Thomas H. Wolff (1954–2000)

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Thomas H. Wolff, a leading analyst and a winner of the Salem and Bôcher Prizes, was killed in an automobile accident on July 31, 2000, when he was forty-six years old.

Tom was raised in a mathematical environment. His uncle, Clifford Gardner, was a professor at NYU's Courant Institute of Mathematics for many years, and Tom's mother, Lucile, was a technical editor of volume 1 of the English translation of the celebrated book *Methods of Mathematical Physics* by Courant and Hilbert.

Tom was an undergraduate at Harvard, where, he once told me, he regularly played poker with a fellow student named Bill Gates. After graduating from Harvard in 1975, Tom went to Berkeley, where he got his Ph.D. under Don Sarason in 1979. Tom then spent one year at the University of Washington and two at the University of Chicago before coming to Caltech in the fall of 1982 as an assistant professor.

Tom spent most of the rest of his career at Caltech, although, for personal reasons, he resigned twice, spending two years (1986–88) at Courant and three (1992–95) at Berkeley. His promotion or appointment to a professorship at Caltech three times is a record for our institution.

Tom is survived by his widow, Carol Shubin, a mathematics professor at California State University, Northridge; two sons (aged three and five at his death); his parents; and two sisters.

—Barry Simon

Photographs included in this article are courtesy of Carol Shubin.

Lennart Carleson

Harmonic analysis has a position in mathematics comparable to that of the theory of the atom in physics. By understanding what goes on at the micro level, we can understand large-scale phenomena (even such as meteorology). By Fourier expansions we can analyze and understand global functions, arithmetic problems, or differential equations. As physics has a miracle method in quantum mechanics, we use the theory of analytic functions, and of course, in a deeper sense, all this comes together as one piece. These methods are efficient when the number of variables n is either small or very large (where the theories become probability). In the intermediate range, say $10 < n < 100$, physicists and chemists have been more successful than we mathematicians, and it remains an important challenge for the future to develop relevant harmonic analysis in this range.

The problems in harmonic analysis (of few variables) today, after two hundred years of research, are very combinatorial and very complicated. Tom Wolff had a unique talent and a profound knowledge in the area. The fundamental problems that he considered required long preparation, deep concentration, and an ability to keep a very complicated set of arguments simultaneously active and available in his mind. Two quotations come to mind. The first is Newton's answer to how he

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found the law of gravitation: "By constantly thinking about it," and the second is Tom's remark when he was awarded the Bôcher Prize in 1999: "It's never been easy for me." Nobody else could have done what he did.

Let me highlight some of Tom's most striking results, all coming from combinatorial harmonic analysis. Tom's thesis and early work were in the direction of complex analysis. (His thesis is described below by D. Sarason.) While working on his thesis Tom found a new proof of the corona theorem for H^∞ of the unit disk. Here he managed to summarize the combinatorics in a beautiful lemma on bounded solutions, u of $\bar\partial u = f$. As first pointed out by Hörmander, one should first solve the relevant equations in a nonanalytic way and then modify the solution to make it analytic bounded, which leads to the above equation. To get the boundedness of the solutions, the old combinatorics came back, and this is where Tom's lemma works; see [4].

This lemma has had a large impact, although Tom never published it. He did not return much to function algebras. I can mention a joint paper with Alan Noell (1989) which gives an almost complete description of sets E on $|z| = 1$ where an analytic function $f(z)$ in $|z| < 1$, satisfying $|f(z) - f(w)| \le C|z - w|^\alpha$, $|f(z)| \le 1$, $f(0) = 0$, can be $= 1$ (so-called peak sets). This is surprisingly hard!

From function algebras Tom turned to potential theory, where his most important work in the 1980s can be found. The first fundamental result [5] is from 1983. L. I. Hedberg had proved that if Ω is an open set in $\mathbb{R}^n$, then $C_0^\infty(\Omega)$ is dense in all higher-order Sobolev spaces $W_0^{m,p}(\Omega)$ when $p > 2 - \frac{m}{n}$. The exceptions $1 < p \le 2 - \frac{m}{n}$ had their origin in potential theory and more precisely in the missing Kellogg lemma on the size of the set of thin boundary points outside that range. Tom closed this gap by a clever construction. Today more streamlined approaches exist.

