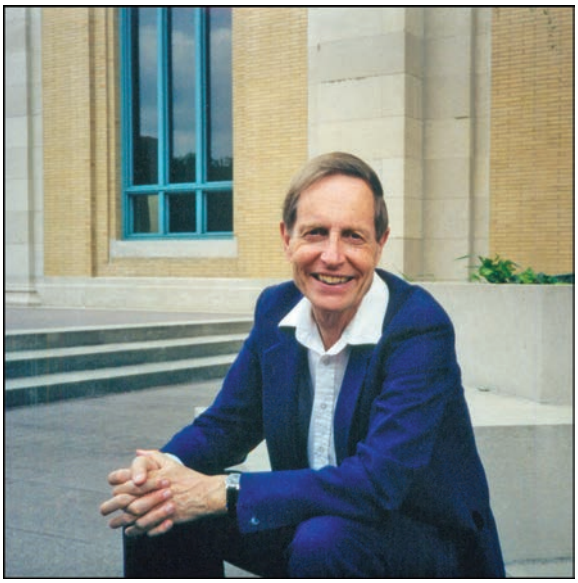
Paul Roesel Garabedian (1927–2010)

Jerry L. Kazdan



Paul Garabedian, 2000.

Background

Mathematics was in Paul's genes: both his father and his two uncles were mathematicians. Paul and his sister, Caroline Coulon, were home schooled. At age sixteen he was not admitted to Harvard because Paul's father's Ph.D. advisor, G. D. Birkhoff, was concerned that he might be immature. Instead, Paul went to Brown—and excelled. He simply was a prodigy. In the enlightening oral history conducted by Philip Davis in 2004 [2], Paul remarked (with insight), "I was a child prodigy. I am still a child prodigy, but there are very few people who know that, perhaps yourself and a few others." Philip Davis laughed.

After graduating from Brown in 1946 be began graduate school at Harvard. Two years later at the age of twenty he received his Ph.D., working with Lars Ahlfors. Paul also benefited from many conversations with Max Schiffer, who was visiting Harvard's applied mathematics department. This was the beginning of Paul's important mathematical interaction with Schiffer—and a lifetime of work where complex analysis played a major role either for itself or as a key ingredient in applications.

In 1948, after receiving his Ph.D., Paul was a National Research Council Fellow at Stanford. The following year he became an assistant professor at the University of California, Berkeley. One year later he simply resigned from the university before it became mandatory to sign the loyalty oath required of University of California faculty.

Stanford immediately offered Paul a position. He flourished there: promoted to associate professor in 1952 and to professor in 1956. In 1959 Paul came to NYU's Courant Institute. He was appointed the director of the Division of Computational Fluid Dynamics at the Courant Institute in 1978. Paul remained a member of the NYU faculty for fifty-one years. Paul also often spoke fondly of his ten years at Stanford, maintained many friends there, and returned often to visit and give lectures.

The obituary published by the Courant Institute [1] is a good source for more information.

Research

One early influential result with Schiffer (1950) was where they used Hilbert space ideas with the Bergman kernel function to prove existence theorems for elliptic partial differential equations.

Another problem he considered deeply was the *Bieberbach Conjecture* for the family of holomorphic functions $f(z)$ that are 1-1 maps of the unit disk to the complex plane normalized by $f(0) = 0$ and $f'(0) = 1$. Then $f(z) = z + a_2z^2 + a_3z^3 + \cdots$. In 1916 Bieberbach proved that $|a_2| \le 2$ with equality essentially only for the Koebe function

$$f(z) := \frac{z}{(1-z)^2} = z + 2z^2 + 3z^3 + \cdots .$$

Bieberbach conjectured that $|a_n| \le n$, with equality only for this function.

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In 1985, after more than sixty years, the conjecture was proved true by de Branges. But the road was not straight. In 1923 Löwner proved that $|a_3| \leq 3$, but it took until 1955 for Garabedian and Schiffer to prove that $|a_4| \leq 4$. The proof by Garabedian and Schiffer that $|a_4| \leq 4$ involved tedious numerical calculations. Although Paul's desk was always notoriously empty except for one small pad, this work was a real exception. These calculations on the Bieberbach Conjecture for a_4 were not done in his head.

Subsequently Charzynski and Schiffer (1960) found a much simpler, more conceptual, proof that $|a_4| \leq 4$, one that was also now *shorter* than Löwner's proof that $|a_3| \leq 3$ but did not imply that $|a_3| \leq 3$. Since one anticipates that the difficulty of the proof should increase with n, this violation led Paul to question the conjecture itself. Some related conjectures had already been found to be naive.

As a thesis topic for me as a graduate student at the Courant Institute, Paul suggested that I try to find a counterexample. So I tried. With Eva Swenson's superb help on the computer, I sought—but did not find. This lack of monotonicity in the difficulty of proof persisted. Peterson and Ozawa (1968, 1969, respectively) proved that $|a_6| \leq 6$ before Pederson and Schiffer (1972) proved that $|a_5| \leq 5$. The eventual proof by de Branges uses Löwner's approach. Crooked paths make life more interesting. I personally value this experience of struggling with a conjecture whose resolution is uncertain. It taught me to be dubious about conjectures—and diffuse evidence—until they are really proved.

