COMMUNICATION

Joseph B. Keller (1923–2016)

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Editor's Note: Alice S. Whittemore, George Papanicolaou, Donald S. Cohen, L. Mahadevan, and Bernard J. Matkowsky have kindly contributed to this memorial article.

Alice S. Whittemore

Joe Keller (right) with his younger brother Herbert, who also became a mathematician, in their childhood home of Paterson, NJ, 1930.

Joseph Bishop Keller was an applied mathematician of world renown, whose research interests spanned a wide range of topics, including wave propagation, semi-classical mechanics, geophysical fluid dynamics, operations research, finance, biomechanics, epidemiology, biostatistics, and the mathematics of sports. His work combined a love of physics, mathematics, and natural phenomena with an irrepressible curiosity to pursue explanations of practical and often playful enigmas.

Keller was born in Paterson, New Jersey in 1923. His father emigrated from Russia in flight from anti-Semitic pogroms and sold liquor wholesale during Prohibition, later opening a bar. His mother, who emigrated from England where her family had fled Russia, did the bookkeeping for the bar. Joe's father challenged his two sons (Joe and Herbert) with math puzzles at dinner. Both boys became mathematicians.

Joe received his bachelor's degree from New York University in 1943. He was an instructor in physics at Princeton during 1943–1944 and then became a research assistant in the Columbia University Division of War Research during 1944–1945. After receiving his PhD from NYU in 1948, he joined the faculty there and participated in the building of the Courant Institute of Mathematical Sciences. In 1979, he joined Stanford University where he served as an active member of the departments of mathematics and mechanical engineering.

In 1974, when Joe was based at the Courant Institute, he was asked by Donald Thomsen, the founder of the SIAM Institute for Mathematics and Society (SIMS), to oversee a SIMS transplant fellowship at NYU Medical Center. The mission of SIMS was to help research mathematicians apply their training to societal problems by temporarily transplanting them from their theoretical academic environments to settings engaged with applied problems. Joe agreed to Thomsen's request and promptly arranged an initial meeting with the prospective transplant, me. My research goals were to explore biomedical problems involving epidemiology and biostatistics, rather than the problems in group theory with which I had been struggling. During this two-year fellowship, Joe provided characteristically supportive and inspiring mentoring at weekly blackboard sessions.

Keller on his 1943 graduation from New York University.

We were intrigued by the unknown biological mechanisms underlying the formation of cancers and the role of environmental exposures (like cigarette smoke) in causing them. Several investigators had proposed quantitative theories of carcinogenesis in attempts to explain the temporal behavior of cancer occurrence in humans and laboratory rodents exposed to carcinogens. The theories involved the transformation of normal cells to malignancy and the subsequent proliferation of malignant cells to form a detectable tumor.

A major puzzle was why the incidence of many cancers increases with the fifth or sixth power of an individual's age. For example, if I'm twice as old as you, my cancer risks are 32 or 64 times yours. To account for this puzzle, investigators proposed that a normal cell

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becomes malignant after suffering multiple sequential mutations. Once transformed, it proliferates more rapidly than normal cells until its progeny form a detectable tumor. Exposures to carcinogens increase cancer incidence by increasing the common rate at which the mutations occur. This theory explains the steep rise in incidence with age, but it conflicts with the linear or quadratic dependence of incidence on dose of carcinogens. To avoid this conflict, Armitage and Doll instead assumed that the sequential mutations occur at different rates, only some of which are affected by a given carcinogen. Although this multistage theory accounts for some of the cancer data, it has some biological defects. The main defect is the lack of any direct experimental evidence that cancer occurs in more than two stages. This led Armitage and Doll to modify the theory so that only two stages were needed, but cells in the intermediate stage could multiply more rapidly than normal cells, providing an increasing supply of partially transformed cells awaiting a final transition to malignancy.

[Problems] must be neither too difficult nor too mathematically trivial.

These and other theories had been presented in a wide variety of medical journals with varying degrees of mathematical rigor, and their predictions were compared to observed patterns of cancer incidence in epidemiological or experimental data. Joe quickly saw the utility of a review and synthesis of the many different theories, using a common framework of stochastic equations to describe the rates of cell transformation and tumor growth in our joint 1978 SIAM Review paper on "Quantitative Theories of Carcinogenesis."

The modified two-stage theory and its subsequent extensions form the basis for much of our current understanding of how genetic and non-genetic factors cause human cancers. They explain certain enigmas regarding the roles of cigarette smoking in lung cancer, of mammographic density in breast cancer, and of genetics in colorectal cancer and retinoblastoma (a malignancy of the eye). For example, they explain why lung cancer incidence rates are proportional to the fourth power of smoking duration but only to the square of smoking rate (packs per day) and why the lung cancer incidence rates for ex-smokers never drop to the rates of lifelong nonsmokers.

