaticals have been spent at the Institute of Advanced Study, University of California (Los Angeles), Brown University, and the University of Tennessee. At present she is Chairman of the Mathematics Department at Goucher College. Her mathematical interests are in analysis, especially integration, measure theory, and partial differential equations. She also has a strong interest in probability and statistics, and in the use of computers in the mathematics curriculum. She has been active in the affairs of the MAA; she was First Vice-President in 1972–73, and has been President since January, 1978. This paper is based on talks given to the Southeastern Section and the Iowa Section of the MAA, in April 1978.—*Editors*

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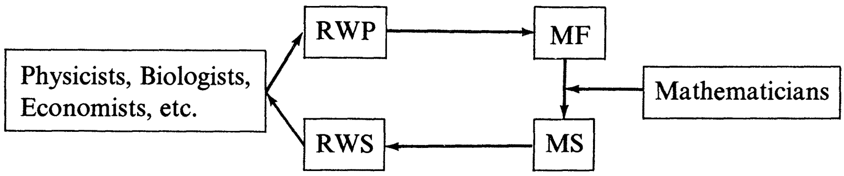
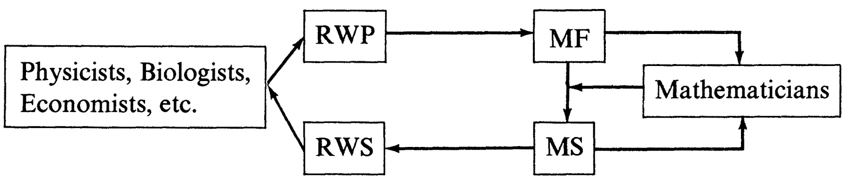
*This point of view is expressed in the following quotation, translated from a letter from Hermite to Stieltjes, 28 November 1882: "I am only an algebraist and have never gone outside the domain of pure mathematics. I am nevertheless completely convinced that the most abstract speculations of Analysis are evidence of realities that exist outside ourselves and will eventually come to our attention. I even think that the work of the pure geometers is directed, without their being aware of it, toward such an end, and the history of Science seems to me to prove that a mathematical discovery comes about at the precise time when it is needed for each new advance in the study of those phenomena of the real world that are amenable to calculation."



It is my thesis that there is something missing from this flow-chart. Real-world problems and their mathematical formulation often are the source of interesting problems in pure mathematics, and mathematical solutions of these models are sometimes the stimulation for generalizations which yield important concepts and theorems in mathematics. I propose that we examine some specific areas of mathematics in which applications have played an important part and then try to draw some general conclusions about the role of applications in mathematics (let's drop the "pure"—I'm never quite sure what it means in a given discussion).



Example 1. Our first example is geometry. As its name indicates, it began as an empirical science, that of land measurement by the Egyptians. Because of the flooding of the Nile, the size of a man's farm was never the same from one year to the next; and since the amount of taxes he paid was determined by how much land he owned, it was extremely important to have procedures for accurate measurement of land. It was the Greeks, culminating in Euclid, who made geometry into a postulational system with theorems and proofs that was a model of mathematical thinking for the next 2,000 years. However, it was still considered a description of the real world, and much of its authority during this long period came from this fact. In the eighteenth and nineteenth centuries, mathematicians failed in attempts to prove Euclid's fifth postulate by showing that its denial led to a contradiction. Instead, there came a gradual realization, probably first by Gauss, that the hyperbolic geometry of Saccheri, Bolyai, and Lobachevsky did not lead to a contradiction. In fact, as Klein, Beltrami, and Poincaré showed, both hyperbolic geometry and the elliptic geometry of Riemann and Schläfli were as logically consistent as Euclidean geometry. All were abstract mathematical systems in the modern sense, each with its own power and beauty, but none with a special claim as a description of the physical space we inhabit. Cayley was able to show that elliptic geometry was closely related to projective geometry, which had also been developed during the eighteenth and nineteenth centuries [**1**].