Tom next turned to harmonic measure. Let Ω be a domain in the plane containing ∞, and suppose that we can solve the Dirichlet problem in Ω for arbitrary continuous f on $\partial\Omega$. The value of the solution at ∞ is given by integrating f against a positive probability measure ω in the plane supported on $\partial\Omega$. This is called harmonic measure and is also the hitting probability on $\partial\Omega$ of a Brownian particle in Ω, starting at ∞. From this interpretation, it is natural to surmise that most measure lies in the exposed parts of $\partial\Omega$. The harmonic measure should therefore be 1-dimensional; more precisely: $\exists ?E$ of dimension ≤ 1 on $\partial\Omega$ with $\omega(E) = 1$. By using the Riemann map, N. G. Makarov proved this in 1985 in a very precise sense for simply connected domains. For general domains it is much more complicated. The main difficulty comes from those parts of $\partial\Omega$ where $\omega > 0$, but barely so. Consider, for example, $\Omega =$ complement of disjoint disks

$$D_{\nu\mu} : |z - a_{\nu\mu}| \le \tau_{\nu\mu}, \quad a_{\nu\mu} = \frac{\nu}{N} + i\frac{\mu}{N},$$
$$0 < |\nu|, |\mu| \le N,$$

and choose $\tau_{\nu\mu}$ so that $\omega(D_{\nu\mu}) = \frac{1}{4N^2}$ for all ν and μ. The radii in the outer part of the square must be very small, but a proof is not easy and the precise size of $\tau_{\nu\mu}$ is not known (to me). It was a tour de force when Tom and Peter Jones in 1988 proved the statement about $\dim(E) \le 1$ in full generality [6], and Tom even proved (published in 1993 but from the same time) that E can be chosen of σ-finite length. In the above example, Tom's method proves that $\sum \tau_{\nu\mu} \le C$, $|a_{\nu\mu}| \ge 1/2$.

There is a natural conjecture for the analogous problem in higher dimension n (Øksendal 1981) that $E \subset \partial\Omega$ can be chosen of dimension $n - 1$. In 1987 Tom produced a counterexample to this conjecture for $n = 3$. This was a sensation not only because of its complexity but also because it simultaneously disproved two other conjectures. It was published only in 1991 [11].

The construction is based on the following lemma: For each unit vector e in $\mathbb{R}^3$, there is a harmonic function u in $x_3 > 0$, vanishing at ∞, so that

$$(*) \qquad \int_{\mathbb{R}^2} \log|e + \nabla u|\, dx_1 dx_2 < 0.$$

This fails in two dimensions by subharmonicity. This entropy-type of integral is relevant because the counterexample domain Ω is obtained in a dynamic way by a snowflake construction where almost independent products of gradient vectors as above occur. (A result by Bourgain tells us that $\dim(E) \le n - \delta_n$ is possible, but the correct δ_n is not known.) Tom's construction also solves (partially) a problem of Bers: There is u harmonic in $x_3 > 0$, $C^{1+\alpha}$ up to $x_3 = 0$, such that $u = |\nabla u| = 0$ on a set of positive measure on $x_3 = 0$. Here α is

Ph.D. Students of Tom Wolff

Ivo Klemes, Caltech, 1985
Peter Holden, Caltech, 1987
Gregory Hungerford, Caltech, 1988
Stewart Gleason, NYU, 1990
Dean Evasius, Caltech, 1992
Wensheng Wang, Caltech, 1993
Lawrence Kolasa, Caltech, 1994
Wilhelm Schlag, Caltech, 1996
David Alvarez, UC Berkeley, 1997
Themistoklis Mitsis, Caltech, 1998
Oleg Kovrijkine, Caltech, 2000
Burak Erdogan, Caltech, 2001

Tom Wolff at his desk in Luminy in the summer of 1994.

a fixed number, but it is not known if we can take, say, $\alpha = 1$ (most likely not!). The construction also disproves a third conjecture which I omit. In more recent work, the explicit and painful construction $(*)$ has been simplified and extended to certain powers of $|e + \nabla u|$, but a complete picture is still missing.