Joe's catholic curiosity about all natural phenomena included a broad range of biological, biomechanical, and bio-mathematical enigmas. In addition to his work on lung cancer in smokers and skin tumors in mice, he wrote about the mechanisms underlying breathing attacks in asthmatics, vision in kittens, running in athletes, crawling in worms, genetics in families, hearing in humans, and leukemia in children. This work, together with his major contributions in mathematical physics and applied mechanics, has earned him some of the world's highest scientific honors, including the National Medal of Science and foreign membership in the Royal Society of London.

When asked how he selected problems to work on, Joe replied that he needed to understand the phenomenon underlying the problem and he needed to see that it had a mathematical aspect. Moreover he needed to see that its

Mathematician and future wife Alice Whittemore with Joe Keller in NJ, 1975.

Whittemore and Keller on a long-distance hiking trail (Grande Randonnée) in France, circa 1995.

solution could be enlightening and significant. And, like Goldilock's chair, it must be neither too difficult nor too mathematically trivial. Joe loved working with students and colleagues, many of whom approached him for help in solving problems they had encountered in their own work, and these associations led to fruitful collaborations.

One evening soon after the start of my transplant fellowship at NYU, Joe and I were both hungry at the conclusion of our weekly blackboard session, so we decided to grab a bite at a nearby Chinese restaurant. My mathematical education continued at dinner, but now the subject was inverse problems. Joe explained that in a typical mathematical problem, you are given a question for which you must provide an answer. In an inverse problem, however, you are given the answer and your job is to provide a corresponding question. For example, you might be asked to provide a question to which the answer is "1 and -1," and your question might be, "What are the roots of the equation $x^2 - 1 = 0$?" To further clarify the concept, he then gave me several other answers in need of corresponding questions. When he asked me to provide a question whose answer was "Dr. Livingston I presume," I suggested a question based on Stanley's search for Livingston near the Nile. He promptly informed me that, while that question was acceptable, the optimal question was "And what is your full name, Dr. Presume?"

That evening, neither of us knew that we were starting a life of work, fun, and happiness together that would last until his death 42 years later.

George Papanicolaou

I first met Joe Keller in September 1965 when I arrived at the Courant Institute as a graduate student interested in applied mathematics. Joe was giving a one-semester course in methods of theoretical physics, and I took it, along with some other first-year graduate courses in mathematics. What was different about Joe's teaching was that it presented mathematics as empowering, even in this basic class, and not as an edifice to be maintained and enriched for its own sake. He had an outward oriented view of mathematics and an infectious confidence of what could be done with a deeper understanding of formulations and methodology, which fit well the problems under consideration, often coming from outside mathematics. His viewpoint was somewhere between mathematics, physics, and engineering, because he cared about methods, their analysis and scope, but he also cared about the emerging results and their interpretation, and the potential impact they could have.

In the second half of the 1960s Joe had already branched into many different research areas, quite distinct from diffraction theory, which was where the bulk of his research was in the 50s and early 60s, culminating in his geometrical theory of diffraction. This theory was a brilliant synthesis of high-frequency asymptotic analysis, the careful use of the very few exact solutions of diffraction problems that were available, and a consistent geometric interpretation of the wave components that contributed to the overall wave field. The insight that his geometrical theory of diffraction provided came from its elegant conceptual simplicity. From the location and geometry of corners, edges, and other features of the scattering environment, one could in principle write down the high-frequency form of the field anywhere, but this would require numerical computations except in relatively simple cases. Scattering phenomena in the high-frequency regime even today are difficult to analyze with direct numerical computations because the region of interest could extend over thousands or millions of wavelengths in a three-dimensional setting.

There was something special about Joe Keller and seminars.

I learned diffraction theory and uniform asymptotic methods by going to the Friday afternoon applied mathematics seminar at Courant. This was a more general theory that could, in diffraction for example, express the field across a shadow boundary in a way that was almost exact near it. It was a lot more elaborate and less geometrical than Joe's original geometrical theory of diffraction. But it was also much closer to a complete mathematical theory, something that the theory of Fourier integral operators and microlocal analysis began to develop in the 1970s and later. Joe was deeply interested in this, and some of his results on uniform asymptotic methods are still the best available today, but he was already moving into nonlinear waves, random media, and many other areas. He was much more interested in new horizons where asymptotic methods could be used in a transformative way.

