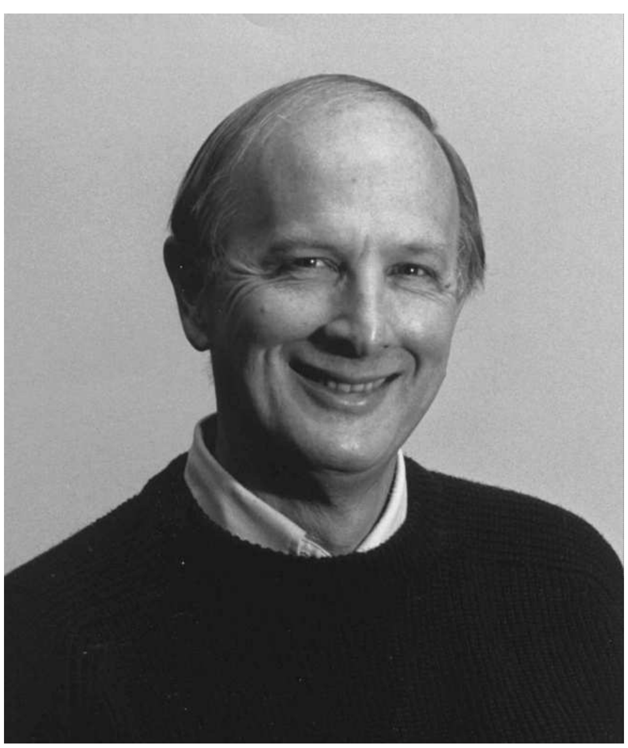
In Memoriam
Frederick J. Almgren Jr., 1933–1997

Fred Almgren was one of the founding editors of *Experimental Mathematics*. He remained an active editor until mid-1996, when he was found to have myelodysplasia, a form of bone marrow cancer. Subsequently, Fred underwent a risky but potentially effective treatment at Brigham and Women's Hospital in Boston: his old marrow cells were destroyed, and he received a marrow cell transplant.



He was released after about a month and returned home to Princeton, and for several more weeks gained health and strength rapidly. He appeared to be on the road to a remarkable recovery, gradually resuming many of his normal activities, such as taking long walks with his wife Jean. In January, however, he developed pneumonia, and was hospitalized again. Complications ensued, and he died on February 5, 1997.

I first met Fred in 1966, at the International Congress of Mathematicians in Moscow. I gave a splinter talk in which the result happened to be something that Leslie Federer had already proved, but I didn't know this. Fred sat silently through the talk, only revealing the bad news to me when we were alone.

Fred was a great believer in the value of using computers to support progress in pure math. He and I worked together with Al Marden as part of the Geometry Supercomputer Project in Minneapolis, which later developed into the Geometry Center (see page 11). He was a strong contributor to the considerable successes of the Center. For example, it was his support that led to Ken Brakke's important Evolver software.

Experimental Mathematics, the journal, has lost an important editor; experimental mathematics, the subject, has lost an important practitioner. We will miss him for his enthusiasm and for his dedication, and as a tribute to him we have included in this issue reminiscences and memorial statements that we believe will be of interest to our readers.

– David Epstein, Chief Editor

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Doing Math with Fred

Elliott H. Lieb

Fred's passing leaves a big gap and I am grateful for the opportunity to share some of my thoughts about him and personal feelings. He had a huge impact on my professional life, as he did on the lives of many mathematicians, and I would like to say a few words about its origins and meaning.

Before his untimely passing, Fred was surely one of the leading geometers in the world and a central figure in the discipline of geometric measure theory. The modern era of this field is perhaps some forty years old and Fred and his many students and co-workers solved some long-standing problems in geometry and the calculus of variations, and opened new directions for the future of both subjects. One example of his many contributions is that he enlarged the notion of surface to ones of possibly infinite complexity in order to attack the two-centuries-old classical problem of surfaces that minimize area.

As I am far from being the most qualified person to speak of Fred's mathematical accomplishments, I hope I will be forgiven for focussing instead on our own interaction and the what it meant to both of us. This is the account and testimony of just one person who cherished his friendship.

Oddly, although I had been in Princeton since 1974 (and Fred since 1962) we passed few words before about 1983. I cannot remember the exact time or what it was that ordained this meeting, but after that it was as though we had been colleagues from the beginning of time.