During the early part of the 1990s, Tom's research was focused on "unique continuation" (UC). In its simplest form this concerns differential inequalities such as $|\Delta u| \leq V_0(x)|u(x)|$ or $|\Delta u| \leq V_1(x)|\nabla u|$. If we know that u vanishes to infinite order at a point ($x = 0$) (strong UC, SUC) or in an open set (UC), under which conditions on V_i can we conclude that $u \equiv 0$? This has obvious implications for uniqueness of solutions to PDE. All work here originates in a paper by T. Carleman from the 1930s. One studies norms of functions such as $|x|^{-t}|u|$ or $\exp(k \cdot x)|u|$ as the parameter t or k grows. For V_0 and SUC, Jerison-Kenig proved in dimension d that $V_0 \in L^{d/2}$ suffices, which is the optimal exponent, and Tom proved the analogous statement for $V_1 \in L^d$, $d = 3, 4$. He also proved that $\frac{3d-4}{2}$ suffices. For UC, Tom proved that exponents $d/2$ for V_0 and d for V_1 suffice, but here it is not at all clear if these exponents are relevant: e.g., L^1 may suffice. His method is an impressively detailed analysis of the sets where real Laplace transforms

$$\int \exp(k \cdot x) d\mu(x)$$

of positive measures are of maximal size [10].

Tom's main interest during the last years of his life was "Kakeya sets". The methods here had the right mix of classical harmonic analysis, geometry, and combinatorics to fit his unique talents. As a problem in harmonic analysis, it goes back to Charles Fefferman's solution of the "ball-multiplier-problem" (1971). If $\hat{f}(\xi)$, $\xi \in \mathbb{R}^d$, $d \geq 2$, is the Fourier transform of $f \in L^p$, $p > 1$, is $\hat{f}$, restricted to $|\xi| \leq 1$, the Fourier transform of some $g \in L^p$? This is true for $d = 1$ (Hilbert transform) and for $p = 2$ and all d, but Fefferman gave a counterexample for all other p and d. The obstruction comes from thin layers $1 - \delta \leq |\xi| \leq 1$ which contain long ($\sim \delta^{1/2}$) rectangles of width δ of all directions. In this way the counterexample comes from Besicovitch's 1928 construction (as a solution of a problem of Kakeya's concerning "sets where you can turn a needle") of a compact set in $\mathbb{R}^2$ of zero measure that contains a line segment of length 1 in every direction. (For this classical theory see [3].) It is rather easy to see that the Hausdorff dimension of a Kakeya set is 2. The corresponding conjecture in $\mathbb{R}^n$ concerning needles in any direction is open. One can also study restriction problems of $\hat{f}(\xi)$ to $|\xi| = 1$ (Stein 1976). Kakeya problems for circles (the dimension of a compact set in $\mathbb{R}^2$ that contains a circle of every small radius) are similarly related to these restriction problems. Bourgain introduced the idea of fattening the segments and circles to have width δ and then looking for the size of the constant $C(\delta)$ in L^p-estimates of the corresponding maximal functions (δ-method).

Here Tom contributed very important new ideas and incorporated methods from combinatorial geometry. The most striking is the proof (1997) (using the δ-method; here an L^3-estimate is relevant; see [12]) that a Kakeya set for circles in $\mathbb{R}^2$ has dimension 2. For the needle case in $\mathbb{R}^n$, the dimension estimate $\frac{n+1}{2}$ is easy, but Tom proved (1995) $\frac{n+2}{2}$. A growth rate better than $\frac{1}{2} \times n$ as $n \to \infty$ has been proved by Bourgain.

Much remains here, and we are still hoping for a deeper understanding of how lower-dimensional objects fit into higher dimension. There is little doubt that this type of mathematics is of great importance for the analysis of the 3-dimensional structure of molecules and therefore of, for example, polymers.

What I have summarized here is a sample of Tom's work. The common denominator is combinatorial harmonic analysis, and it was here that he had his unique talent. Where most of us would give up because of the complexity, he would organize the facts and concentrate for a very long time until the goal was reached. He had, however, wide interests in mathematics and was always eager to understand and discuss. This resulted in papers on interpolation spaces, Hardy spaces, geometry, and, let me finally mention, a nice joint paper with Barry Simon on spectra for self-adjoint operators.

