Jerry Tunnell (1950–2022)

Joe Buhler, Alex Kontorovich, and Stephen D. Miller

Figure 1. Jerry in Paris in 1980.

Jerrold (Jerry) Tunnell was born in Dallas, Texas, in 1950. His family moved to New Mexico when he was very young, and he attended high school in Farmington, NM. His interest in mathematics led him to choose to do his undergraduate work at Harvey Mudd College. His peers noticed (as described below by Ambassador Richard Jones[1]) that he tested out of some lower-level mathematics classes, and they soon learned to ask Jerry for help on their math homework. Jerry then went to Harvard University for graduate studies, starting in 1972 and finishing with a PhD in 1977 under John Tate. After six years in Princeton (at the Institute for Advanced Study and Princeton University) he joined the faculty at Rutgers University, where he remained until his untimely death in a bicycle accident in April, 2022.

Jerry's primary research area was algebraic number theory, much of it related in one way or another to Robert Langlands's seminal ideas and conjectures connecting algebraic number theory and automorphic representations. As with other overarching "one-to-one correspondences" in mathematics, the beauty of the ideas can be stunning, and knowledge on either side often illuminates the other.

After Jerry started his graduate work in the early 1970s many of Tate's students began to work on aspects of the Langlands program. One interesting case was a conjectural bijection between certain modular forms f of weight 1 and suitable two-dimensional Galois representations ρ : $\mathrm{Gal}(\bar{\mathbf{Q}}/\mathbf{Q}) \to \mathbf{GL}(2, \mathbf{C})$. P. Deligne and J-P. Serre had just written a paper that proved that if the modular form f existed, then there was a corresponding Galois representation ρ. If ρ was constructed from one-dimensional representations (by taking a direct sum or inducing), it was known how to find the modular form f. But no one had any idea about how to do this in the genuinely two-dimensional tetrahedral, octahedral, or icosahedral cases.[2] Some of Tate's students were given the rather elaborate "exercise" of finding f for some specific tetrahedral and octahedral ρ. Jerry, and three other students of Tate (Dan Flath, Bob Kottwitz, and Jim Weissinger) were able to do a few examples by starting with a quartic polynomial, finding a Galois extension of $\mathbf{Q}$ whose Galois group G was a subgroup of $\mathbf{GL}(2, \mathbf{C})$ (equipped with the tautological representation ρ), and using this to find out enough about the putative modular form f to prove its existence. One pleasant corollary was that this proved Artin's conjecture for such ρ, i.e., that the corresponding L-series, $L(\rho, s)$, was a holomorphic function of s. This vivid experience was a strong motivation for Jerry's thesis work, which proved a local version of the Langlands correspondence for (many) local fields F, relating representations of Weil–Deligne groups of F to representations of $\mathbf{GL}(2, F)$. This was an impressive piece of work, e.g., requiring a careful examination of supercuspidal representations, and the results were used in subsequent research by other mathematicians.

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[1] *A classmate, roommate, and lifelong friend of Jerry's.*

[2] *For definitions and details, see Guy Henniart's contribution.*

Jerry was outgoing and socially active, joining in whatever activities were popular with the graduate students at the time (playing table tennis in the common room, learning to juggle, riding a bicycle to Cape Cod for a weekend excursion, etc.). One of his notable traits was that he enjoyed debate. This was not antagonistic or confrontational in the slightest, but instead reflected a real joy in (a) discovering disagreement, and (b) attempting to get at the truth via something like the Socratic method. His goal was not so much to win as to enjoy the interchange and see where it led. This was surely useful for his teaching, and the occasional tenacity (a.k.a. stubbornness) that he displayed was surely useful for his research.

One of the first applications of Jerry's thesis was in his own work. While finishing his thesis, he realized that he could extend Langlands's proof of the "Deligne–Serre" converse in the tetrahedral (and some octahedral) cases to all octahedral cases. This required using his thesis, Langlands's "base change" for automorphic representations, and results of H. Jacquet, I. Piatetski-Shapiro, and J. Shalika. This result became known as the Langlands–Tunnell theorem.

Jerry's thesis and the Langlands–Tunnell theorem marked the beginning of a decade of major contributions to mathematics.

One of his seminal papers looked at the ancient problem of finding "congruent numbers." A positive integer n is a congruent number if it is the area of a right triangle whose sides are rational numbers; this turns out to be equivalent to saying that the elliptic curve $y^2 = x^3 - n^2 x$ has infinitely many rational solutions. Surprisingly, this question is also related to modular forms with weight 3/2 and this led Jerry to an efficient algorithm which determines, in many cases, whether or not n is a congruent number.[3] This idea was pursued in several directions by many others in subsequent years.