What was strange, or perhaps it is better to say noteworthy, about this was the rather large divergence in our backgrounds—in more ways than one. We came from opposite sides of the political spectrum, opposite sides of the country (Fred originated in Alabama, I in Boston), different backgrounds, different attitudes to the military and authority (Fred was a navy pilot who used to land planes on aircraft carriers, while my only direct involvement with the military consisted of an aborted ROTC experience), and different attitudes towards computers (Fred, despite his conservative political views, was one of the early revolutionaries, while I held on to my pencil until the last, but finally succumbed to the new wave under Fred's patient influence and guidance).

Most striking, however, was our apolar scientific background. Fred came from one of the most rigorous of mathematical traditions, Federer's geometric measure theory, while I originated from a theoretical physics tradition in which rigorous thinking was considered to be cerebral calcification. Over the years I had managed to shake off this unenlightened view and become a mathematician, but I could not, and never will, come up to Fred's level of precision of thought. It was truly impressive and it is hard to parallel, even among mathematicians. The slightest ambiguity or unfinished loose end would set his mind in action, and it would not stop until he had put the whole business in a full-fledged logical framework from which it could be confidently viewed. The ultimate in this direction was a paper of one thousand seven hundred and twenty pages that Fred wrote, and never published, but which circulated in samizdat among all workers in geometric measure theory and beyond, and is legendary for the depth of the problems it analyzes.

We collaborated on two major projects: singularities in liquid crystals and continuity of symmetric decreasing rearrangement. They ended up taking several years each and left substantial marks on our careers and perceptions of mathematics. A mathematics lecture is not my aim here, but I would like to say that there was a resonance between us that I have seldom enjoyed otherwise, and the same must have been true on Fred's side because it turns out, somewhat unexpectedly, that I was his major collaborator, by far, in the math department, in terms of number of papers, at least. Indeed, one of the features of Fred's work, that tends to mark it apart in our times, is the fact that much of it was written solo. In both cases, I came to Fred with some questions and the intuition that he would know how to lead us to a solution. In both cases we started out thinking we knew what the answer was, assuming that there is justice in the universe; in both cases Fred took the sketch of a proof we had and started asking embarrassing questions about some of the little cracks that eventually turned out to be fault lines. Not only that, he was able to use our errors to turn matters upside down and find out what the true answer ought to be. It was then a matter of verifying Fred's uncanny intuition—a major task in its own right.

Fred contributed in many ways to the mathematical environment in Princeton. Our collaboration was only one part of it. Another was the departmental computer network which he helped create and certainly nurtured and to which he devoted large pieces of his time and energy. We have benefited hugely from this stable, convenient, accessible system that, as much as anything, has changed our lives in the last ten years or so.

We all have reasons to miss Fred; he profoundly enriched the field of mathematics as well as the careers of the many mathematicians whose trajectories, like mine, intersected his.

Elliott Lieb is Professor of Mathematics and Physics at Princeton University. He delivered the preceding remarks at the Memorial Service for Fred Almgren held at the Princeton University Chapel on March 15, 1997.

Joy in Everything

Jean Taylor

Fred found joy in everything. He used every possible excuse for celebrations: birthdays of course, but also especially fast times in jogging, good grades of his children, a paper of his or mine being accepted for publication, or even a paper just finished. As someone noted in a letter to me, he even found joy in his medical condition. For example, he wanted to see what the doctor saw when doing a bronchoscopy, where a tiny TV camera is inserted into the lung's bronchial tubes using fiber optics. So after the significant part of his bronchoscopy was done in Princeton, I was called in to join him and we watched the camera poke around and then be withdrawn. I've seen the inside of his lungs, and watched his vocal chords operate!

The drawer by his side of the bed is that of a six-year-old kid, not a 63-year-old man. Among its contents are a gyroscope, two magnets, four colored balls from Cheerios boxes, two magnifying glasses, a fishing lure, a Star Trek communicator button, and five pretty rocks.

Many people did not know that he was a jet fighter pilot in the Navy, or a championship pole vaulter while an undergrad at Princeton. He delighted to tell his graduate students stories about his flying days, including how he once flew his plane through the top of a tree and had a piece of wood in a wing when he landed. He joined the Navy Research Reserve after finishing his three years of active duty, and stayed in it for seventeen years. And we bought really good wine, which we called "Navy wine," with his Navy pension.