Donald Sarason

It was my good fortune that Tom Wolff became interested in my area and decided to write his dissertation with me. He worked on some questions in one-variable function theory with which I was then involved.

Tom's graduate school years (1975–79) came toward the beginning of the BMO era, initiated around 1972 by Charles Fefferman and Elias Stein. Fefferman's duality theorem for the space of functions of bounded mean oscillation, a high point in the program to develop real-variable methods in harmonic analysis, was having a large impact in the complex realm as well.

In the study of Toeplitz operators there had arisen a certain function space known as QC (for quasicontinuous). It consists of the bounded functions on the unit circle in the complex plane writable as sums of continuous functions and Hilbert transforms of continuous functions. The space turns out, for nonobvious reasons, to be closed both under the essential supremum norm and under multiplication, thus is a Banach subalgebra of the algebra L^∞ (on the circle). From Fefferman's theorem one can derive an alternative description of QC: it consists of the bounded functions on the circle that have vanishing mean oscillation. A related algebra is QA, consisting of the functions in QC that are boundary functions of bounded holomorphic functions in the unit disk.

A basic problem about QC which had stumped me was that of characterizing its zero sets. I had been unable even to formulate a plausible conjecture. While I did not assign Tom this problem specifically, he seems to have gravitated to it instinctively. He eventually solved it in a very imaginative and unexpected way.

Tom came into my office at one point rather discouraged, feeling he wasn't getting anywhere. I forget exactly what I said to him, but undoubtedly it consisted of the usual reassuring words about how nearly every mathematics Ph.D. student has a similar experience, about how a mathematician working on a hard problem can expect to be stuck for long periods, about how progress would come if he just kept at it. It was not too many days later that he reappeared in my office, this time to announce his beautiful resolution of the zero set problem. He in fact simultaneously resolved several other questions about QC. Needless to say, such a moment is a dissertation supervisor's ultimate gratification.

Tom's basic theorem states that every function in L^∞ can be multiplied into QC by a nonzero function in QA. Since nonzero functions in QA are in fact nonzero almost everywhere, it follows immediately that every measurable subset of the circle is the zero set of a function in QC, a result that surprised me.

Tom's proof of the theorem involved an insightful application of BMO techniques. The theorem was unexpected because QC functions possess a sort of average continuity that general L^∞ functions lack. The theorem says that, in some sense, the set of discontinuities of a general L^∞ function is smaller than we had imagined. Tom showed that a Banach algebra perspective enables one to make this statement precise.

Everyone who knew Tom during his student days in Berkeley recognized his intelligence. It was not until that day he came to my office with his breakthrough that I appreciated his remarkable talent.

Tom's dissertation is somewhat removed from the main currents in harmonic and complex analysis. It is thus not widely known, despite its elegance and beauty. After completing it, but while still a Berkeley student, Tom made another breakthrough. Word of his work on the corona problem spread quickly and made him famous.

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Sun-Yung Alice Chang

I first met Tom Wolff in April 1979, the year he finished his Ph.D. at UC Berkeley. We both had written our thesis under the supervision of Donald Sarason. The topics of our theses—the study of the behavior of functions defined on some subalgebra between H^∞ (bounded analytic functions) and L^∞ of the unit circle on the plane—are closely related. I was an assistant professor at the University of Maryland, College Park, where Lennart Carleson was visiting during the special year in harmonic analysis. Tom came to give a lecture on his simple, elegant re-proof of Carleson's result on the Corona problem. It was a striking experience to see this shy, humble young man talking about his work in front of the world's leading expert on the subject. The beauty of Tom's proof and the sharpness of his mind left a deep impression.

Later on we had many occasions to meet in conferences and to have joint seminars together while he was at Caltech and I was at UCLA. Tom had a comprehensive and incisive understanding of many topics in analysis. Those of us around him all greatly benefited from the interactions with him. In particular, during the month of May 1990, while Tom and I both were visitors at IHÉS (Institut des Hautes Études Scientifiques), we spent long hours talking. I got to know and was deeply impressed by his work on Bers' problem, his criticisms—

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unsparing of his own work—of contemporary mathematics, his heavy sense of responsibility toward his students, and also his point of view on politics in the mathematical community and society in general.