There was something special about Joe Keller and seminars. He was a very good listener and very quickly got to the essential point of what was being presented ahead of everyone else in the room, including often the speaker. In anything that had to do with waves, asymptotics, and related areas his comments and questions during the seminars made a huge difference to me at the time, and no doubt to others, because I saw where the fault lines were in the methodology and the theory. It is hard to get that by reading papers or listening to a polished presentation at a seminar, except when Joe Keller is in the room. After Joe left Courant in the 1970s to go to Stanford, we at Courant worked hard to keep the Friday seminar going in the tradition that Joe had started, and I think we did well in several areas. When I joined him at Stanford in the early 1990s, the first thing I realized was that Joe's reputation as a special participant at seminars was firmly established. And that was not just at the Friday applied math seminar that he had transplanted at Stanford, but also in materials science, in applied physics, and especially in the fluid dynamics seminar. His active participation in the geophysical fluid dynamics program every summer at Woods Hole is

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Joe, here at a Stanford seminar in 2003, was a very good listener and very quickly got to the essential point of what was being presented ahead of everyone else in the room.

fondly remembered by generations of graduate students for his deeply knowledgeable comments that kept things sharply focused. At the fluids seminar at Stanford, Joe's comments and questions were expected, especially when the speaker was a bit obscure or too fast. And, of course, Joe would have no patience with pretentious speakers as his not always diplomatic comments indicated, to the delight of the regulars at the seminar.

I worked with Joe on waves in random media, which is a field that was very much influenced by his thinking. Wave propagation in inhomogeneous media had received attention in the early part of the twentieth century, and even earlier by Maxwell and others, but it really became important after the Second World War, because of sonar and to a lesser extent radar, as well as seismic exploration. It had already been rather well developed to address the passage of light through the atmosphere, motivated by astronomy and astrophysics. This was done with radiative transport theory, which was phenomenological and unrelated to Maxwell's theory. Joe formulated clearly the mathematical aspects of waves in random media, including the identification of regimes for different types of phenomena depending on the several length scales and other parameters, somewhat like the dimensionless formulation of fluid dynamics. Throughout the 60s and early 1970s he lectured often on this topic and his seminars were very well received.

He moved on to many other research areas: nonlinear waves, various fluid dynamics problems including lubrication theory, the effective properties of materials with and without variational principles, homogenization theory in materials, effective boundary conditions for numerical computations, mathematical biology, and even American options in financial mathematics. His contributions have had an enormous and lasting impact in applied mathematics.

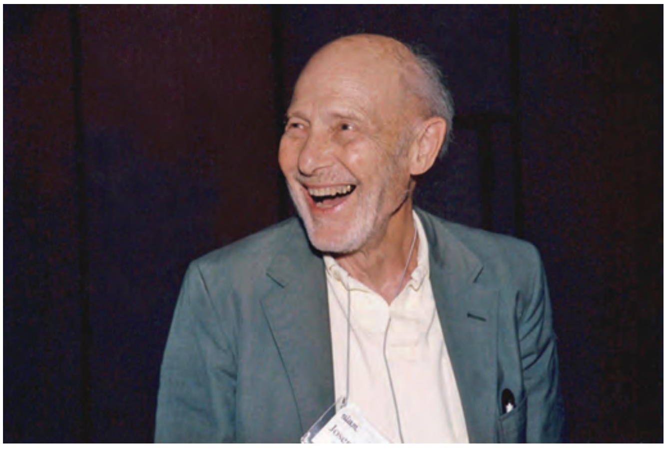
Donald S. Cohen

I was a graduate student at New York University (NYU) (1959–1962) and then a post-doc part time between NYU and Columbia University (1963–1965). The NYU group was not known as the Courant Institute at that time, and they were housed in two old buildings near Washington Square in New York City.

Richard Courant had succeeded in bringing a few refugees from Nazi Germany to New York to attempt to continue the great Göttingen tradition, and while I was there Courant, K. O. Friedrichs, Fritz John, Wilhelm Magnus, and J. J. Stoker were senior people still lecturing and teaching courses. Young people, slightly older than I, including Cathleen Morawetz, Jürgen Moser, Peter Lax, Louis Nirenberg, Paul Garabedian, Harold Grad, and Joe Keller, were then creating the reputations for which they later received many prestigious awards. I took courses from several of them and listened to many lectures from the others.

Twentieth-century physics produced important and difficult systems of differential and integral equations, and much of mathematics at Göttingen was devoted to attempts to understand their solutions. Theory and techniques from many parts of analysis were studied in these attempts. That same philosophy dominated NYU. Functional analysis, algebra, topology, geometry, and approximate methods were also considered with that goal in mind. It was a fantastic place to be.

One of the younger stars-to-be was Joe Keller. He insisted on being called Joe by everyone. Reputation and respect were derived from his command of mathematics and the way he ran his group. He was playful, could be chided and equally chided back, but it was clear that he was in command. His domain was the seventh floor of a small old building at 25 Waverly Place. His door in the middle of a short hallway was always open, and so were those of the surrounding grad students and post-docs. This was prior



Joe was playful, could be chided and equally chided back.