In the first part of the twentieth century, however, precisely at the time mathematicians were completing the abstraction of geometry, the flow was reversed. Einstein, looking for a basis for his genéral theory of relativity, found it in the geometry of Riemann. The idea that physical space is finite but unbounded is a cliché of modern-day physics; but, to quote the physicist Freeman Dyson, "Einstein took the revolutionary step of identifying our physical space-time with a curved non-Euclidean space . . . on the basis of very general arguments and aesthetic judgments. The observational tests of the theory were made only after it was essentially complete, and they did not play any part in the creative process. Einstein himself seems to have trusted his mathematical intuition so firmly that he had no feeling of nervousness about the outcome of the observations. The positive results of the observations were, of course, decisive in convincing other physicists that he was right" [**2**]. That it was possible for the same man to hold both points of view is illustrated by Hilbert, who made important contributions to both axiomatic geometry and to general relativity.

And now there is a final swinging of the pendulum. Says Roger Penrose [**3**]: "The debt to pure geometry that relativity had owed has now been amply repaid. For many of the ideas of the modern subject of differential geometry received their initial stimulus from concepts arising from Einstein's general relativity." These include manifolds, tangent spaces, and parts of complex geometry.

Example 2. My second example is the area of numerical calculations. John Napier invented logarithms for the very practical and utilitarian purpose of making multiplication easier by essentially replacing it by addition. Nevertheless, logarithms and the number e, which is the base of natural logarithms, became indispensable in the development of modern analysis. Infinite series were used as an aid to calculation long before anyone worried about convergence, and before they assumed their central role.

Indeed, the whole subject of numerical analysis has developed right alongside mathematics. According to Philip Davis, numerical analysis is a branch of both applied mathematics and computer science. It formulates algorithms, analyzes errors (not blunders), studies rates of convergence, and compares algorithms. Such work, in this day of computers, is very important. But its real influence on mathematics itself has been to change its flavor. Instead of relying almost exclusively on existential methods, as was the case 50 years ago, mathematicians are now also using constructive or algorithmic methods, under the stimulating necessity of getting numerical solutions.

Of course, computers have changed the way both scientists and mathematicians regard numerical computation. For the former, they have opened up previously inaccessible areas to mathematical and numerical treatment. For the latter, they have changed what we consider important. Some of you have heard Henry Pollak talk on this subject. I would like to illustrate it by a simple example which I first heard from the late George Forsythe: The usual solution of the quadratic equation $ax^2+bx+c=0$, where $ac\neq 0$, is, setting $\Delta=b^2-4ac$:

$$\text{(1a) } x_1=\frac{-b+\sqrt{\Delta}}{2a}, \qquad \text{(1b) } x_2=\frac{-b-\sqrt{\Delta}}{2a}.$$

However, since neither root of the quadratic is 0, an equivalent equation can be found by dividing by x^2 and setting $t=1/x$: $ct^2+bt+a=0$. If we find the roots t_1 and t_2 of this equation and then take reciprocals, we get alternative formulas for x_1 and x_2:

$$\text{(2a) } x_1=\frac{2c}{-b-\sqrt{\Delta}}, \qquad \text{(2b) } x_2=\frac{2c}{-b+\sqrt{\Delta}}$$

Mathematically, the two sets of formulas give identical results. But due to round-off errors and the inexact process of taking square roots on a computer, there are times when one choice is much more nearly exact than the other. Indeed, Forsythe proves that one should use (1a) and (2b) when the coefficient $b\geqslant 0$, and (2a) and (1b) when $b<0$.