I always enjoyed talking to Tom about the mathematics projects each of us was involved in. Although our interests later diverged, Tom's comments were always insightful and honest. We also had two joint papers together; each grew out of discussions where his contribution was so significant that I, together with my coauthor, invited him to join our work. In [2] we partially answered a question posed by Charles Fefferman: Let v be a nonnegative integrable function on $\mathbb{R}^d$; when is it true that

$$\int_{\mathbb{R}^d} |u|^2 v \, dx \le c \int_{\mathbb{R}^d} |\nabla u|^2 \, dx$$

for some constant c? When $c = 1$, this is equivalent to the positivity of the associated Schrödinger operator $L = \Delta - v$ when L is essentially self-adjoint on C_0^∞. We also answered a question raised by Elias Stein by showing that if a square function $S(f)$—which is a variant of Lusin's area function—is bounded, then the function f is in the Orlicz space $\exp(L^2)$ and this is the best order. This latter result has been useful in the work of Robert Fefferman, Carlos Kenig, and Jill Pipher; in the study of Hardy spaces; and also in the work of Chuck Moore and Michael Wilson. In [1] we gave examples of sequences on a compact d-dimensional manifold ($d \ge 3$) in a fixed conformal class satisfying a uniform $L^{(d/2)}$ bound on curvature and a bound on volume that are not compact in a C^0 topology, which indicated that the result in the thesis of Matt Gursky is sharp.

Our mathematical community has lost a leader at the prime of his productivity, and many of us who worked with Tom have lost a friend.

Peter W. Jones

I was a Dickson Instructor at the University of Chicago when I first heard about Tom Wolff. He had written an outstanding thesis under the direction of Donald Sarason and created quite a stir with his now celebrated method of proving Lennart Carleson's Corona Theorem. At this time the analysis group at Chicago was led by Alberto Calderón, with Bill Beckner and Bob Fefferman the junior professors. Antoni Zygmund was old and infirm, but made a point of coming in every day and never missed a seminar. I presented Tom's results to Calderón, Beckner, and Fefferman and urged them to try to bring him to the university. In the spirit that reigned in the department, the matter was discussed for a minute or two, and then Calderón declared the situation to be obvious: Tom must come.

At the time Tom arrived the University of Chicago was an exciting place for Fourier analysts. The full ramifications of H^1-BMO duality, proved a decade earlier by Charles Fefferman, were still being worked out. (Tom's new proof of the Corona Theorem was very much a product of that program and is now seen as a model application of those L^2 methods.) Calderón had recently proven the L^2 boundedness of the Cauchy Integral on small constant Lipschitz curves, and it was clear that a whole new area had opened around that result. There were lots of young analysts who flocked to Chicago in those years, but virtually from the start it was clear that Tom had a special brilliance.

Very rapidly after arriving, Tom broadened his scope and was working on conjectures in many different areas. His approach to mathematics was remarkable and obvious even to those who knew him only slightly. Tom's hallmark was to select a problem where the present tools of harmonic analysis were wholly inadequate for the task. The exact area was not necessarily so important, but he had a knack for finding precisely the central problems. Beginning with almost no background, he would interview experts at length and devour the literature. Within a month or two he knew basically everything that was relevant to the problem and would then turn in earnest to the attack.

This is the part I remember best: when Tom was in full gear. He would enter a period of extreme concentration, several notches up from his usual intense state. During this period nothing could distract him, and he would stay in Eckhart Hall until late in the night, when he returned the few hundred meters to his apartment. In the day he could be seen pacing the corridors with a cup of coffee in hand. Other frequent haunts were the coffee lounge or the front steps of Eckhart Hall, where he could be found smoking a cigarette. He had a very characteristic body language, with his shoulders hunched slightly forward and a distant gaze in his eyes. All the time he was calculating lemmas or trying out a different tack. Tom was not always communicative about what he was working on, even though it was clear that something was up. I would sneak into his office on these occasions and look at his voluminous notepads, trying to divine the exact nature of the problem. Eventually, the mathematical door would open a crack as Tom discovered a new technique, usually of astonishing originality. The end would now be in sight, as Tom unleashed his tremendous technical abilities and overcame the remaining difficulties. Tom attributed his results solely to hard work, but I never found this a satisfactory explanation and believe the true answer to be a mystery.