When I married him, he didn't know anything about cooking; his idea of a meal was to reheat canned spaghetti. But he took a number of cooking classes at the Princeton Adult School, and became a very good cook, as those who have eaten his baked stuffed salmon, bouillabaisse, or pasta know. He used to tell me that he asked the dumb questions in the cooking class, and he was the only one to have the nerve to try to flip pancakes (he wondered if one of his was still behind a stove in the high school...)

We also took a couple of classes together, including a massage class at the YWCA. Fred's story (as he used to say, "that's my story and I'm sticking with it") was that when he went to sign us up, the woman taking registrations said there was only one spot left, so he said, "that's OK, I just love running my hands over women's bodies". The woman promptly signed up the two of us.

Fred loved adventures, and we had a lot of them: scuba diving in Bermuda, the Caribbean, and the Great Barrier Reef; going on a camping safari in Tanzania; sailing the Spirit of Massachusetts to many winnings of the Mayor's Cup in the Gloucester Schooner Race; hiking in the mountains (and climbing up a few), kayaking—we did a lot. We were just waiting for another year to elapse after his bone marrow transplant so we could go trekking in the Himalayas.

Jean and Fred at his 25th reunion, in Princeton.

But mostly he loved his family and doing mathematics. His idea of how to spend any day, weekends included, was to do mathematics most of the day, go jogging, and then have a good dinner at home with "an interesting bottle of wine." As often as not, mathematics was the subject of the dinner table conversation, which is perhaps one reason why all three of his children are interested in mathematics. He was very patient, and stubborn; he got our daughter Karen to walk all the way up Mount Ralston when she was five, and my mother to do so when she was 67, telling stories all the way.

Fred had his bone marrow transplant in Boston in October, and on November 8 I drove him back to Princeton. Between then and the time when he reentered the hospital in January, he had to limit strictly his contact with the outside world, and I spent nearly all day with him at home. He was very tired and was in bed a lot. But it was an extraordinary thing for us to be able to drop all outside commitments and just spend time together doing nothing but talking. We became very close.

Fred had many letters and cards while he was in the hospital in October, and he appreciated them all. It moved him very much to see that other people cared. He never managed to tackle thanking people individually, partly because the task seemed so daunting, but know that it meant a great deal to him.

So if he had to die, this was the way to do it; it gave us friends and family a chance to tell him how much he meant to us, and for him to tell us how much we meant to him. I just don't know why he had to die. But since it eventually happens to all of us, if you are sitting next to someone you love or appreciate very much, and you haven't told them that lately, I would like to close by recommending that you turn to them and tell them so.

Jean Taylor was Almgren's wife, his co-author on eight papers, and before that his first Ph.D. student. She is a Professor of Mathematics at Rutgers University (taylor@math.rutgers.edu). *She delivered the preceding remarks at the Princeton Memorial Service.*

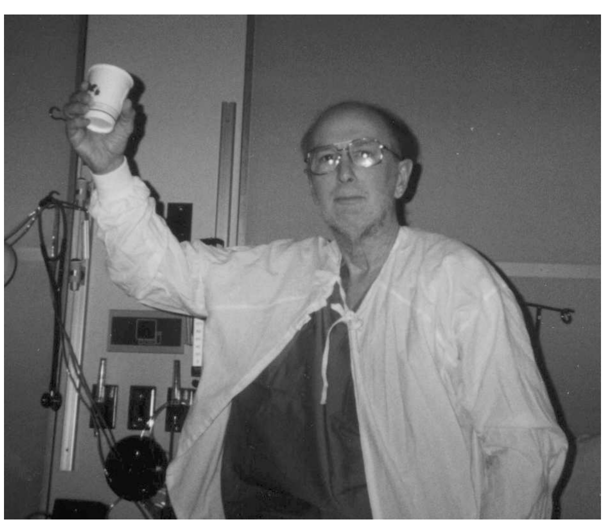
Integrate by Parts, and Other Fatherly Wisdom

Robert Almgren

If I could give you just one phrase to tell you what my father was like, I would tell you that his motto was "play to win". That was what he said last fall as he decided to undergo the transplant.