In the early 1950s, Paul, in joint work with Donald Spencer on the $\bar{\partial}$-Neumann problem, was attempting to understand some problems involving functions of several complex variables. This work was seminal. See Denny Hill's note below for his personal insight on the collaboration.

In 1952 Paul's article with Lewy and Schiffer on Riabouchinsky flow was one of his earliest works on fluid flow.

Beginning in the mid 1950s, Paul's research focus was decisively influenced by specific questions on transonic fluid flow that were asked by David Young at Ramo-Wooldridge Corporation. Paul's contributions involved analytic continuation to the complex plane and were viewed with suspicion—until people calmed down and realized that even though they were mystified, the results seemed to be both correct and better than anything their own research could yield. This work was influenced by earlier results of Hans Lewy. Paul's beautiful *Partial Differential Equations* text is a wonderful introduction to these seminal ideas. This work was also the beginning of his pioneering use of computers to solve basic problems in science and engineering. It is surprising that although Paul didn't write computer code himself, he was superb as a leader to produce fundamental results. The article below by Antony Jameson gives an introduction to the problems and Paul's profound contributions, particularly to the design of a transonic airfoil, now used in commercial airplanes. This is a wonderful example of very applied mathematics at its best.

In his next period, Paul focused on *magnetic fusion*, where one studies fluid flow coupled with an electromagnetic field. One goal is the important practical application of designing a nuclear "power plant" based on fusion, not fission. The valuable articles by Geoffrey McFadden [3], [4] discuss both this and Paul's work on transonic airfoils.

While most of Paul's work after 1970 was on applied mathematics, he published a few very short gems on topics in pure mathematics, such as a simple proof of a variant of the Lewy example of a linear unsolvable PDE. These always involve complex analysis and often some functional analysis.

Paul had twenty-seven Ph.D. students and many postdocs.

Honors

Fairchild Distinguished Scholar, Caltech (1975)

NASA Public Service Group Achievement Award (1976)

Boris Pregel Award, New York Academy of Sciences (1980)

Birkhoff Prize of the AMS and SIAM (1983)

Theodore von Kármán Prize, SIAM (1989)

National Academy of Sciences Award in Applied Mathematics and Numerical Analysis (1998)

American Physical Fellow (2004)

SIAM Fellow in the inaugural class (2009)

He was a member of both the National Academy of Sciences and the American Academy of Arts and Sciences.

Paul is survived by his wife, Lynnel, his daughters, Emily and Cathy, and two grandchildren, as well as his sister, Caroline Coulon. He was an active father, who played an important role in his daughters' lives. Once when Paul and I happened to meet in Japan, he described proudly several trips he took to Kyoto accompanied by his younger daughter, Cathy, and how she helped "take care" of him. For me, these stories were a model. I regret that I never had an opportunity to have a similar experience with either of my own daughters.

References

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Peter D. Lax

Paul Garabedian was an outstanding pure mathematician as well as one of the most original applied mathematicians. His strength was that he used very sophisticated pure mathematics to solve applied mathematics problems. Sometimes this had the effect that the engineering community did not comprehend his methods and therefore was reluctant to use his results (more about this later).

Paul's father was a mathematician, a Ph.D. student of G. D. Birkhoff at Harvard; his mother also had an advanced university degree. Paul and his sister were educated at home. When Paul turned sixteen, he was already excelling in mathematics; his father took him to Birkhoff as a prospective Harvard student. Birkhoff was concerned that home schooling did not prepare Paul for college life. As he put it, "Suicides give a school a bad name." He suggested that Paul go to boarding school for a year, and then he would be admitted to Harvard. His father took Paul to Brown University; there, too, the admission people were worried about the lack of social skills and suggested that Paul go to boarding school for a year. "If he goes to boarding school, he goes to Harvard," said his father, so Brown admitted him right away. His roommate was the fifteen-year-old Al Novikoff. According to Al, Paul taught him a lot of mathematics, and he taught Paul about women.

Paul was a brilliant undergraduate; he finished in 1946 and was admitted to Harvard as a graduate student. He earned his Ph.D. in 1948 under the direction of Lars Ahlfors; his dissertation was on the Szegő kernel function.

His first academic position was at Berkeley. He arrived at a time of anticommunist hysteria. The trustees of the university demanded that the faculty sign a loyalty oath. Paul was among the faculty who refused (Hans Lewy was another). He was immediately hired by Szegő at Stanford, where he spent nine fruitful years. He collaborated with his colleagues Don Spencer and Max Schiffer and made his first venture into applied mathematics. An outstanding problem in the 1950s was whether a ballistic missile would burn up upon reentering the earth's atmosphere. Since at that time ballistic missiles existed only on the drawing board, the decision had to be made by theoreticians. Paul gave a brilliant mathematical formulation of the problem which could then be solved on the relatively slow computers available at the time. His calculation showed that ballistic missiles would not burn up upon reentry.

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Paul with Peter Lax (1968).

While at Stanford, Paul married Gladys Rappaport, a graduate student in statistics. She wrote the computer code for the reentry problem; miraculously, the code worked immediately. A second, more sophisticated, program contained some bugs; Paul gave her hell.