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It was my fortune to write two papers with Tom. One of these, also coauthored by Donald Marshall, showed that (e.g.) if given corona data in the disk algebra, one could find corona solutions with one of the functions invertible. This was not too hard, but the other problem we tackled was tough to crack. Øksendal had conjectured that for an arbitrary domain in $\mathbb{R}^n$, the harmonic measure was supported on a set of (Hausdorff) dimension at most $n-1$. N. G. Makarov first proved a very sharp version of this for simply connected planar domains. Tom's paper with me solved the problem for general planar domains, using a delicate geometric construction. It was during this time that I got to see first-hand all of Tom's amazing talents, both conceptual and technical. Later, Tom disproved Øksendal's conjecture for $n \geq 3$, and this has deep physical significance. It is not much of an exaggeration to say that this explains why lungs work! Perhaps the most astonishing thing about that particular paper is that Tom also solved two other well-known, only philosophically related, conjectures. It took quite some time for Tom to write up that paper. I recall him explaining that the problem-solving phase was exciting but the writing was drudgery.

Time after time, Tom Wolff would pick a central problem in an area and solve it. After a few more results, the field would be changed forever. Tom would move on to an entirely new domain of research, and the rest of the analysts would spend years trying to catch up. In the mathematical community, the common and rapid response to these breakthroughs was that they were seen, not just as watershed events, but as lightning strikes that permanently altered the landscape.

Peter D. Lax

I have known Tom's parents for over fifty years. I met them through Tom's maternal uncle, Clifford Gardner, an outstanding applied mathematician whose accomplishments have been recognized by a Norbert Wiener Prize, awarded jointly by the AMS and SIAM (Society for Industrial and Applied Mathematics). Clifford was the first to arouse Tom's interest in mathematics. The second was Jürgen Moser. Their paths crossed at Loon Lake, an isolated spot in Franklin County in northernmost New York State, where the Mosers and the Wolffs have had vacation homes since 1970. Jürgen and young Tom were drawn together by their love of hiking (although Jürgen liked well-mapped footpaths, while Tom loved to bushwhack, which made my sons dub Tom "the Viking"), a love of the cello, and a joint project to build—with their bare hands—a log cabin in the woods, a retreat from the madding crowd. All this gave ample chance for them to discuss mathematics.

Wolff with his uncle Clifford Gardner (November 1994).

When Tom's affairs brought him to settle on the East Coast, we at the Courant Institute jumped at the chance to appoint him to the faculty. We were very enthusiastic about his research, but a little apprehensive about his teaching because of his shyness. We needn't have worried; within a short time he became the most popular teacher in the department because of the clarity of his lectures and the wholehearted support he offered his students. We were very sorry to lose him when another change in his life brought him back to his beloved California.

When news of the accident that took Tom's life reached Loon Lake this past summer, it was a thunderbolt from a sunny sky. "As flies to wanton boys are we to the gods, they kill us for their sport."

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Nikolai Makarov

The first time I heard of Tom Wolff was in the early 1980s when the news of his amazing new proof of the Corona Theorem reached Leningrad. I still remember the excitement we felt as the proof was presented in the analysis seminar. Before long, several other spectacular results by Tom followed. For us working in Leningrad on problems in linear and complex analysis, his name became almost legendary.

In the course of his career Wolff deeply influenced various fields of modern analysis. He made groundbreaking discoveries in harmonic and complex analysis, potential theory, and differential equations. In the last, exceptionally productive,

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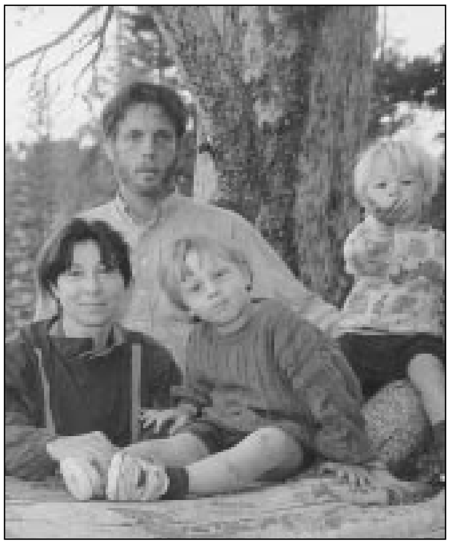
years of his life, the Kakeya problem techniques became his main topic, and Wolff made great progress in some of the most fundamental problems of harmonic analysis related to geometric measure theory. His work had remarkable depth and breadth.