He didn't have to do it: with supportive care he could have lived with the disease for another six months or a year, which the doctors said would be "quality time". But, once he found out about this risky intensive treatment, which offered the possibility of a complete permanent cure, there was no question what he would do.

He needed all of his courage to walk into the hospital and start the process, but his confidence in his choice, and his enthusiasm for this new adventure, never left him. From his bed in the intensive care unit, he kept right on inspiring everyone around him. His nurse said that the one word she would use to describe him, throughout the whole procedure, was "delightful". When he finally left the hospital, he left as big a hole there as he does for all of us here.



At the hospital, October 1996.

A lot of fathers give life advice to their sons. My father used to tell me: "Son, when you're in doubt, when you don't know which way to turn or what to do, I want you to remember two things. First, draw a picture. Second, integrate by parts." Those two pieces of wisdom have helped me get through a lot of tough situations.

He was also known to say, at least once, "I don't put any pressure on my son to follow me in my work. He can do anything he wants to. He can be an algebraist, a topologist, a geometer..."

But the funny part is that he actually did not put pressure on his kids to follow him. He would have supported whatever we did. Even so, two of us are working in mathematics, and the third is studying it with great success and evident enthusiasm. The reason is that it was obvious to all of us how much joy our father took in his work. If not, he wouldn't have been doing it.

His rule for success in life was to put all your eggs in one basket, and watch that basket. In life, as in mathematics, he had a fantastic ability to focus on what was really important. He didn't waste any energy on things he didn't care about. But for things he did care about, he had a tremendous joyful intensity that came across immediately to everyone who met him.

A lot of times I would talk to him about decisions I was making. Often, I would describe everything I thought I needed to do, and all the constraints I was under, and eventually I would get around to saying, "Well, I know it's an absurd idea, but if I really had the choice, what I would want to do is such-and-such." He would say, "Well, it sounds to me like that's exactly what you ought to do." And it sounded so obvious once he said it.

Somehow, in talking to him you felt that he wouldn't accept anything less than you doing what you really wanted, and what you thought was really important. His principle was to take responsibility for the choices you make in your life, and not to make excuses for doing anything you didn't want to do.

He used to say, "Anything that's worth doing is worth doing badly." It took me a long time to figure out what he meant, but I think it was simply that you shouldn't be afraid of doing what you want to do, regardless of how you compare with anyone else.

He had his own theories about raising children. He thought that kids should be born in the spring, so that the first few months of their life would be spent outdoors in summer weather. For one thing, they would see the world as a warm and welcoming place, and they would have a sunny outlook on life. Also, you would save money since your children would need fewer clothes while growing fastest.

Another one of his theories was that you should deliberately teach your children to appreciate the finer things in life, so that when they grew up they would be motivated to be successful. But I think that that may have been just one more excuse for him to open up a bottle of good champagne, which he liked to do fairly frequently.

Certainly, he was not shy about enjoying all the fine things that life has to offer. I don't know anyone else who as consistently played to win: he enjoyed everything he did, and did everything he enjoyed. I hope that all of us here today can join in recognizing the great good fortune that we have shared in knowing such a man.

Rob Almgren, Fred's eldest child, is an Assistant Professor of Mathematics at the University of Chicago (almgren@math.uchicago.edu). *He delivered the preceding remarks at the Princeton Memorial Service.*

Left: Fred and his two older children, Rob and Ann, on Disappointment Peak in the Grand Tetons, in the mid 1970's. Ann is also a mathematician; she works on computational fluid dynamics at the Lawrence Berkeley Laboratory. A few years ago Ann and Fred were both members of the School of Mathematics at the Institute for Advanced Study, the first father-daughter pair of members as far as anyone knew. Right: with youngest daughter Karen, newly graduated from high school, in June 1996.

Remarks from a Fellow *Experimental Mathematics* Editor

Robert Kusner, *University of Massachusetts, Amherst*

I first met Fred Almgren during the 1984 summer workshop on Geometric Measure Theory in Arcata, CA, when I was a Berkeley graduate student. A bit earlier I had read some of Fred's seminal papers on the regularity of area-minimizing hypersurfaces and on the homotopy types of cycle spaces. Both experiences left me deeply impressed and somewhat intimidated! Thus, a few years later, while I was on a job-hunting tour, it came as a very pleasant surprise to be invited to visit Fred at his Princeton office, on one of the highest floors of Fine Hall.