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to the existence of the desktop computer and the internet, so people were physically present, and the excitement for mathematics was everywhere. Joe was easily approachable, and he was often in the various offices where he supervised research in a wide variety of areas.

An important part of Joe's existence at that time was the weekly basketball seminar involving both those with very little ability as well as some with very serious ability. As in all his endeavors Joe was very competitive. He himself was only moderately skilled, but he was extraordinarily aggressive, and he didn't mind getting physically abused, as long as it was clean, legal basketball. The real purpose of the seminar was to have a good time and then go to Wah Kee Chinese Restaurant in New York City's Chinatown. Joe always collected the check, told each of us how much to pay, and then paid the total, remarking that his service charge was to supplement his salary. I have no doubt that he personally paid a good portion of the meals for those of us who were grad students.

Joe was partially bald and, for a long while, had a full bushy beard. One morning he appeared clean shaven and said that a total stranger told him that he had his head on upside down, thus the shave. (It was well known that one of Courant's admonitions was to tell a story as it should be told rather than how it actually happened.)

I never took a course that he taught nor read more than a few parts of some of his papers. Nevertheless, Joe did more to shape my early outlook and development than anyone else. He often came into my office for a few hours late in the afternoon, wondering about some problem then on his mind. Almost everything he saw suggested a mathematical problem to him. The question was how to formulate tractable problems from which answers could be extracted to give reasonable explanations of the phenomena. I was a physics major as an undergraduate and had learned a great deal of classical physics at both Brown and Cornell before I found my intellectual home at NYU. Seeking dimensionless groupings and looking at equations that replaced the basic general electromagnetic equations or those from fluid or solid mechanics was just done by a physicist, and ad hoc "laws" (optimally conservation laws) were accepted as answers. I did this when I could (not very often as a grad student), and it clearly was unacceptable to Joe, who wanted to know where these things came from by some rational process applied to rigorously derived theory. Joe's goal was to make the nature of approximations rationally follow from his manipulations and to be able to give explanations with the meaning of the approximations clear. Moreover, he wanted to answer deeper questions when no theory of any kind existed. In those days much of his work was to eventually provide a beautiful theory of high frequency diffraction of both penetrable and impenetrable bodies and through homogeneous and inhomogeneous media. He worked at the blackboard until nothing more seemed promising. Watching all this, done by an exceptionally gifted researcher, was a wonderful education.

Joe wanted to know everything that was being done by everyone. In addition to my selection of graduate courses, one day he suggested that I go to a series of lectures by Friedrichs on spectral theory of operators in Hilbert space. He and I both attended; the attendees consisted of almost all the faculty and a few grad student and post docs. We also went to lectures by Nirenberg on L^p estimates for solutions and their derivatives of elliptic partial differential equations satisfying general boundary conditions, and lectures by Moser (who was visiting) on what would later become part of KAM theory and the difficulty of small divisors for quasiperiodic orbits in dynamical systems. Joe studied and knew more pure mathematics than is generally known about him (he taught the graduate course on topology the year before I arrived); he was able to rapidly assimilate the pertinent ideas and how they would be useful to him for some of the many problems he had in mind.

One Monday morning after he asked several of us gathered in an office what we had done during the weekend, he was asked what he had done. He replied simply, "Oh, I got married." That was his somewhat playful announcement of his marriage to Evelyn Fox, a post-doc who was never a part of the physics-oriented research done by the rest of us and whom none of us knew that Joe was courting.

After that, Joe, for obvious reasons, spent less time in his office. There was a new building and a new name was given to the department. The grad students called the new building the Courant Hilton to contrast its elegance with the dilapidated conditions of the two old buildings. The senior professors occupied large corner offices, and their people were scattered throughout the building. I thought that the exciting atmosphere of 25 Waverly Place disappeared, and Joe often told me that it was significantly different for him.

After that, I saw Joe many times throughout the years at Caltech, where I've been for over fifty years and Joe's brother Herb is a professor, and at Los Alamos where I consulted. We always talked in depth about math and physics, and his curiosity and intensity never diminished. Everything he looked at seemed to suggest something needing investigation, and when he presented his results, as he often did in talks, the extraordinary depth and originality of his investigations became apparent.

My persistent memory of him at all ages is of the young Joe Keller, mentally and physically very active, often playful, telling terrible jokes involving what he called inverse problems, and deeply interested in learning new things and solving extremely difficult problems with a true teacher's desire to lecture on the results, thereby continually educating succeeding generations of interested people. Part of this group of those fortunate to have interacted with him constitutes what some have called the Keller School.