Another major paper resulted from a collaboration with Jonathan Rogawski that showed that much of the Deligne–Serre theorem could be extended to any totally real ground field.

Throughout his career, Jerry would visit New Mexico regularly. Some of his most intense nonacademic interests, not very well known even to his academic and East Coast friends and colleagues, were rooted in his long and deep connections to New Mexico and its history. This led him to acquire art and artifacts from Navajo (and other Native American) and Spanish sources. About 15 years ago, Jerry and his wife Marlene purchased an abandoned church in a small town in New Mexico, with the intent of remodeling it and the adjacent parsonage. They spent many of their summers there, during which Jerry was able to spend even more time on his hobbies; Marlene is quite sure some items in his truly extensive collection were snuck into their house with the connivance of their son, Matt.

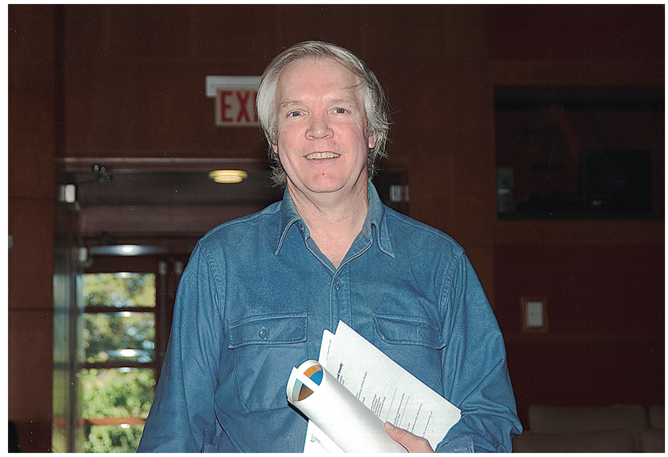

Figure 2. Jerry at IAS, Princeton.

Jerry developed a real expertise in, and understanding of, these artifacts, and he became well known to artists, collectors, and museum directors. One of the jewelry makers that Jerry had "discovered" at an outdoor market learned only recently that Jerry had died in a bicycle accident. On hearing this, he burst into tears, saying that he owed his recognition and success to Jerry for having promoted his work to galleries all over the state.

Jerry's publication record later in his career does not accurately reflect his continued interest in learning and researching new mathematics, nor his impact on students and colleagues at Rutgers. He was a dedicated mentor and teacher who took the time to work closely with his students, providing guidance and support throughout their studies. He also participated actively in seminars, and was often the person who would ask the most penetrating question at the end of a talk. Jerry attended and hosted dinners, parties, and other events where he could connect with students on a personal level and foster a sense of community among them. And, of course, he rarely shied away from amiable debates with students or colleagues.

One the most vivid applications of Jerry's work was its role in Andrew Wiles's proof of Fermat's last theorem, arguably the most famous proof of the twentieth century.[4] Wiles, with Richard Taylor, showed that certain semistable elliptic curves were modular, which implied that no nontrivial integral solutions to $x^n + y^n = z^n$ existed for $n > 2$. At a certain crucial juncture, the Langlands–Tunnell theorem was a key ingredient in the argument.

[3] *See David Rohrlich's contribution.*

[4] *See Guy Henniart's contribution.*

David Rohrlich

Although I had been out of touch with Jerry Tunnell for many years, the news of his death was a brutal shock. In an earlier period of my life I had seen him frequently, first when he was finishing up his thesis at Harvard and I was a postdoc there, then when he was an assistant professor at Princeton and I was a faculty member at Rutgers, and later when he joined me at Rutgers, where we were colleagues for several years. He had an imperturbable manner and a tendency toward understatement, and he radiated a calm self-confidence without coming across as conceited. He was not one to divulge personal information gratuitously, and we never became close friends, but he was always fun to be with, and once you became involved in a conversation with him, he was an engaging interlocutor, freely revealing his knowledge and experiences and opinions in realms that you had never heard him talk about before. He was truly a man of the world, the epitome of sophistication without ostentation.

Jerry's list of mathematical publications is short but studded with diamonds. Of particular note are his work on the local Langlands conjecture for GL(2) [Tun78], the octahedral case of the Artin conjecture [Tun81], the Deligne–Serre theorem for Hilbert modular forms (joint with Rogawski) [RT83], and the connection between the congruent number problem and half-integal weight modular forms [Tun83]. In this last paper, Jerry managed to do something that very few mathematicians can ever hope to do: He used the tools of contemporary number theory and automorphic forms to make a major advance toward the solution of a problem that had remained open for a thousand years.