Fred had me explain some of my calculations on noncompact bubbles and triply periodic foam structures, while he asked questions and two of his students observed. In hindsight, I have reinterpreted this chat as perhaps a mere prelude to my mathematical initiation: heading to the top floor of Fine for a spectacular late-winter view of the Princeton environs and for some liquid refreshment from Fred's "private reserve"!

Soon the four of us were speculating on such intriguing questions as the height of nearby structures. After a few moments trying to decide if the Graduate College tower a mile or two away was higher than Fine Hall, it was Fred who offered the following Comparison Theorem:

> *If it is higher than the horizon, it is higher than we are.*

The proof was left for us to ponder, but all earlier speculations were settled definitively as easy corollaries! (And indeed the top of the Graduate College tower is higher than Fine Hall.) A little later, I turned to Fred and, recognizing this was a useful theorem for folks who fly, asked him whether he had ever had been a pilot: he modestly replied yes, and we turned to other topics. Only many years later did I learn that Fred had flown jet fighters!

During the past decade I have gotten to know Fred and his family better, as colleagues and as friends. We had hoped to renew our friendship, as well as work on a few projects together, while I was visiting the Institute for Advanced Study this year. Alas. Yet, added to the deep sadness I feel upon Fred's passing, I can't help but think he was an extremely brave man, always teaching us something important, right up to the very end.

We can best remember Fred by trying to emulate his gentleness while also looking for the next challenging problem to solve.

The most pleasant memory I can carry forth of him is of a gentle man jogging down past my house and stopping his jog to have a discussion about what data structures he might use in the program he was going to write next.

— David Dobkin
Professor of Computer Science, Princeton University
Cofounder of the Geometry Center

On Being a Student of Almgren's

Frank Morgan

When I first met with Prof. Almgren (as he was called by his students back then) to discuss the prospect of working with him, he suggested two thesis topics and proceeded to sketch out a schedule for my finishing by the end of my third year. Finishing in three years was an old Princeton math department policy that he alone still tried to implement. At that very first meeting he gave me dates by which I should decide on my topic, have conjectures and ideas for proofs, have sketched out the proofs, have a first draft of my thesis—right up to the date for my thesis defense. I loved this. I like being very organized. And every graduate student worries about never finishing, about a thesis that drags on forever: this schedule provided a hope and vision of finishing promptly.

Almgren clearly preferred one of the two thesis topics, but I chose the other: developing a practical structure theory for unrectifiable sets (Almgren had been one of the first mathematicians to appreciate Mandelbrot's fractals). Over the next few months, in a way still mysterious to me, my chosen topic evolved into his favorite topic: putting a measure on the space of curves in $\mathbb{R}^3$ and proving that almost every curve bounds a unique area-minimizing surface.

My first assignment had been to understand Federer's structure theory for rectifiable sets. I remember getting up very early every morning of that first Christmas vacation to spend hours pouring over those little balls and cylinders and X shapes. Later I discovered that every reasonable conjecture seemed to have already been proved by Marstrand. So I guess it was not too long until I was ready to switch to trying to put a measure on the space of curves.

My first "great discovery" was a measure on all compact subsets of the unit cube, with total measure e. In time I had computed that the measure of the set of singletons was 1, of doubletons was $1/2!$,

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John Sullivan

In the late 1970s, I was in the public schools in Princeton with Fred Almgren's children, Rob and Ann. I first met Fred and his wife Jean Taylor one spring at Rob's birthday party. I remember being impressed that they were all going off to Australia for the summer—maybe this was one of my first hints of some of the attractions of academic life. In retrospect, it was, as well, my first glimpse of Fred's zest for making the most of life.

When I returned to Princeton for grad school, I thought I might work on Riemann Surfaces. But Almgren's fascinating introductory course on Geometric Measure Theory, and also the possibility of doing mathematical work on computers, soon led me to work with him. As one of the founders of the Geometry Supercomputer Project (and later the Geometry Center) in Minneapolis, Fred was one of the first mathematicians to recognize the value of computers, and especially computer graphics, in solving geometric problems in pure mathematics.

His mathematical work was always characterized by his willingness to learn new techniques. He would master and use whatever tools it took to solve a problem. So it was natural that when he found some problems amenable to computer exploration, he would embrace this approach.