L. Mahadevan

Applied mathematics in the middle of the twentieth century was the intellectual continuation of nineteenth-century natural philosophy, which included mechanics, thermodynamics, optics, hydrodynamics, and electromagnetism. After the Second World War, and particularly with the dawn of the Space Age, the subject blossomed to include the creation of new mathematical tools to solve analytically intractable problems approximately and the application of mathematical ideas creatively to engineering, physics, biology, and beyond.

While it is difficult to imagine any individual excelling in both of these domains, Joseph Keller, perhaps the pre-eminent applied mathematician of this era, did just this. He was recognized for his foundational mathematical contributions in the domains of asymptotic analysis, perturbation methods, and hybrid numerical-analytical methods, and their deployment over a very wide range of areas, including wave propagation, quantum, statistical, and continuum mechanics, and transport phenomena in both deterministic and stochastic settings.

Following his PhD in 1948 at New York University, Keller thrived in the mid-century intellectual ferment at the Institute for Mathematical Sciences, which his mentor Richard Courant had set up. Over nearly three decades, his work expanded from initial studies in wave propagation to include the entire gamut of natural philosophy, quantum and statistical mechanics, and applications to engineering. He spent the last three and a half decades of his life at Stanford, where he expanded his interests further into engineering and biology, with occasional forays into medicine, sports, and finance. His celebrated studies in these fields led to many honors and have been written about in depth by others here and elsewhere.

[He could formulate a question] when others did not even realize that there was something to be asked.

In addition to the specific problems that he illuminated and the techniques that he created, there are a number of scientific and mathematical themes that appear repeatedly in his work: his exquisite taste in questions and problems, his use of analogies to illuminate problems in one area with ideas from another, and a deep physical intuition for and mathematical economy in creating and using techniques to solve problems. Surely others will see more and different threads in the rich tapestry that he wove, but the following vignettes of his approach to applied mathematics-as-a-science might serve to open a window into how he thought.

An enduring hallmark of his style was an ability to formulate a tractable mathematical question in any subject, often when others did not even realize that there was something to be asked. This led to a wide range of papers in which he explored such problems as the conditions, often for a fair coin toss (only possible for a coin spinning about a diameter, sometimes referred to as the Keller flip, and asymptotically correct in the limit of large angular and vertical velocities, *Amer. Math. Monthly*, 1986), the number of shuffles needed to mix a deck of cards (seven, deduced using a simple argument that complemented earlier work by Persi Diaconis), the mechanics of impacting rigid bodies in the presence of friction (that can lead to very counter-intuitive motions such as the reverse bounce, *J. App. Mech.*, 1986), and a correction to Archimedes' principle for buoyant objects [3] (that accounted for the effects of surface tension, *Phys. Fluids*, 1998), among others. A guiding principle in many of these analyses is that there are fruits aplenty at the fertile boundary between two scientific fields and sometimes quite literally between two media. Keller had a keen eye for how to spot and pick these fruits!

The all-too-human need to optimize served as a wellspring of problems that he dipped into repeatedly. Likely inspired by his work on inverse scattering problems in electromagnetism, Keller studied many optimal design, control, and strategy problems in engineering, mechanics, and physiology. For example, he provided the solution to a problem first posed by Lagrange—the shape of the strongest column [1], given its volume and length—by posing it in terms of an eigenvalue problem for a particular Sturm-Liouville operator and using isoperimetric inequalities to derive the result that a triangular cross-section is best. Later, others refined and generalized this, with continuing implications for structural optimization. Keller also calculated optimal strategies for running a race (for short distances, an anaerobic strategy is best, but once the distance is larger than about 300m, one must switch to an aerobic strategy by accelerating to the maximum speed as quickly as possible and then staying at that speed, coasting past the finish line with no energy left, *Phys. Today* 1973), or for maximizing longevity (some, but not too much caloric restriction and exercise is good, and Keller practiced as he wrote). He also worked out strategies for ranking baseball teams (in an early precursor to Google's page-rank algorithm, for a Christmas lecture in the 1970s at New York University, he showed how the Perron-Frobenius theorem could be used to guarantee the existence of a ranking vector based on the relative strengths of the teams involved), and for best inspection practices in a factory (formulated and solved as a variational problem). In each case, he was able to get to the mathematical essence of a problem that not only illuminated its specific origin, but also its broader ramifications.