In 1957 Paul was named scientific attaché in London; the duties of the attaché were to travel around Europe and report on new scientific developments. During his visit to Italy, Paul learned that De Giorgi had solved a Hilbert problem about the regularity of solutions of nonlinear elliptic equations (at about the same time John Nash also solved this problem by an entirely different method). The paper by De Giorgi was published in a very obscure journal; the international mathematical community learned about it from Paul's report.

In 1959 Jim Stoker, Courant's successor as director of the institute at NYU, visited Stanford. He had had his eye on Garabedian for some time; so then and there he made an offer to Paul to come to NYU. After an afternoon of negotiations, well lubricated by martinis, Paul accepted. It was a happy outcome for both parties; Paul enjoyed and was stimulated by the atmosphere of the Courant Institute, and he liked to live in New York City.

Paul's father was of Armenian descent; his mother was not, and Paul resembled his mother.

Paul with his sister, Caroline, and niece, Aline (France, 1962).

When interviewed for admission to Harvard graduate school, Paul noticed that the interviewer was making notes; left alone momentarily, he took a peek at the notes; they said, "Wrong name, right appearance."

Paul was a hero to the Armenian mathematical community. He was once invited to a mathematical congress in Yerevan, the capital of Soviet Armenia. He accepted gladly. When he stepped off the plane, the reception committee was shocked to behold the blue-eyed, blond guest; "That's not Garabedian; it is a CIA agent."

While in New York his first marriage ended in divorce. His second marriage, to Lynnel, was very happy. They had two adored daughters.

I would like to give now the broad outlines of some of Paul's research. It started in function theory and potential theory. At Stanford he began his collaboration with Max Schiffer on the Bieberbach conjecture; very likely they were introduced to this problem by their colleague Charles Loewner, who did the deepest work on this problem back in 1923. Their approach was to use the calculus of variations. The formulas involved in this research were formidable. Ultimately, the complete solution, by de Branges, used the Loewner representation of schlicht functions.

There is a natural connection of analytic functions to fluid dynamics; two-dimensional incompressible, irrotational flows are described by analytic functions. Paul's interest in fluid mechanics was much broader. He tackled many of the classical problems in fluid dynamics, such as the flow around a rising bubble, the shape of an electrified droplet, the vertical entry of a wedge into water, and other problems of flows with a free boundary.

In the 1970s aircraft companies were designing planes that could fly near the speed of sound. That meant that over part of the wing the airflow would be supersonic. Cathleen Morawetz had shown that in general such transonic flows contain shocks; shocks increase the drag of the airplane and therefore they should be avoided if possible.

One of Paul's most influential works, with David Korn, was the design of airfoils that carry shockless transonic flows. This required solving partial differential equations that are partly elliptic, partly hyperbolic. They accomplished this by introducing complex coordinates. This was so sophisticated a mathematical idea that the aerodynamic community was unable to comprehend it; therefore they ignored it. Finally a mathematically minded aerodynamicist in Canada tested the airfoil in a wind tunnel and found the flow to be indeed free of shocks.

After that, aerodynamicists pounced on the Garabedian-Korn design. One of the leading French aircraft companies invited Paul to be a consultant. Paul accepted the offer, under the condition that they fly him to France on the Concord.

In 1964 Paul published a text on the theory of partial differential equations, designed as a text for a graduate course. It treated all the usual theoretical subjects, as well as numerical methods for solving partial differential equations, including the use of complex coordinates. The book became extremely popular and still is today.

Starting about thirty years ago Paul turned his attention to the mathematical and engineering problems of nuclear fusion. The physics subject, called magneto-hydrodynamics (MHD), is about flows of high temperature plasmas that typically contain shock-like discontinuities. Paul published about sixty papers on the subject, more than a third of his total publication of 167 papers (the last one appeared in 2010). He believed in the practicality of generating energy by nuclear fusion. Until the last months of his life, very weak physically but razor sharp mentally, he continued to work on problems of MHD. If fusion energy ever becomes a reality, Paul's work will have played an important part in its success.

With much outstanding mathematics one can imagine the work having been done by a number of mathematicians. Not so with most of Paul's accomplishments; his outlook was unique. He will be remembered for a long time.

Albert B. J. Novikoff

Here goes (stream of consciousness): we were both around sixteen years old, too young to be at (Brown) University, but it was war time, and compromises

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were the order of the day. I came first, from Bronx Science, and Paul arrived a year later, fresh from home schooling in a professor's household. I was, comparatively, the sophisticate, but we both wore long underwear in the winter, scandalizing our respective roommates and condemning us to room together. Items of forbidden winter clothing remain in my mind, stigmata of being under age and of controlling mothers, specifically, mittens and galoshes. He was already tall, but like a giant boy Pinocchio, all kneecaps and shins. I had not yet started the viola, and he, miraculously, already played Chopin ballades and (on the violin) the great Bach *Chaconne*. I had no one from Bronx Science to compare him to.

Mathematically, he had a comparable background. His father still marveled at his own experience at Harvard, under the great G. D. Birkhoff, and what it was like to be in class and watch the great man thinking his way before your very eyes. His father, by the way, played me an organ symphony by Widor, whose existence had until then escaped me.