Before I was even ready to pick an advisor or a thesis topic, Fred had me writing code for Voronoi diagrams in three dimensions. For several summers he brought a "Minimal Surface Team" (including Jean, Ken Brakke, and me as well as other students) to Minneapolis. There we worked on such software to study problems about soap films and related geometries.

Fred was trying to rekindle mathematical interest in Kelvin's problem of partitioning space into equal-volume cells with the least possible interface area. He hoped our programs, including Ken's Evolver, would help us discover a counter-example to Kelvin's conjectured solution. Although we were

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of tripletons was 1/3!, etc., and I was pinning my hopes on the remaining measure of 1. It turned out that the collection of sets with zero elements had measure 1, and so the set of curves had measure 0, alas. Almgren remained hopeful and encouraging. I finally found a good measure, a kind of generalization of Brownian motion to smooth curves.

In his geometric measure theory class, Almgren sometimes would teach from dry, technical, yellowed notes like Federer's text. Just when it was becoming unbearable, he would put the notes down and speak extemporaneously of the beautiful underlying geometric ideas. Every such minute was a lesson of a lifetime. It could be just an insight into a technical proof, such as why an approximately differentiable Lipschitz function must be differentiable: since the function is Lipschitz, the shrinking bases of the protruding peaks force their heights to diminish rapidly. It could be a glimpse into the origin and purpose of the subject, perhaps the question or counterexample that started it all. It could be a sharing of his struggles to prove the momentous regularity theorem for area-minimizing surfaces in general dimension and codimension. Whatever it was, it was an invaluable inspiration and education.

Almgren had us take turns teaching early topics. I remember his generous appreciation of my presentation of the Deformation Theorem—especially of my illustration, which survives as Figure 5.1.1 of *Geometric Measure Theory: A Beginner's Guide.*

Some great mathematicans seem to work by incomprehensible brilliant leaps and insights, but in Almgren I found a comprehensible definition of intelligence: facing a question head on and faithfully persisting in overcoming every obstacle. His theory of soap films, for example, unlike the earlier, more convenient classical theory of minimal surfaces, faithfully modeled physical reality, with all of its daunting complexities. He would not compromise, and he would not give up.

I cannot imagine a more attentive and encouraging advisor. He was always available, always optimistic, always appreciative of any progress ("Well,

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never successful in this search, five years later Denis Weaire and Robert Phelan made an ingenious discovery of a new and better partition, vindicating Fred's view that Kelvin could be beaten (and even making use of our software).

Those summer visits to Minneapolis gave me and the other graduate students more chance to socialize informally with Fred and Jean. I remember in particular one day when we had gone to one of the lakes to swim, and discovered someone offering free windsurfing trials. To my surprise, Fred and Jean were accomplished windsurfers. They tried to teach me too, but the winds were a bit strong for a novice.

When Fred invited a seminar speaker to Princeton, he would host a dinner party afterwards, and all his students were invited. Fred was in charge of the meal, but was a master at delegating tasks that others could handle. By watching and helping, I learned to cook spaghetti carbonara and other pasta dishes. Under his guidance, the food always turned out well.

Knowing that some visitors might be reluctant to divulge all their current mathematical projects, Fred always hoped they might relax a bit more after a couple of glasses of good wine. Lively discussions in good company were the usual result.

When I started working on my thesis project with Almgren, he said, "If I knew how to solve this problem, I'd tell you". He came up with many good suggestions along the way, but he would not have suggested a problem that he knew how to finish off. He gave me his best advice, and then left me alone to work things out. Towards the end, however, his help was essential to finish the last missing lemma.

Fred's lectures and classes were always full of wonderful geometric insights and pictures. But his writing was often in the drier style that he must have learned from Federer. My own writing style tends to be much more informal: I write in a free style, and then have to remind myself to go back in and mark off the definitions, theorems and proofs for the reader's convenience. After I turned in

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you have been working.") One time I mentioned an idea for an example of a curve in $\mathbb{R}^4$ bounding a continuum of area-minimizing surfaces. His excitement surprised me: "You've made my day." So I wrote it up, in what I thought was the most efficient way possible, in a horribly convoluted argument, and gave him my draft. A short while later I ran into him in the hall, and he invited me to sit down with him on a bench for ten minutes. In those ten minutes I learned about as much about writing papers as in the rest of my life. He asked me to imagine Federer, receiving the paper, and the response I should strive to evoke: "What an interesting theorem! How could he prove that? Let's look at the first lemma. I'll grant him that. Lemma 2? Given Lemma 1, I'll grant him that." And so on, until he believes the whole theorem.