In addition to adroitly exploiting the no-man's-land between domains, Keller was a master of using analogies to bridge fields, aided by a combination of physical intuition and mathematical expertise. In one of his most cited works in the area of semi-classical mechanics that straddles the quantum and the classical [2], Keller resolved a puzzle linking different frameworks of quantum mechanics. Using an analogy between the high-frequency limit of the reduced wave equation and the Schrödinger equation, he showed how to solve problems in one domain using knowledge from the other, elaborated on by him and Sol Rubinow (pictured), and many others later. In a different setting, recognizing the inherent linearity and analogies between the equations of electrostatics, slow fluid flow,

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Joe Keller (right) and Solomon Rubinow, pictured here in Woods Hole MA, mid-1960s.

and elastostatics, Keller realized how the presence of a small parameter (due to geometry, as in narrow slits; due to large contrast in properties, as in dielectric mixtures, suspensions, etc.) makes the problem of determining the effective properties amenable to analysis using the theory of harmonic functions. This allowed him to deduce a theorem for the effective conductivity of composites [3], and variations on this idea over the following decades have been the source of much elaboration in applied mathematics in the context of homogenization [redacted] theory—a subject that deals with the statistically averaged properties of materials, with implications for engineering.

The virtue of concision seems to have been another (perhaps unspoken) theme. Indeed, a number of his papers were no more than a page or two, had few references, and yet packed an impact. In one such exemplar (*Amer. J. Phys.*, 1959), he showed that the large amplitude motion of a string that has been stretched to many times its rest length can be described by a linear wave equation, and later generalized it to finite deformed continua. Interestingly, this has a macroscopic realization in a toy—a helical spring called a Slinky®, and a microscopic realization in highly stretched polymers. In another short paper, half a page long, he discussed how to reconcile the transition from one power law to another in turbulent boundary layer flow using a soluble differential equation (*Phys. Fluids*, 2002). And in a four-paragraph paper in *Theoretical Population Biology* (2004), he tackled the link between mortality rate and age, showing how a simple model can explain its initial increase followed by saturation in old age!

Keller was happiest when discussing a new problem or solution and wore his fame lightly. Although he received many prestigious awards, he got a particular pleasure from two Ig Nobel Prizes for "research that makes you laugh, and then makes you think." Very likely, after him, the prestige of the Ig Nobel went up! The first was for explaining the teapot effect (shared with J. M. Vanden-Broeck) and the second was for explaining the dynamics of ponytails (shared with R. Ball, R. Goldstein, and P. Warren, who calculated their shape). He came to the ceremony, wizard-like, wearing a pony-tailed fez to explain his idea and enjoyed the riotous ceremony, paper planes and all, as the author can attest to. And what exactly did he do and why should one care?

Anyone who has poured tea from a kettle knows to be wary of the dribble along the spout that can ruin the rest of the afternoon. Most onlookers asked to explain this effect will mumble something about surface tension. Inspired by experiments of the rheologist Marcus Reiner (who poured colored tea underwater, where interfacial forces are unimportant but the effect persists), Keller wrote a note (Teapot effect, *J. Appl Phys.*) in the 1950s about how inertial effects (and Bernoulli's principle) can explain this phenomenon, and later worked out a more complete theory. Some sixty years later, likely inspired by the swaying ponytails of runners in front of him during his hikes, he asked why a ponytail swings from side to side while the head bobs up and down? The key is an instability of a flexible string forced periodically and vertically at its boundary. Keller showed ("Ponytail motion," *SIAM J. Appl. Math.*) that under some fairly general assumptions, it is possible to derive a Hill equation for this phenomenon, which arises generically in the theory of parametrically driven oscillators, in celestial mechanics and a variety of other situations (and is the theoretical basis for the Nobel Prize winning ion-trap of Paul and Dehmelt). This insight allowed Keller to deduce the conditions for instability and show that for a ponytail bobbing at a frequency of a few hertz, the most unstable length is about 25 cm. Test it yourself if your hair is long enough!

His eclectic interests in science along with a warm and friendly demeanor made him easily approachable and an inspiration to all. He was particularly encouraging of young mathematicians and scientists, and mentored many both formally and informally. At the Woods Hole Oceanographic Institution, where he was on the summer faculty at the Geophysical Fluid Dynamics Program for more than fifty years, it was commonplace to see him on the porch every afternoon, with students and colleagues who sought him out as a universal consultant. His scientific legacy—an unquenchable curiosity about nature, and a humility embodied in the belief that every problem is worth thinking about, and learning from—will last.

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Bernard J. Matkowsky

When I attended college in the 1950s, very few programs in Applied Mathematics existed, and those were largely unknown. While I liked problems in engineering and science, I wasn't satisfied with the methods employed to analyze them. I preferred the approach of mathematicians, though

I still wanted to be involved with problems in science and engineering. It wasn't until I was a graduate student in electrical engineering in 1960 that some professors suggested that I might find what I was looking for at the Institute of Mathematical Sciences attached to NYU. One professor went even further, suggesting that I work only with Joe Keller. Fortunately for me, I followed this advice and am grateful to this day for having done so. Joe has had a profound influence on, and has served as an inspiration to, me as well as to generations of students at NYU and at Stanford who are part of the so-called Keller School of Applied Mathematics.