Music, mathematics, and the intellectual life were his heritage, but apparently he had been left out of "kidhood," and my great (ha!) contribution was to introduce him to the humbler parts of human destiny. He was quick to enjoy the company of the graduate students we both frequented and remained tremendously grateful to me over the decades, when I did no more than turn a key in a metaphorical door.

As his roommate (and hopeless competitor in math classes) I can report that he was a "clean desk" type from the get-go; only the prescribed texts sullied the empty plane of his desk. What he worked on was his own, in every sense. I think he took pleasure in being obstinately independent, unbowing to written authority.

By the time he was taking senior (and also graduate) courses, the graduate students all recognized that he was exceptional. I never told him, but once a group of us were remarking on this when the senior member of our circle, a future physicist, remarked wryly, "He's going to be annoyingly useful some day."

Now the public in general has (or had) little understanding or interest in what mathematicians actually do. Regarding this, Fritz John noted some years ago that the mathematician's sole recognition is "the grudging appreciation of a few friends". Clearly, Paul started collecting his quota of such recognition from an early age.

As I learned from an early visit to his parents in Wheaton, MA, where his father was not only professor of mathematics but school organist and director of the (girls) choir, a lot was pretty much set in stone.

Paul with David Gilbarg (1950s).

A few years later I was at Stanford and heard Schiffer boast of the recent discoveries of his student, Paul, at Harvard. I had not ever been party to such generous pride. I remember him looking back at Harvard and commenting that he had been "pretty good" at ideal theory and wondering just what units he was using.

Other associations are Saint-Venant's problem (whatever that was, involving René de Possel), the Bieberbach Conjecture, et al.

He mellowed enormously with a happy marriage and adoring daughters and took to making jokes at his own expense. Just after moving to his new apartment, not so long ago, he had occasion to decide which reprints he wanted to save, and said, "I seem to have written each paper five times."

With more emotion than I care to admit,
Al Novikoff

C. Denson Hill

In late August of 1961 I arrived at Idlewild [now Kennedy] Airport, having $300 in my pocket and no idea how to get to NYU, where I was supposed to become a new graduate student. So I squandered part of it by taking a taxi into Manhattan and then asked the taxi driver if he knew of a cheap hotel near Washington Square Park. After a night of squishing cockroaches, I went to look for the

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famous Courant Institute. I was dismayed to find out that it was located in an old hat factory, and I was shocked at how rude people were on the street and how much of a slum the neighborhood appeared to be. I had expected something quite different. But they had offered me a fellowship, and I had no other place to go, so I went inside to look for somebody to talk to. Almost immediately I found a kind secretary, who took me to "tea," saying I might find some faculty member there. At Rice University I had already developed an interest in PDE, applied mathematics, and had some experience with numerical analysis. It was because of those interests that I had been advised to go to Courant, but nobody had suggested whom I should see there or exactly what I should do upon arrival.

At tea, there was only one guy, who I thought was another graduate student. But the secretary introduced him to me as "Professor Garabedian." He looked much too young to be a professor. Over tea he politely asked me what my interests were, which courses I had taken, etc. Then he announced that he was writing a book on PDE and that he needed somebody to read and correct the manuscript, and he asked me if I would like to help him on his project. Of course I agreed. That is how, within twenty minutes of entering the building, I became a graduate student of Paul Garabedian.

So for my first two and a half years as a graduate student, I had the side job of going over various drafts of Garabedian's book [1]. At that time Paul had another student, Jerry Kazdan. He was a few years ahead of me, and Jerry had made detailed criticisms of the first draft of the book. My job was to go over various subsequent drafts. I wound up making extensive revisions to Chapter 10 on integral equations and Chapter 15 on free boundary problems. Needless to say, I learned an enormous amount, not only from the manuscript itself, but also from the sometimes heated discussions with Paul about my suggestions for changes. I was always pressing him to make precise definitions and state precise theorems, and Paul was always resisting. He kept telling me that he wanted the book to be read by engineers and other applied scientists and that he was trying to provide insight, not a list of theorems. His point was that if he tried to prove the sharpest versions of various theorems, it would clutter up the exposition and obscure the elegance of many arguments. Probably at that time I was too much enamored of my recent affair with Bourbaki. But luckily I had also read Goursat, so I did in fact get his point. After more than fifty years, I see clearly now that he was right.

What impressed me the most was Garabedian's amazing ability to step out into imaginary directions and use several complex variables to gain insight into various questions about PDE's. In this aspect he was very much in the spirit of J. Hadamard and H. Lewy. So I read Hadamard's book on the Cauchy problem, and a number of Lewy's papers. (This set me up for my later work with Aldo Andreotti.) But eventually when I went to Paul and asked him to suggest a thesis problem which had something to do with several complex variables (a subject I knew nothing about at the time), he said, "Well, I used to work in SCV, but I gave it up because I did not understand it." Later I found out that Garabedian was the man who invented the $\bar{\partial}$-Neumann problem.