Example 3. Calculus, and the whole group of ideas which followed it and which we lump under the heading of analysis, or theory of functions, is one of the cornerstones of modern mathematics. It began, quite specifically, with attempts to develop a theory to account for the observations and quantitative measurements of various phenomena made by Galileo, Copernicus, Kepler, and others in the sixteenth and seventeenth centuries. Galileo found that the distance traveled by falling bodies is proportional to the square of elapsed time of fall. Kepler made some early attempts at integration in order to measure the volume of kegs, or bodies bounded by curved surfaces. Finally, Isaac Newton, in his *Principia Mathematica*, stated in mathematical form certain physical principles which he said governed *all* matter. His law of universal gravitational attraction and his three laws of motion became the foundation of the science of mechanics. In the course of developing these laws he invented his calculus of fluxions as a tool. The concepts of *Principia Mathematica* were quickly accepted in the Western scientific world, and are still, according to the meteorologist P. D. Thompson, "the keystones in the study of the behavior of physical systems in natural environments encountered on earth" [4].

But for 150 years after Newton there was only an inexact semi-physical formulation of the calculus, although during that period some very significant advances were made. Often the same men worked in both mathematics and mechanics. Thus Euler and D. Bernoulli studied the mechanical properties of gases and liquids, as well as making fundamental contributions to mathematics. Euler, Gauss, Jacobi, Cauchy, Riemann—all had varying standards of abstraction and rigor in the mathematics they did. It was not until Weierstrass that analysis became abstract and unempirical. (I wonder how much of this was because Weierstrass had been a lawyer before he became a mathematician.)

Example 4. My fourth example is the body of mathematics lumped under the term *Fourier series*, or more generally, harmonic analysis. When Fourier was studying the phenomenon of heat conduction in the early nineteenth century, he realized that trigonometric functions like $\sin x$, $\cos 2x$, $5\sin 3x$, were periodic and had different periods and amplitudes, so that linear combinations of them could represent fairly intricate periodic phenomena. But it turned out that what he needed was a representation of a function $f(x)$ by an infinite series of such terms:

$$f(x) \sim \sum (a_n \cos nx + b_n \sin nx),$$

which we have since come to call Fourier series. Although, by that time, power series expansions of the form

$$f(x) \sim \sum a_n x^n$$

were fairly well understood, such an expansion requires that f have derivatives of all orders in the neighborhood of the origin, since $a_n = f^{(n)}(0)/n!$. On the other hand, Fourier series could represent functions which were continuous, or even piecewise continuous (if you accepted the mean value at points of discontinuity as representing the function), since formally at least the coefficients are of the form

$$b_n = \frac{1}{\pi}\int_0^{2\pi} f(x)\sin nx\,dx, \qquad a_n = \frac{1}{\pi}\int_0^{2\pi} f(x)\cos nx\,dx, \qquad a_0 = \frac{1}{2\pi}\int_0^{2\pi} f(x)\,dx,$$

and this depends in turn on relations of the form

$$\int_0^{2\pi} \sin nx \sin mx\,dx = 0, \qquad \int_0^{2\pi} \sin nx \cos mx\,dx = 0, \qquad \int_0^{2\pi} \cos nx \cos mx\,dx = 0 \qquad (n \neq m).$$

The study of the convergence properties of Fourier series, and of which functions could be represented by them, interested many mathematicians during the nineteenth century and led them in surprising directions. For example, Georg Cantor was working on this when he found he had to deal with the infinite sets of numbers at which such a series could converge; from this he was led to the study of infinite sets in general and his far-reaching work on cardinal and ordinal numbers. Henri Lebesgue, on the other hand, was intrigued by the fact that the usual Riemann integral was not good enough to allow him to compute the Fourier coefficients of general functions $f(x)$, and so he extended the notion of the integral to take care of this—the indispensable Lebesgue integral.

While the study of Fourier series went on during the nineteenth century, many other series of special functions with analogous properties were discovered, often in connection with a particular physical problem and its resulting differential equation, associated with names like Legendre, Jacobi, Laguerre, Bessel, and Hermite. In each case, one had a denumerably infinite set of functions orthogonal to each other—the integral of the product of two different functions of the set over the basic interval was 0—allowing expansions of fairly arbitrary functions in infinite series of terms from this set. All of these became known as Fourier series; and they have played a fundamental role in the modern development of vector spaces, since they furnish nontrivial examples of complete orthonormal bases in certain inner-product spaces. In addition, they have led to many important generalizations.