At the beginning of my thesis work, Almgren was willing to serve as a geometric measure theory oracle, telling me what to concentrate on and advising on other concerns to "worry about that later." Then one time he challenged me on one of those latter concerns, and I responded, "But you told me to worry about that later;" and he, "Now is later." I'll never forget that moment. I knew for the first time that I was through the hardest part and that I would finish my thesis.

After my thesis defense, he took me to lunch, welcomed me to the profession, and invited me to call him Fred. He also told me that if I had my thesis ready for publication in time, he would take me that summer to the geometric measure theory center in Trento, a beautiful spot in the Italian subalps, most famous for the Council of Trent. It was my first trip to Europe, and my most memorable.

So I remember the classes, the famous spaghetti dinners at his house, encounters at meetings, seminars, hikes, but most of all I remember Fred as I always thought of him, working in his office, ever available, ever eager to help.

Frank Morgan was one of Almgren's earlier Ph.D. students, having obtained his degree in 1977. He is now at Williams College (Frank.Morgan@williams.edu).

the final draft of my thesis, when I next saw Fred his first comment was "I've never seen anything quite like it." I guess at first glance it didn't look formal enough, but in the end he was happy with it. I will always remember what he said to me a couple of months later when I had just passed my oral defense: "Goodbye as a student, and welcome as a colleague."

Fred's advice on coauthorship was that it is always better to err on the side of generosity: this does nobody harm, and leads to rewarding and productive collaborations. Fred was a good source of advice about life as well as about mathematics.

John Sullivan concluded his Ph.D. in 1990. After working at the Geometry Center and the University of Minnesota, he is now at the University of Illinois at Urbana–Champaign (sullivan@math.uiuc.edu).

With Frank Morgan (foreground) and Christophe Margarin (middle) at the workshop on Elliptic and Parabolic Methods in Geometry (Geometry Center, 1994).

Fred Almgren and the Geometry Center

Albert Marden, *Geometry Center Founding Director*

"There are so many more intriguing and important things to do when at the Center than there are the hours in which to do them."

Thus wrote Fred Almgren about his visits to the Geometry Center. He was one of the founding members of the Geometry Supercomputer Project (the Center's predecessor) and contributed enormously to its success. The Minimal Surface Team that he assembled thrived over a period of almost a decade, spending several weeks in Minneapolis each summer, using the Center's facilities and interacting with its staff to create some of the most innovative experimental and theoretical work in optimal geometries — the name of the field that studies the energy-minimizing evolution of interfaces, and seeks to model such diverse phenomena as soap bubbles and crystal growth.

* * *

It all started in late 1985, when a remarkable group of mathematicians and computer scientists in several areas of geometry met at the University of Minnesota to work on a formal proposal, to be submitted to the National Science Foundation, for the funding of a project centered around *geometric visualization*. Our goal was to use visualization both as tool for experimentation, exploration, and inspiration in research, and as a vehicle for bringing mathematical ideas to students of all ages and the general public.

One motivation for our effort was the desire to get something of mathematical significance out of the Cray 2 computer, which was about to arrive on our campus. In my own areas of reasearch, I became aware of some exciting computational challanges that seemed worthy of a Cray, and conversations with other mathematicians strenghtened my belief that, in spite of the relatively primitive state of the art, it was possible to use computation and visualization to obtain meaningful mathematical advances and insight.

With the help and advice of others, I brought together the *Geometry Computing Group*, which besides myself included Fred Almgren, Jim Cannon, David Dobkin, Adrian Douady, David Epstein, John Hubbard, Benoît Mandelbrot, David Mumford, Bob Tarjan, Bill Thurston, and Allan Wilks. These names represented a broad range of interrelated specialities and were strongly attracted by the opportunity of working together; they also had keen interest in exploring the new computational tools. Fred, in particular, had not been using computers systematically to pursue his research, but he understood clearly the potential of the idea and supported it wholeheartedly.