The story, as told to me by Paul, went like this: For some period he was working with D. Spencer. It was a friendly collaboration in which each was quite polite and respectful to the other. But the problem was that he never understood anything Spencer said, and he was not sure if Spencer understood him either. So it wound up with each of them writing his own paper and putting the other guy's name on it [2], [3]. The $\bar{\partial}$-Neumann problem was formulated in the paper [2] written by Garabedian. As is well known, Spencer later pushed his student J. J. Kohn to solve the problem, which was difficult due to the noncoercive nature of the Neumann boundary condition for $\bar{\partial}$.

Another story Paul told me was how he managed to get his Ph.D. at Harvard, in only two years, by solving his thesis problem over the weekend. Ahlfors had returned to Harvard as a full professor in 1946, and Garabedian was his first Ph.D. student there. Paul walked into Ahlfors's office on a Thursday, or maybe a Friday, and asked him for a thesis problem to work on. Ahlfors promptly suggested a problem involving the Szegő kernel. But Garabedian did not understand exactly what Lars was getting at. Fortunately another mathematician, Menahem Schiffer, had also arrived at Harvard as a research lecturer in 1946, but he was over in applied mathematics. Paul had discovered that Schiffer was easy to talk to, so he went over to the applied mathematics department to ask Schiffer just what it was that Ahlfors wanted him to do. Schiffer was much more clear than Ahlfors had been and was able to explain to Paul what the problem actually was. Knowing now just what the question was, Garabedian went home, worked hard over the weekend, and went back to Ahlfors's office on Monday and presented the solution. Ahlfors said, "You have a thesis." Later Garabedian and Schiffer became colleagues and collaborators at Stanford.

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Antony Jameson

Paul Garabedian's Contributions to Transonic Airfoil and Wing Design

This note on Paul Garabedian's work on transonic airfoil and wing design is written from the perspective of aeronautical engineering as well as applied mathematics. Paul's contributions in this area had a profound and lasting impact on the way people set about designing wings in the aircraft industry. Transonic flow is of great relevance to aircraft design because it is the most efficient regime for long-range transport aircraft. Transonic flow is also of great mathematical interest. Outside the boundary layer and wake the flow is well represented by the transonic potential flow equation which is of mixed type, elliptic in the subsonic zone and hyperbolic in the supersonic zone. This equation proved quite intractable to analytical methods of solution. In order to reduce the drag one must look for shapes that minimize the shock strength or even produce shock-free flow. This was the problem that Paul chose to tackle. It had been established, however, by Cathleen Morawetz that shock-free solutions are isolated points and shocks will appear with small perturbations of the shape or the flight condition. So the problem of designing a shock-free shape is not well posed.

Paul elected to pursue an inverse approach. Following earlier work by Lighthill, he used the hodograph transformation in which the velocity components u and v are treated as the independent variables and the coordinates x and y become the dependent variables. While this results in a linear equation of mixed type, it remains hard to find solutions in the hodograph plane which correspond to physically realizable shapes. Nieuwland had previously generated a family of hodograph solutions which resulted in airfoils that were not practically useful. Paul applied the method of complex characteristics which he had successfully used to solve the supersonic blunt body problem in earlier work to solve the equations in the hodograph plane. He was able to find boundary conditions and integration paths that resulted in usable shock-free airfoils for a range of Mach numbers and lift coefficients. Working with his assistant Frances Bauer and his doctoral student David Korn, he published the first results in the book *Supercritical Wing Sections*.

In this period he made contact with Richard Whitcomb at NASA Langley, who had experimentally developed a supercritical airfoil with a flat-topped shape and heavy rear camber which produced a comparatively weak shock at its design condition. Paul's shock-free 78-06-10 airfoil had a similar though smoother shape. This influenced Whitcomb's thinking, and he decided to fund further studies of supercritical airfoils at the Courant Institute. Paul also made contact with R. T. Jones at NASA Ames and obtained additional funding from Jones to pursue studies of yawed wings.

In 1970, as a staff engineer at Grumman, I was asked to look into the state of the art in supercritical wing technology. I soon found out that Paul's group was at the cutting edge. Eventually I joined the group as a senior research scientist in 1972. Paul was now working on a second book, *Supercritical Wing Sections II*, which presented an improved series of shock-free airfoils, a transonic analysis method (Program H) which included a boundary layer correction, some results of experimental tests, and some preliminary results for yawed wings. My principal assignment was to write the three-dimensional analysis code for yawed wings (Program J, or Flo17) which subsequently evolved into a widely used code for calculating transonic flow over swept wings (Flo22). Program H and Flo22 are still in use today for preliminary design work at Boeing.

The concept of a yawed flying wing for supersonic cruise was the subject of intensive studies at NASA fifteen years later, but no viable design emerged. In the meanwhile Paul continued his studies of supercritical wing design, issuing a third book, *Supercritical Wing Sections III*, in 1977. With Geoffrey McFadden he also developed a three-dimensional inverse design method. By 1980 his interest had switched to magnetic containment

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of plasma for fusion reactors, and this remained the main focus of his research for the rest of his career.

In the period I worked for him, Paul was a wonderful mentor. He exposed me to broad areas of mathematics in which my knowledge was quite deficient. He would do this in a very subtle way by casually asking if I was not aware of this or that, for example, the Bateman variational principle. Then I would be forced to go and find out what he was talking about. He had an extraordinary youthful appearance—at age forty-four one might easily have taken him to be twenty-eight.