Example 5. The use of differential equations to describe physical processes has gone on since Newton first stated, in his second law of motion, that when a point-mass moves in a given direction under the action of a force its acceleration, the second derivative of distance, is proportional to the force. In my previous example, I referred to the solution of certain kinds of second-order differential equations, called Sturm-Liouville equations, which were of great interest to nineteenth-century mathematicians.

When we consider linear nth order equations, with constant coefficients, we are immediately struck by their analogy to nth order algebraic equations, obtained by replacing the kth order derivative by the kth power of a variable. It was an engineer, Oliver Heaviside, who first exploited this in a systematic way at the end of the nineteenth century, to get a set of arbitrary rules for solving such differential equations, which he called the operational calculus. Many mathematicians were horrified since there was absolutely no theoretical justification for what he was doing; but it worked, and more and more engineers, physicists, and chemists used it. Slowly, mathematicians, beginning with Bromwich in Australia and then Plancherel in France, Doetsch in Germany, Wiener and Widder in the United States, realized that if one applied a certain linear operator to a function $F(t)$, which consisted in multiplying by e^{-st} and integrating over the interval $[0,\infty)$ the resulting function $f(s)$ (called the Laplace transform of F) behaved in such a way that differentiation of F with respect to t corresponded essentially to multiplication of $f(s)$ by s. Consequently, a differential equation in F became an algebraic equation in $f(s)$, which was then solved and the inverse transform evaluated by the theory of residues. At about the same time other integral transforms, such as the Fourier transform and the Mellin transform, were developed. I should mention, by the way, that Laplace first used the transform which bears his name in his great work on celestial mechanics.

One can describe the motion of a point in the plane by a simple system of differential equations which represent the components of force applied to the x and y coordinates, and get solutions of the form $(x(t),y(t))$. Early in the twentieth century, Poincaré made a revolutionary study of the path of such a point considering the solutions as giving parametric equations of the path. He was particularly interested in its asymptotic behavior—what happens as t becomes large—and showed that in general it led to either singular points or limit cycles. The study of such singular points and limit cycles, called the Poincaré-Bendixson theory, has led to solution of many engineering problems in servo-mechanisms and automatic control. It has also had a profound effect on what has come to be called the qualitative theory of differential equations and the theory of differential equations in the large. In fact, this is one area where the distinction between pure and applied mathematics disappears completely, as anyone recognizes who has been at the Center for Dynamical Systems at Brown.

Example 6. My next example is graph theory, which started in a mathematical sense with Euler's Königsberg bridge problem in 1735, but where the earliest contributions were made by Kirchhoff in 1847 with his study of electrical networks, and by Cayley in 1857 with his work in organic chemistry. A graph, let me remind you, is a diagram consisting of a finite number of points, some of which are connected by edges, usually directed. The famous problem of whether you can color any plane map using at most 4 colors belongs in this area—it was proposed to de Morgan by Guthrie in 1850. Interest in the subject of graphs among mathematicians fluctuated over the next hundred years. Among those who contributed to graph theory during this period, let me mention Kuratowski in Russia and Whitney and MacLane in the United States. Since 1950, there has been a steady increase in interest in the subject, due to its many applications. It has been said that physics, chemistry, genetics, psychology, economics, engineering, operations research, all use the results of graph theory. The important question to computer and electronic engineers of when an electrical circuit can be realized as a printed circuit, for example, has led to an extensive analysis of the corresponding problem in graphs.

A look at *Studies in Graph Theory*, volumes 11 and 12 of the MAA series, edited by Fulkerson, will give you an idea of the present ramifications of the subject. Fulkerson himself has written a paper on flow network and combinatorial operations research. A. J. Hoffman has a paper on eigenvalues of graphs where he uses the notion of adjacency matrices. Whitney and Tutte have an interesting paper on Kempe chains and the four-color problem. By the way, Appel and Haken at the University of Illinois have apparently proved the four-color conjecture, using a computer. I say "apparently" because it brings up the very interesting question of just what constitutes a proof in this age of computers.