As a student I read as many of Keller's papers as I could get hold of. Of course I read his papers on the geometrical theory of diffraction. I read his papers on the asymptotic solution of eigenvalues, which was related to my thesis work. I read his papers on boundary layer problems, as well as his work on perturbation of nonlinear boundary value problems and bifurcation theory and a host of others. I learned from them all. Though unrelated to my thesis, one perhaps lesser-known paper nevertheless made a particularly strong impression on me.

In science and engineering a number of different theories based on different mathematical models were often proposed to explain a given phenomenon. However, it wasn't always clear which model was appropriate under what conditions. Some models were postulated in an ad-hoc manner, some were based on simplifying assumptions, while others were purported to be "approximations" to a more general model, though they were not derived in a systematic manner, nor was it always clear how the different models were related to one another. In the purported approximation of one by another, simpler, model some terms were retained, while others, though possibly of comparable size, were discarded. Needless to say, these approaches were not very satisfying, especially to a young student.

In "A Theory of Thin Jets," Keller (with his PhD student Mortimer Weitz) considered the problem of jet flow, specifically, the problem of determining the shape of the jet and the velocity distribution within it. The theory of jets is based on the equations of hydrodynamics, though only a limited number of problems were successfully treated this way. More general problems were treated with the simpler hydraulic theory, in which both the pressure and the velocity on each cross section are assumed to be constant, though these assumptions are incompatible with the equations of hydrodynamics. Thus, hydraulic theory is based on different, approximate equations. Joe writes: "Two questions which immediately arise are: What is the relationship between the two theories and How can the results of Hydraulic Theory be improved? In this paper we answer these questions by presenting a method of solution of the hydrodynamic problem as a series in powers of the jet thickness divided by some typical length of the jet (epsilon), i.e., an asymptotic expansion in epsilon. The first term of this solution is found to be the solution given by hydraulic theory, thus answering the first question. The higher order terms of the series yield corrections to the hydraulic theory, thus answering the second question."

Joe's paper was a revelation to me, not only for presenting a nice solution to the given problem, but more importantly, for presenting a systematic, rational approach to a general question that had long troubled me. I have employed this approach to analyze a variety of problems in various fields in the years since.

He had an uncanny ability to describe real world problems by simple yet realistic models.

Joseph Bishop Keller was the foremost contemporary creator of mathematical techniques to solve problems in science and engineering. He earned this reputation by his outstanding research contributions to both mathematical methodology and a wide variety of areas of application. Through his own work, as well as that of his students and other scientists with whom he interacted, he had a profound influence on the way that problems are formulated and solved mathematically. Joe combined unmatched creativity in the development of mathematical methods with very deep physical insight. He had an uncanny ability to describe real world problems by simple yet realistic models, to solve those mathematical problems by sophisticated techniques (many of which he himself created), and then to explain the results and their consequences in simple terms. He was a master of asymptotics and a virtuoso in showing how to adapt ideas found useful in one area to others. His work is characterized by originality, depth, breadth, and elegance, and the results he obtained have sustained importance. We briefly describe certain highlights.

One of Joe's most outstanding contributions is the Geometrical Theory of Diffraction (GTD), which he originated for solving problems of wave propagation. He began thinking about such problems during World War II while working on sonar for the Columbia University Division of War Research. GTD is an important extension of the Geometrical Theory of Optics (GTO), in which wave propagation is described by rays. The extension includes phenomena such as diffraction and the occurrence of signals where GTO predicts none. Joe developed a systematic way to treat high-frequency wave propagation and thus derived and solved the equations determining the rays, the paths along which signals propagate, as well as those governing how signals propagate along the rays. He predicts what happens as the rays encounter obstacles or inhomogeneities of the medium in which they travel. Prior to Joe's work, only a few isolated problems were solved and understood, and there was no general theory for the solution

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of more complex and technologically important problems. Now there exist books devoted to Joe's theory. Engineers and scientists employ his systematic theory to this day. Indeed, his theory is indispensable for those working on radar, antenna design, and general high-frequency systems in complicated environments. His theory has been and is still applied to a wide variety of other problems in which signals are transmitted by waves. Such problems occur in acoustics (as in sonar), elastodynamics (as in quantitative non-destructive testing), and seismic exploration for oil, to name but a few. It is commonplace in all these fields to see articles that read, "we employ Keller's method to…"

Joe also showed that his methods for wave propagation could be extended to other classes of problems, such as semi-classical mechanics. In this fundamental and penetrating work, Joe generalized work of Planck, Bohr, Sommerfeld, Wilson, Einstein, and Brioullin to derive the correct quantization rules for non-separable systems, thus yielding results valid in any coordinate system. His results, referred to as the Einstein-Brioullin-Keller (EBK) quantization rules, are currently employed by many chemical physicists and other scientists. In his work on semi-classical quantization he introduced an important measure corresponding to the number of times a closed curve passes through a caustic surface. This measure, later generalized to curves on Lagrangian manifolds by Maslov, is referred to as the Keller-Maslov index. This index was subsequently extended by Joe to eigenvalue problems in bounded domains, not necessarily associated with quantum mechanics, but governed by general systems of partial differential equations.