To the best of my knowledge none of the airfoils listed in either of the two books was directly used in an actual aircraft, but they had a profound and lasting impact on the aircraft industry by showing for the first time that practically useful supercritical airfoils which are shock free or produce very weak shocks could be designed. This permanently changed the way engineers think about transonic wing design.

The 75-06-12 "Garabedian-Korn" airfoil has been widely used as a benchmark to validate new numerical methods for computational fluid dynamics (CFD). Due to three-dimensional effects, particularly near the fuselage, a satisfactory swept wing cannot be designed with a fixed wing section from root to tip. In a numerical experiment I have substituted the Garabedian-Korn section into a representative modern transonic wing design, the NASA Common Research Model (CRM), which is the test shape for the latest AIAA Drag Prediction Workshops. The initial design produced a very strong shock wave across the entire span. However, using an optimization method based on techniques drawn from control theory for partial differential equations, the wing can be redesigned to produce an essentially shock-free flow. This demonstrates that the Garabedian-Korn section could still be used as the starting point for a competitive wing. This calculation took four hours using a quad-core workstation which is about 5,000 times faster than the Control Data 6600 computers at the Courant Institute in the early 1970s and has about 8,000 times the memory. Evidently such a calculation would not have been feasible in that era. Nevertheless, the outcome after forty years is that all modern transonic commercial aircraft, including business jets as well as airliners, have wing sections which strongly resemble the sections designed by Paul Garabedian.

Eva V. Swenson

Paul Garabedian was my Ph.D. thesis supervisor during 1961–1965. I am indebted to him for taking me under his wing, for patiently coaching and supporting me through the Ph.D. process, and for maintaining an interest in my career and my life ever after. He provided me with a steady compass point that I knew I could turn to anytime I felt I needed to.

As a graduate student and research assistant, I felt fortunate to be assigned the office adjoining his which was quite small, enough for only one person. It allowed me to concentrate on work, and it provided enough space for the voluminous computer printouts that I was generating at that time. By contrast, his own office was a huge corner office minimally furnished with desk, chair, plant, and a couple of file cabinets. The desk itself held very few items; most notably he always had a $4'' \times 6''$ notepad and a pen. His office spoke volumes of his clear and uncluttered thinking. With Paul's steady guidance and gentle encouragement, I pursued various explorations until one day Paul excitedly declared, "Eva, I think you have your result!" He showed me that producing a Ph.D. thesis can be challenging and satisfying, and he enabled me to experience that fantastically rich world where pure and applied mathematics intersect.

He wasn't all work, though. Paul knew very well how to live in balance. I remember well the gatherings on Friday evenings with Paul and Lynnel and his graduate students and their respective girl/boyfriends of the day. On those occasions, I felt that he was just one of us, having a good time. We would go to the public pool for a swim, then to his apartment for martinis. Then we would go to the Old Mill Restaurant for cheap but excellent steaks. One summer, he invited us all out to his place on Fire Island. I remember a scene where we were all sunbathing on the beach, except that Paul was off to one side with his $4'' \times 6''$ notepad and pen, unobtrusively continuing to work on the theorem of the day.

In retrospect, I realize how extremely fortunate I was to have Paul as my mentor. He taught me through lectures, discussions, and, especially, by example. I learned to stay focused: to look for the essential and to trim the superfluous. I learned that if an idea can't be contained in a $4'' \times 6''$ notepad, it is not yet "ready for prime time."

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Paul with daughters Cathy and Emily, 1989.

Ruth Bers Shapiro

The following is drawn from a talk that Ruth Bers Shapiro gave at the memorial for Paul on December 4, 2010. Ruth Bers Shapiro's email address is `ruthbersshap@aol.com`.

Remembering Paul

I am honored that Lynnel asked me to speak today about Paul as a friend and as a husband and father. Paul, Lynnel, and I often spoke of the many ways our lives intersected. Here is a brief chronology. I first met Paul in the early 1950s on a visit to Palo Alto. Paul had been a student of my father, Lipman Bers, at Brown University in the early 1940s. Before our meeting, my father described Paul with great affection as a highly gifted mathematician. He had neglected to tell me, however, that Paul was also handsome, had an irrepressible twinkle in his eye, and loved having a good time. He was by far the coolest mathematician I had ever met.

In the fall of 1957, Lynnel and I met in our freshman year at the University of Michigan, became roommates, and have remained best friends since then. After graduating from college and while working towards a master's degree in English literature at NYU, Lynnel took a job at the Courant Institute. I was not surprised that when she met Paul, she found him charming, adventuresome—before their meeting he had sailed from San Francisco to Hawaii on a four-man sailboat—and romantic.

They were from the start and remained throughout their marriage closely matched on how they saw the world. In politics, vociferous liberals. In religion, confirmed atheists but fully tolerant of others' beliefs. In friendships, unswerving in their loyalty and devotion to their friends. It is hard to speak of one without the other, but I do want to say a few words about Paul's essential values.

Although he could be exacting and discriminating about mathematical elegance, scientific rigor, or musical performance—perhaps even elitist or snobbish—in terms of political and social justice, Paul was consummately egalitarian. He could not tolerate discrimination or injustice of any kind, and he felt that every human being deserved a fair break, an opportunity to have a good life. It was painful to him that in our wealthy nation, people were left stranded. He believed in a visceral way that the privileged and fortunate needed to care for the poor and disenfranchised. He was outspoken and courageous.