Example 7. Twentieth-century physics has become completely intertwined with mathematics. I have already referred to general relativity theory. Another basic development has been quantum mechanics. When Cayley invented matrices in the nineteenth century, he prided himself on their uselessness; but in 1925, Heisenberg found them to be precisely the tool he needed to describe his conception of atomic structure—so-called matrix mechanics. Schrödinger, at about the same time, used the Sturm-Liouville theory of differential equations to describe his concept of atomic structure—so-called wave mechanics. The two theories were soon recognized as equivalent; indeed, in 1927, von Neumann unified the theory in terms of linear operators in a complex Hilbert space.

Group theory, which originated in the study by Lagrange and Galois of symmetry and symmetric transformations on the roots of an equation, became a central idea in algebra, and, indeed, in all of mathematics, during the nineteenth century. In the twentieth century, the group became a fundamental concept in the mathematical description of the physical world. For example, crystallography begins with the description of all possible space groups—230 of them. More recently, group theory has been used in the description of the elementary particles. It provided a key to the classification of recently discovered particles, called the 8-fold way, which enabled Gell-Mann and Ne'eman to predict the existence of a certain baryon before it was discovered experimentally in 1964. In turn, the mathematical study of continuous Lie groups and their resulting Lie algebras have been stimulated by these physical applications. (I should mention in passing that recent interest in large finite simple groups has been stimulated by problems in algebraic coding theory.)

The theory of analytic functions of a complex variable has been called the greatest achievement of nineteenth century mathematics. And, of course, its applications to physics and engineering have been numerous. But the theory of functions of several complex variables seemed to belong to the domain of pure mathematics. There were some early results by Weierstrass, Poincaré, and Hartogs, but then interest languished until after World War II. Since then there has been an awakened interest and much fundamental work has been done, largely because of the requirements of quantum field theory. It turns out that the probability distributions of collisions of elementary particles are best described by piecewise analytic functions of several complex variables. The theory of residues has been extended to integrals over hypersurfaces in n-dimensions—the so-called Feynman integrals were invented by a physicist.

Example 8. Probability theory started with the absolutely practical request of a gambler to Vieta and Pascal to explain the odds in tossing two coins. Much work was done on Bernoulli trials—repeated experiments where the probability of success in a single trial is constant, and also with the celebrated normal curve which appeared over and over as an empirical law. However, according to Mark Kac, after Laplace probability almost disappeared as a mathematical discipline in the Western world until the 1920's, even though during the nineteenth century physicists like Maxwell and Boltzmann used it spectacularly in the study of Brownian motion. (Exception must be made for the work of Russian mathematicians like Chebysheff and Markov.) But then Poincare and Hilbert revived interest in the subject. Poincaré's little book on the calculus of probability is still a model of clear exposition which I recommend to any graduate student preparing for a French exam. And with the work of Kolmogoroff in 1935, the theory of probability finally became a precise and sophisticated discipline, based on Lebesgue measure; Paul Lévy and Norbert Wiener also played significant roles in its development.

The subject of statistical mechanics, which had been studied by Boltzmann and Gibbs, gave rise to the ergodic hypothesis—that a quantity depending on a mechanical system with a large number of components would have identical averages over *time* and over *space*. Mathematicians like von Neumann and G. D. Birkhoff gave a clear mathematical formulation of this hypothesis and proved the first theorems in 1931. Then one of the familiar stochastic variables arising from independent repeated trials (Bernoulli trials) was shown to be an example of an ergodic system **[5]**. During the past twenty years, students of Kolmogoroff have generalized another physical concept—that of entropy—which was useful in the study of such probabilistic systems and is now being used to tackle other problems in statistical mechanics. There is a clear exposition of this entire matter in a series of lectures given by Sinai at Moscow State University, and translated in 1977 by V. Scheffer **[6]**.