After a long period writing grants, gathering support, and waiting, our Geometry Supercomputer Project began operating in the fall of 1987. We had been awarded an NSF grant for three years. In 1990 we successfully applied for an expansion; the Project became the Geometry Center, an NSF Science and Technology Center. The Geometry Computing Group was enlarged to eighteen people, and the Center's work spread much further: hundreds of visitors came for weeks or months to teach and learn. We had built an incubator for mathematical ideas and talent. In all of this, Fred played a large role: not only was he closely involved in planning, but the results of the Minimal Surface Team formed a substantial part of the Center research output.

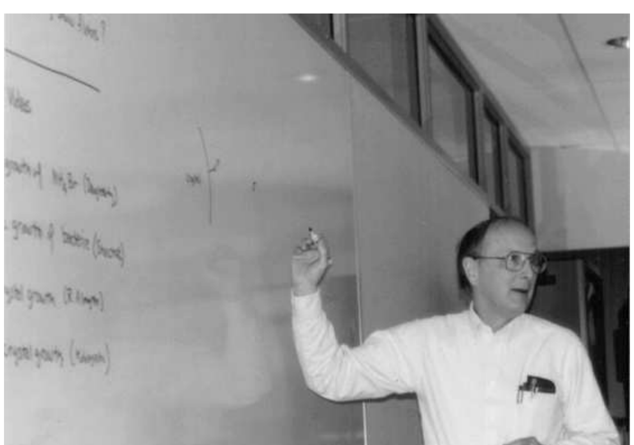
A confluence of untoward events resulted in a change of leadership in early 1994. The active involvement of the founders gradually ended, and the NSF has announced that it will phase out its involvement by mid-1998.

* * *

The Minimal Surface Team is an excellent example of how mathematics research benefits from group interaction. Fred's initiative had a profound effect on the Center and served as a model of other activities. Fred's ability to organize a team, focus its efforts, and persevere until the goal was attained was unmatched.

Each year the team would meet and work intensively, assisted by Center staff. The "permanent" participants, besides Fred and his wife and colleague Jean Taylor, were Robert Almgren, Ken Brakke, and John Sullivan; at various times they were joined by Nelson Max, Andrew Roosen, John Steinke, Andrea Sufke, and others.

Brakke's Surface Evolver (*Experimental Mathematics* **1**, 141–165) was developed over this period, largely at the Center, and became the centerpiece of the Team's extensive software development efforts. It is still the only publicly available program of its kind. It has been used to model a great variety of applications: spacecraft fuel tanks, foam rheology, liquid solders, knot energies, capillary surfaces (including some experiments flown on the Space Shuttle), cell membranes, and sphere eversion.



Informal presentation at a 1992 Geometry Center Faculty meeting devoted to exchange of ideas on open research questions.

This software owes its existence to Fred's vision. Brakke writes: "When I first heard of the Geometry Supercomputer Project, I wanted to be a part of it, and I applied with a proposal for the Evolver. Fred read the proposal and invited me to join his Minimal Surface Team. ... The idea for the program had been in my mind for years, but I probably never would have started it on my own [for lack of computational resources]. Even if I had, it would probably not have gone beyond the toy stage if it had not been used by other people. ... Without our group being together physically, there would have been no seed group of users to start the spread of the Evolver."

* * *

Education and outreach were an important aspect of the Center, and no one was better at it than Fred. His enthusiasm and clarity of presentation quickly brought national attention the the Team's work. I cannot forget his riveting blue eyes (some called it the "Almgren stare"). Fred and Jean pioneered in the use of videos to illustrate their talks. They also faced with equanimity the frustrations involved in getting their computer output properly recorded with what would now be regarded as primitive video equipment.

Reporting on their 1989 invited presentation to NSF director Erich Bloch, Fred and Jean wrote: "We first showed the most current version of the minimal surface team video. Jean then talked for about five minutes, and Fred talked for about five minutes while also showing about a minute of crystal growth video (two crystals with different surface energies growing side by side). Erich Bloch asked: Is this sort of computer work being accepted by the mathematics community? Our answer was a qualified yes. ... Judy Sunley [Director of the Mathematics Division] asked for a copy of the video. She said she wanted to be able to show it to visitors, including, in particular, students."