Paul was a wonderful friend. If there was a problem and he could help, he never turned away, even when it was uncomfortable and might put him in a difficult position. He remained grateful to people who befriended him or helped him in large or small ways.

Before becoming a parent, Paul expressed the worry that his preoccupation with mathematics would make it difficult for him to be a good father. Lynnel reassured him. In fact, Paul was a passionate, empathic, and supportive parent. It is Lynnel's view that he fell in love with his daughters and was transformed by the love he felt for them. I would second that.

I am sure everyone here knows that Paul could be meticulous—he worked on a cleared desk with only the sheet of paper on which he was writing. When it came to Emily and Cathy, only the constructive aspects of order remained. He wanted them to feel free, valued their spontaneity, and was charmed by their developing personalities. When they were young, Paul was known to refer to his daughters as his two best theorems.

Paul loved life. He fought against the ravages of cancer, refusing even an Advil because he did not want anything to interfere with the clarity of his thought—stubborn in ways, but passionate and intensely alive. It is not surprising that his last paper, written with his devoted student Geoffrey McFadden, was published just four months before he died.

I will miss him as long as I live: his humor, warmth, compassion, and his intensity.

Cathy Garabedian

Memories of My Dad

By the time I was born in 1975, my father was forty-eight years old and was already a well-established mathematician. Earlier that year, before I was even born, he had been elected to the National Academy of Sciences. This honor was lost to me, as my earliest memory of my father is being carried to bed on his shoulder, watching our cozy living room

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grow smaller as I clung to the neck of his six-foot frame. There I am sure he kissed me good night and told me he loved me before tucking me into bed, as he did year after year as I grew up.

As I grew older I slowly became aware that my father had an important role outside our home as someone other than "my Daddy." I remember learning when I was about eight years old that he was writing a book about his work, so I asked him if he would dedicate it to me. When the book came out, I didn't even understand the title, but I was extremely proud that it was dedicated to me, my sister, Emily, and the coauthor's daughter. I still have a copy of *Magnetohydrodynamic Equilibrium and Stability of Stellorators* on my shelf with my dad's handwritten dedication and a smiley face along with the printed inscription. It never mattered to me what the book was about; I was proud of my father just for being my dad. Looking back at my life, however, I realize that my father's work had a great influence on me.

My father had great confidence in his own intellectual abilities and taught me and my sister to have the same confidence in ours. When I was in high school, he would help me with my calculus homework. He would read the problem and then think it through and start explaining how we would solve it, sketching out a plot and carefully writing down a couple of formulas. "See, it's easy," he would say, "You can do this." His words came back many times over the years: while studying for organic chemistry tests in college, trying to understand the Hodgkin and Huxley model of the action potential in graduate school, and teaching myself how to program in Matlab to analyze data for my graduate thesis. "It's easy," I'd think to myself. I knew that my dad believed in me. His confidence in me is a strength I carry in myself. He believed in his daughters so strongly that we couldn't help but believe in ourselves.

In fact, as a father of two daughters, he was fervent in his support of equal opportunity for all. He firmly told us that women were as capable as men in math and expressed his respect for the women he had advised as graduate students. This respect extended to his racially and internationally diverse group of students and colleagues. His egalitarian belief system informed his social and political views, such as his belief in a strong public education system for all and assistance programs for the disadvantaged. I believe that part of his choice and comfort with living in New York City was based in his appreciation of the multitudes of different people who live together in the city.

My father's work as a mathematician took my family around the world, giving us exposure to cultures beyond what we could see even in New York. I spent my sixth birthday in Hong Kong, on the way to Kyoto, Japan. My dad had become enamored with Japan the year before while he was there for a conference, and he and my mother decided to take the family there for his sabbatical. During our year in Japan, Dad started teaching himself Japanese, proudly practicing phrases from his language books and pointing out familiar Kanji signs to us in the subways.

Some of my dad's travels that could have kept him away from his family instead became precious memories of time I shared alone with him. When I was in middle school, he took me back to Japan while he visited colleagues there and gave talks at Kyoto University. My sister was in high school, too busy with classes to join us, and my mom now worked full time. For three years these trips were our special father-daughter time. On the plane the airline attendants would dote on us, thinking it sweet that a single father was looking after his little girl. During my dad's meetings I would do my school assignments, preparing for the presentations on "My trip to Japan" that I would give to my class when I returned.

Aside from math my dad's passion was music. He would sit down almost every evening at our grand piano and play pieces by Chopin, Beethoven, or Mendelssohn. For a man whose work involved the logic and precision of mathematical calculations, his love of the Romantic composers revealed a more emotional side. He would fill the room with strong, intense passages followed by soft, beautiful melodies. My parents encouraged me to take piano lessons from the age of seven so I could share this interest. Later my dad brought out his old violin so we could play duets; we practiced Beethoven's *Spring Sonata* together, culminating in a concert for my piano teacher that earned us both stickers as a token of our accomplishment. But my dad loved nothing more than hearing me sing. In the mornings he'd brush my hair before school, and as he wove it into two braids, I would sing him all the songs I could think of: *Somewhere Over the Rainbow*, *Tomorrow* (from "Annie"), and tunes from "The Sound of Music". When I started performing in musicals, he would come to every show, making sure to get there early to get a good seat and beaming when I came out to take my bow. There was no more proud, dedicated parent.