Example 9. Some economists, like Lawrence Klein, say flatly that economics is a mathematical discipline. They point to the use of optimization, game theory, linear programming, and mathematical models of price equilibrium (now being rephrased in probabilistic terms). However, I shall turn for my next example to the general theory of optimization which has had applications to, and draws from, operations research, managerial science, control theory, statistics, and mathematics. Optimization became viable with the computer; in fact, it has been estimated that one-fourth of all scientific computing involves optimization. According to Dantzig and Eaves, mathematical optimization does four things: (1) develops a mathematical structure called a program, which models a real world situation; (2) investigates existence and attributes of optimal, or near optimal, solutions, and how to characterize them; (3) designs algorithms for the computation of optimal solutions; and (4) implements mathematical solutions in a particular application, evaluates results and then modifies it. In volume 10 of the MAA series called *Studies in Optimization*, which appeared in 1974, Dantzig and Eaves say "Optimization theory is a fertile ground for new and promising problems, problems upon which to build new mathematical theories. It could be a source of new, exciting, and relevant problems which would motivate the mathematical student." Besides this volume, containing papers by Kuhn, Tucker, and the two editors, one can also look at "Towards Global Optimization" by L. Dixon and G. P. Szego.

Example 10. I have left my own field, partial differential equations, for the last, and I shall discuss it briefly. Following well-known physical prototypes (wave mechanics, heat conduction, potential theory) boundary value problems in partial differential equations with constant coefficients were classified into three general types: hyperbolic, parabolic, and elliptic. The same classification was easily generalized to linear p.d.e. with variable coefficients, and boundary value problems were proposed and solved, and in some cases, applied to solve real-world problems in many areas. Meanwhile, the theory was extended to quasilinear and to non-linear p.d.e.; in each case, the classification into the three types was preserved, sometimes without real justification. Using the notion of a generalized function called a *distribution* invented by Laurent Schwartz 25 years ago, the concept of a solution was extended and this in turn was helpful in solving many physical problems. The physical notions of conservation of energy and momentum were used as the basis for studying a whole class of equations known as conservation laws; solutions for boundary value problems for such equations were obtained in terms of distributions. The concept of boundary layers, taken from aerodynamics, led to a general theory of singular perturbations, but much remains to be done in the way of a general existence theory of partial differential equations.

I could cite other examples, such as the whole area of statistics and experimental design; but instead, let us try to formulate some general conclusions:

1. There has been a continuous and fruitful interplay between science and mathematics from the very beginning. Von Neumann, in his essay "The Mathematician" [7], classifies science into three groups: descriptive, experimental, theoretical. Or rather, these are three stages through which a science passes. The descriptive ones call most on mathematics and have the least to give. The experimental sciences use mathematics freely and begin to return the investment. In the theoretical sciences, there is a free interchange of ideas with mathematics. Notice, I said interchange, not identification. A mathematician's motives are internal: his intellectual curiosity, his sense of form and pattern, his taste. A scientist, even a theoretical one, finds his motives elsewhere, as von Neumann points out.

2. No one can predict what mathematics will become useful for applications or when this will happen.

3. Mathematical ideas may originate as abstract concepts and then have useful applications or they can originate in the context of applications and be generalized to abstract concepts.

Well, what about the future? The danger of overspecialization that many people have pointed out is not one that bothers me. At present, most of us are willing to sit and listen to what others have to say about advances in their fields. But that is not the same as becoming creatively interested. I was reading the book by F. D. Thompson in which he describes the remarkable breakthrough that occurred in numerical weather prediction during the period 1951–55 [**4**]. In 1949, von Neumann had organized a special group at the Institute of Advanced Study to go over the partial differential equations of weather prediction, which had been formulated by Richardson in 1922. Computers had been introduced, but they could only handle data. By the time the programmers had written the instructions necessary to handle the data collected from weather stations, it was too late to predict the weather for a given day. It was under the pressure of this problem that he conceived the revolutionary idea of storing programs as well as data—that is, essentially letting the computer decide how to handle data. With this established, the project was under way and, according to Thompson, "It was a small dedicated group of people who knew something about meteorology, physics, mathematics, numerical analysis, and computer science who were able, in the short period of four years, to completely solve the problem of mathematical weather prediction." Incidentally, now it is a new ballgame, since one considers relativistic hydrodynamics of probabilistic solutions.