One part of Wolff's legacy that I can appreciate the most was his work concerning harmonic measure in the complex plane and in higher dimensions. I had been working on related problems, and when I first saw Tom's results, they looked like a miracle to me. He proved (with Peter Jones) a long-standing conjecture that harmonic measure in the plane lives on a set of dimension at most one. He also showed that a similar fact does not hold in higher dimensions. The latter is a stunning example of how one person can change the whole subject. By constructing beautiful and rather surprising counterexamples, Tom disproved all conjectures that existed in this area and showed that the most basic facts about harmonic functions in the plane fail in dimension three or greater. At the same time, he indicated new directions in the study of the higher-dimensional case.



Tom Wolff with family in Mammoth, CA, on his 45th birthday (July 1999).

Tom never wasted time on finding all possible applications of his ideas. After solving the problem, he would move on to a different subject, leaving it to other people to fully implement his inventions. Some of his arguments (such as his proof of the Corona Theorem) impress with elegant simplicity. But more often his argument would be of the "hard analysis" type, and Tom was a genius of "hard analysis". His typical proof would be based on some very complicated and incredibly clever combinatorial construction of a geometric nature.

Reading Wolff's papers is a difficult task, though he put great effort into writing his papers meticulously. It usually takes a long time to fully understand the depth of his ideas, but the upshot is very rewarding for the reader. Tom had great success in raising graduate students. Part of the reason, I believe, was that by merely studying his constructions, the students were able to discover new ideas and master all the latest technologies.

Tom commanded unanimous respect and admiration. He was a perfect colleague: very generous with his insights and ideas, uncompromisingly honest, extremely respectful of other people. He had friends all over the world. Many analysts are deeply indebted to him for his advice and support. I had the privilege of working with Tom at Caltech for several years. He was a fine person, invariably friendly, reliable, and helpful. Talking mathematics with him was always thrilling and inspiring.

Most people would agree that Tom Wolff was one of the greatest analysts of our time. Those of us who were lucky enough to have known him personally feel a tremendous sense of loss of someone very special.

Barry Simon

I was a colleague and admirer of Tom. Three aspects of Tom's personality were especially noticeable. The first was his passion and intensity. Tom was not only passionate about mathematics, he was passionate about mathematicians and went out of his way to help young mathematicians all over the world, both with their mathematics and with making sure they got the recognition they deserved. As for his intensity, Tom was a fixture on campus pacing outside the department, lost in thought. The most common comment I got after Tom's death—from Caltech's provost to a delegation of undergrads—was that they missed him when they came in in the morning and didn't see him pacing there.

The second was his honesty. Tom didn't wave his hands and didn't let others get away with waving their hands. The third was his shyness, which was so strong that one could see him overcome it when dealing with others.

Tom was not only a deep thinker in mathematics but also a technical master. My own joint work with him arose when I was thinking about the implication of some ideas of Kotani for localization in random quantum systems. I realized that one could base things on rank one perturbation generalities if only I could prove a certain fact about measures. After a week of trying, I called in Tom. We spent several hours trying to crack it with no success, but the next morning Tom walked in with the needed result in hand. Our subsequent talks led to a simple way of avoiding the problem totally and resulted in our localization criterion [9].

As a side benefit of our joint work, Tom got interested in problems of localization and returned to the subject ten years later in a beautiful joint paper with Shubin and Vakilian [8] and, just prior to his death, a joint work with Klopp [7]. Typically, these papers exploited subtle ideas from harmonic analysis. In particular, Tom's insight on the role of

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uncertainty principle inequalities in the subject is a significant contribution.

Tom was a gentleman and a gentle man. I miss him.