Joe's work has stimulated a vast literature in both the United States and abroad, not only in many areas of science and engineering where his methods and results are routinely employed, but also in the mathematics community, where his work has been taken up by pure mathematicians. For example, his work has been the impetus for a number of developments in the theory of Fourier integral operators and Lagrangian manifolds.

In addition, Joe often opened up directions of investigation by considering problem areas, that were then enthusiastically taken up by the research community. His pioneering work on the evolution of singularities of nonlinear wave equations is one such example, as is his work on bifurcation theory and nonlinear eigenvalue problems, to which scant attention was paid until the notes of his seminar appeared, and which is now one of the hottest topics of investigation by both pure and applied mathematicians.

Joe also considered problems of wave propagation through heterogeneous, turbulent, or random media, involving the transmission of signals through media such as the atmosphere and oceans, in which fluctuations occur due to the properties of the medium. He originated two methods that are very widely used. The first is the smoothing method, for problems involving small amplitude variations. The second is a multiple scale method, for problems corresponding to rapidly varying coefficients. The second method is capable of dealing with fluctuations that are not small in size, but rather small in scale. This theory, since taken up by others, now known as the theory of homogenization, has had volumes written on it. In each case, Joe showed how to systematically replace the fluctuating coefficients by effective coefficients, which are appropriate averages of the fluctuating coefficients. He then extended the work to show how to systematically derive effective equations for all sorts of problems, not necessarily associated with wave propagation. These include problems of composite media and problems of determining the large-scale macroscopic behavior of a medium that exhibits small-scale microscopic heterogeneity. His work was characterized by a simple formulation, which overcame the nonuniformities restricting earlier theories.

Joe, pictured here at the South Street Seaport, New York, 1990s, began thinking about problems in wave propagation during World War II while working on sonar for the Columbia University Division of War Research.

No stranger to national service, Joe worked on many problems related to national security, and he served on various advisory boards, national panels, and committees. After his work on sonar for the Columbia University Division of War Research, he worked on problems of underwater explosions, in order to predict the shock wave and water waves to be expected at the Bikini atomic bomb tests. At the time there was concern about producing a tsunami that might devastate Japan and other Pacific countries. His analysis showed there was no such danger. He also spent time at Argonne and Los Alamos national laboratories, studying hydrogen bomb explosions. In the early 1950s he served, with Von Neumann, on a committee on underwater atomic bombs for the Air Force Special Weapons Project (AFSWP), to consider the effects of A-bomb explosions on ships and submarines. He headed another project on A-bomb explosions for the AFSWP. During the late 1960s he was a member of JASON, a group of high-level consultants to the Defense Department and other governmental agencies on scientific and technical matters. He served as consultant to AFSWP on other projects, to the US Naval Air Development Center, to the US Army Chemical Corps, and to Argonne, Brookhaven, and Los Alamos national laboratories.

Bernard J. Matkowsky was a 1966 PhD graduate of the Courant Institute of Mathematical Sciences with Joe Keller. This photo dates from 1986, when Keller was awarded the Nemmers Prize from Northwestern University.

For more on Joseph Keller, see the interview with Keller in the August 2004 issue of Notices (`www.ams.org/notices/200407/fea-keller.pdf`) *and his Google scholar profile* (`https://scholar.google.com/citations?user=Pbn6aU8AAAAJ&hl=en`)

Joe was a teacher and expositor *par excellence*. He twice received the MAA's Lester Ford Award for outstanding expository writing. He received awards from all three major US mathematical societies, from various engineering societies, and from national scientific societies in the United States and abroad. The approximately 60 PhD students and numerous post-doctoral associates whom he has trained, now successful applied mathematicians in their own right, further attest to Joe's impact.

Finally, there is Joe Keller the man. Countless numbers of mathematicians, engineers, and scientists have come to him through the years to benefit from his acumen and understanding. To each he listened patiently, contributed helpful insights, and offered words of advice and encouragement. For us he was simply "Joe," teacher, colleague, and friend. The world has lost a giant. He will be sorely missed; his legacy endures.

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