I have said nothing about new problems facing our society in conserving energy and handling the environment, nor about the whole range of biomedical problems. All of these need new mathematical ideas, and I am convinced that it is only by a combined attack like that just described that they can be handled. How should this be accomplished? I am not a believer in giant national programs; I think interested individuals can accomplish more. To the young Ph.D.'s I say: Talk to your friends—the young men and women of your own age in physics, biology, economics, anthropology, psychology. Find out what they are interested in—at some point, you will find a common bond with someone, and from then on, with both of you talking furiously and each learning from the other, you may very well find something of real mathematical interest to you as well as something helpful to them. Remember, they probably know the cut-and-dried mathematics; it's the dreamy, kooky ideas I'm talking about. Next, to the graduate students—if you have the time, or even if you haven't, try to take a graduate course in some field outside of mathematics in which you have some interest and knowledge. I do not mean an introductory survey course—I mean an advanced graduate course where you really find out what the new thinking is in the subject. You may find yourself over your head sometimes, but it will open your eyes to new possibilities; and as you talk to other graduate students, you may give them an idea of what modern mathematics is all about. The undergraduates—well, arrange some joint meetings of your mathematics clubs with clubs in other fields. And, again, talk to your friends. You note there is one group I have left out, the one to which I belong, the senior faculty. We know something of what is going on elsewhere, and some of us, as my examples have shown, have been able to bridge the gap between mathematics and applications. But our methods of thought are set, which makes it much harder for us to see new connections and new relations. I believe that young people, starting in the ways I have indicated, can make real progress in established areas by solving new problems, and in new fields by defining mathematical problems and beginning to solve them.

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THEORETICAL AND APPLIED COMPUTER SCIENCE: ANTAGONISM OR SYMBIOSIS?

KEITH HARROW

1. Introduction. Many computer science students complain bitterly about being forced to take a course in theoretical computer science (typically, either formal languages and automata theory or computability theory). The complaints center about two main points: the course is not relevant to computer science (i.e., programming) and the course is too mathematical (i.e., abstract).

Of course these comments can be answered directly that computer science is not just programming and needs a mathematical foundation. (See, for example, R. Schneider's paper [**23**].) However, we will discuss a different answer, one that will be both more surprising and more interesting to the student: the direct interaction between theoretical and applied computer science. By this we mean specific topics, ideas, proof techniques, etc., that are used in theoretical computer science but which the student has already seen or which the student will soon see in applied areas. In other words, when introducing a new topic in a theoretical course, one can say, "This is also useful in your --- course," or, "This is just like--- that you learned last term." Most students will be more impressed with (and will learn faster from) a series of immediate reinforcements of this type than with vague promises of eventual future use.

Of course, it is unreasonable to expect the majority of students to become theoreticians or even to enjoy these courses as much as those in which they actually create programs. At the very least though, much of the mystery surrounding theoretical computer science can be eliminated. In particular, by stressing the two-way relationships between theoretical and practical areas, we can show our students that theoretical computer science often provides useful tools for other studies. We can also show them a more surprising result—that topics discussed in applied courses are used in theoretical areas as well.

To a large degree, we will ignore the well-known relationship between automata and formal language theory and compiler theory. See the new book by P. Lewis, D. Rosenkrantz, and R.

Keith Harrow received his Ph.D. at the Courant Institute, New York University, in 1973 under Martin Davis. Since that time, he has been an Assistant Professor at Brooklyn College in the Computer and Information Science Department. His main research interest is in theoretical computer science, especially the theory of computation and the analysis of algorithms.—*Editors*.