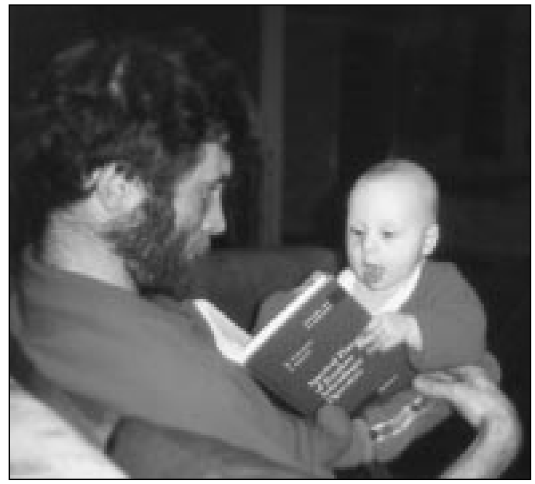
Markus Keel

I first met Tom Wolff four years ago when he'd visit the UCLA analysis seminar, but got to know him better while a postdoc at Caltech these past two years. From his research, to his teaching, to his day-to-day scientific interactions, Tom's unflinching honesty had as much to do with his impact on me and my generation as did his incredible analytical strength.

Tom's utter lack of vanity should be seen first in the light of his work, the depth and breadth of which are outlined by the collection of authors here. Even at the time of his death, Tom's preprints were nourishing entire fields. In [13], for example, he answered (up to an endpoint) a conjecture of Klainerman and Machedon dealing, roughly, with the way that two solutions to the wave equation can interact. Tom's paper surprised everyone in the field, and it seems likely that the arguments he introduced there will seriously impact work on the regularity and stability of nonlinear wave equations.

Tom's teaching had a similar effect on the students and young faculty at Caltech, from his undergraduate calculus courses to the advanced harmonic analysis class he taught here last spring. Twice a week Tom would speed into the room, looking for all the world like he'd just wrestled about 300 wild cats, half of whom were wielding squirt guns loaded with coffee. The lecture that followed was both terrific and terrifying: he'd cover intricate arguments in a way that made them appear almost inevitable, but a weird unease would creep over me towards the end of the term. At my very best, I realized, the mathematics classes I teach are a lot like taxidermy—the stuffing of a cadaver so that if you don't look too closely or too long, you might be fooled into believing it's alive. Tom, on the other hand, put an intensity and care into his lectures which made them nothing short of reanimation. By the end of the course we really believed that the ideas he had presented were still vigorous, still steeped in potential.

The way he listened and spoke in informal mathematical discussions was similarly unique. Tom would listen not just to your words, but for the hidden biases and unacknowledged gaps that color most arguments. For example, I once kept Tom at a table in the Caltech cafeteria to see if he had any ideas for a counterexample to an estimate which I, and every senior mathematician I could corner, thought was probably false. Tom gave a little bit of advice, but seemed reluctant to speak over lunch or maybe hesitant to say anything that was less than absolutely precise. Five minutes after I'd returned to my office I found out his reticence had more to do with his doubting my entire premise: I answered his light knock, opened the door, and in poured Tom to explain that it'd be a real chore to produce a counterexample, since the estimate was actually true. He put the proof on the board, apologized for interrupting my work, set the chalk down, and strode right back out again.



Wolff "reading math" with son Ricky.

Tom Wolff was one of the most potent human beings I've ever met. It was a great thrill to work in the vicinity of the man.

Wilhelm Schlag

I met Tom Wolff in the fall of 1992 at UC Berkeley. I was a first-year graduate student and Tom had just moved there from Caltech. He taught an introductory class in harmonic analysis that same term that left a lasting impression on me. He lectured with great care, paying particular attention to details that are frequently skipped in classes of this type. At the same time he would draw our attention to the essential points, not allowing the details to obscure the main line of the argument. He thus made fundamental results of the subject completely transparent.

I think it was very clear even to us first-year students that he was a master in his field. His seriousness about mathematics and his high standards made him an ideal, albeit demanding, advisor. He insisted on seeing his students every week, and the discussions we had were always fruitful. It was clear that he also enjoyed discussing mathematics with us, and he was basically available at all times.

I think that the most inspiring feature of his personality was his uncompromising and relentless search for the key points of a problem. He would assemble the main facts and methods and then combine them in an almost miraculous way that

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made everything appear simple. This was true of his teaching as well as of his research. His command of the subject as well as his somewhat serious appearance could make him intimidating at times. But this really belied his gentle personality.

I am very lucky to have met Tom, and I regard it as a privilege to have been his student.

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