My dad looked forward to spending the summer at his sanctuary, our house on Fire Island. There he would take walks along the beach each day, enjoying the hard sand and crashing waves at low tide. He would disappear for hours by the ocean. I imagine him mulling over some research problem as he walked mile after mile in the sun, finally returning to our family as the sun got low in the sky. He would wave to us as the outline of his figure got closer and closer on the beach, and he

Paul escorting daughter Emily at her wedding, 2003.

would finally sit down in the sand with us to enjoy the last warm breeze of a summer day. Sometimes he would sit on the porch of our cottage and watch the birds, finding joy in the new hatchlings learning to fly in the spring. My father's sometimes quick temper was balanced by an unbelievable soft spot for the beauty and innocence of nature.

My father was successful at mathematics at a young age and found fulfillment in his work through the very end of his life. He continued to feel passion about his research as he raised his family, running to the office after the presents were opened on Christmas morning to check his computer run, and quietly scribbling ideas in his notebook when he was on vacation with us. However, I never for a moment thought that his family was anything but his top, most important priority. I have never seen as much pride in his eyes as when he walked my sister down the aisle at her wedding, helped my little nephew put on his shoes, and held my giggling niece in his arms before she could even say "Grampa". My family has lost a loving father and husband and we will miss him, but we will never forget the wonderful memories he gave to us, and we will hold them with us always.

Emily Garabedian

The following is drawn from a talk Emily Garabedian gave at the memorial for Paul on December 4, 2010. Emily Garabedian's email address is `emily.garabedian@gmail.com`.

Memories of My Father

I am Paul's older daughter, Emily. Many of you know me. Some of you watched me grow up. Most of this day commemorates Paul's academic achievements and mathematical genius. But I am glad we will spend some time remembering his personal life, celebrating him for the father, husband, brother, grandfather, and friend that he was. I would like to share with you some of my memories of Paul as a father.

Growing up, I remember my father as an intellectual, an academic. He did math, and he played the piano. I never thought of him as an athlete. However, in preparing this eulogy, I realize many of my childhood memories are of sports activities my father did with me, including teaching me how to play tennis. He took me to Paragon Sports and bought me a racquet, and he would reserve a court on the roof of the Coles Sports Center. I'm afraid I never excelled at tennis. For a while, to my father's dismay, I insisted that he reserve a basketball court instead. He didn't play basketball, but he got me a basketball and took me to Coles and practiced with me. I expected my father to help me with my math homework and to give me confidence in my intellectual abilities. But I realize that he also tried to give me the confidence I needed to try out for the basketball team. When I didn't actually make the team, he was there to console me.

My father brought me to the pool at Coles, and we would swim together. Sometimes we would meet people he knew from NYU, and I could hear the pride in his voice when he introduced me, and I knew he thought I was one of the most important people in the world.

My father tried to teach me how to ride a two-wheeled bicycle. He tried to explain to me, at the age of five or six, the physics of keeping the device upright. I learned about gyroscopes and angular momentum, but I wasn't able to ride the bike. In the end it was our doorman who intervened. He would run behind me as I pedaled, holding on to the back of the bike just like my father did, only when I had achieved some speed, the doorman would let go. This was something my father was afraid to do. Although he knew I would have to fall to learn how to ride, he couldn't bear to feel responsible for it.

Paul enjoyed sailing and had a boat, a Flying Junior, that he kept on Fire Island where my family spent the summers. I recall him strapping a life jacket on me over my protests that I was a big girl and I knew how to swim even in water over my head. Paul encouraged me to learn how to sail myself and taught me how lift helps pull a boat forward when it is going upwind. When I proved to be much better at sailing than I was at tennis or

Paul, a lifelong pianist.

basketball, he sold his beloved Flying Junior and bought me a Sunfish, the style of boat that was sailed by the other teenage kids.

In the winter my father would take me ice skating, first at Sky Rink, an indoor rink on the top floor of a skyscraper, and later at the Wollman Rink in Central Park. He knew a few tricks, and we would do figure eights and little leaps he called bunny hops. I wanted him to teach me how to twirl like the figure skaters we would watch together. He wasn't able to do that trick, but he explained to me again about angular momentum and pointed out how the skaters would spin faster when they pulled their arms in close to their bodies. I loved holding hands with my father as we skated around and around the rink. I continued to hold his hand while we skated, many years after I no longer needed it for balance.

My daughter is turning four years old soon, and her big gift is going to be her first two-wheel bicycle. I'm sad that my father won't be around to see her learn to ride it. I was sad when I took my children ice skating last week. I told both my children that their grandfather could have taught them how to skate. I am trying to help my children retain their memories of their grandpa, talking to them about the kind of person he was and the things he liked to do with them, like helping them put on their shoes.

And so we keep my father's memory alive through our recollections of him, his dedication to his family and to his work which will be carried on.