Let us call a right triangle *rational* if the length of each of its three sides is a rational number. Of course the area of such a triangle is also a rational number, and the congruent number problem asks for a characterization of the rational numbers that arise in this way. Since the set of rational right triangles is closed under scaling by rational numbers, one may assume that the three sides of the triangle have lengths x^2-1, $2x$, and x^2+1 for some rational number $x>1$, whence the area is x^3-x. Write this area in the form dy^2, where d is a square-free integer and y is rational. Then d is called a "congruent number," and the problem is to determine the set of positive square-free integers so obtained. Equivalently (after an argument to remove the condition $x>1$), the problem is to characterize the set of positive square-free integers d for which there exists a rational point (x,y) on the elliptic curve $E_d : dy^2 = x^3-x$ with $y \neq 0$. Such a point is necessarily a point of infinite order, so the conjecture of Birch and Swinnerton-Dyer predicts that d is a congruent number if and only if the L-function $L(E_d,s)$ vanishes at $s=1$. Now $L(E_d,s)$ is just $L(E_1,\chi_d,s)$, the twist of $L(E_1,s)$ by the quadratic character χ_d corresponding to d. It follows that the vanishing and nonvanishing of $L(E_d,s)$ as d varies is controlled by a modular form of weight 3/2—a modular form which corresponds via the work of Shimura and Waldspurger to the cusp form of weight 2 underlying E_1. Identifying the relevant form of weight 3/2, Jerry was able to give a necessary condition for d to be a congruent number, a condition which is also sufficient if one grants the conjecture of Birch and Swinnerton-Dyer.

It is worth pointing out that "publishing" means "making public, disseminating," and quite apart from his published journal articles, Jerry was good at disseminating mathematical ideas and information in the midst of conversations. I certainly learned a lot from conversations with him, and I imagine others did as well. A case in point: my first encounter with the notion that there should be a version of the conjecture of Birch and Swinnerton-Dyer for twists of L-functions of elliptic curves by Artin representations was a casual remark made by Jerry at a seminar dinner in Princeton around 1980. The conversations that led to our only joint paper [RT97] are perhaps in a different category, since they were one-on-one and probably quite focused from the start, but for the record, the initiative to discuss Serre's conjecture and then to think about the case $p=2$ was entirely Jerry's. The resulting paper appeared long after it was written, and it is only thanks to Dinakar Ramakrishnan's urging that it appeared at all. But the amusing point to emphasize here is how Jerry characterized our joint project: He cited the British novelist Graham Greene, who distinguished between his serious works and his "entertainments." Jerry proposed [RT97] as an "entertainment," a characterization which I find both delightful and very apt, given the sharp contrast with Jerry's more serious works.

Jerry figures in many of my memories of mathematical gatherings from long ago, and often an amusing or enlightening comment that Jerry made at an event is one of the things that I remember best about it. Most of the people in those memories are still alive, and it is devastating that Jerry is not. He left us much too soon.

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Figure 3. Jerry and Andrew Wiles.

Guy Henniart

In 1981 Jerry published a striking three-page research announcement [Tun81], with proofs, entitled *Artin's conjecture for representations of octahedral type*. The wonderfully concise introduction was:

> Let L/F be a finite Galois extension of number fields. E. Artin conjectured that the L-series of a non-trivial irreducible representation of $\mathrm{Gal}(L/F)$ is entire, and proved this for monomial representations. The non-monomial two-dimensional representations are those whose image in $\mathbf{PGL}(2, \mathbf{C})$ is isomorphic to the group of rigid motions of the tetrahedron, octahedron or icosahedron...Langlands proved Artin's conjecture for all representations of tetrahedral type, and certain octahedral representations when $F = \mathbf{Q}$. The purpose of this note is to prove the conjecture for all octahedral representations by using the methods of Langlands and an analytic result of Jacquet, Piatetskii-Shapiro and Shalika.

Let us explain this a bit more.

The L-series $L(\rho, s)$ of a Galois representation

$$\rho : \mathrm{Gal}(L/F) \to \mathbf{GL}(n, \mathbf{C})$$

of dimension n is a function of a complex variable s which generalizes classical examples of the Riemann zeta function, Dirichlet L-series, and the Dedekind zeta function of a number field. The L-series is initially defined for $\mathbf{Re}(s) > 1$ by an Euler product, but Artin proved that it has a meromorphic continuation to the whole complex plane, and has a nice functional equation relating $L(\rho, s)$ and $L(\rho', 1 - s)$ where ρ' is the dual representation. When $n = 1$ and ρ is nontrivial, the holomorphy of $L(\rho, s)$ follows from class field theory. If ρ is *monomial*, in the sense of being induced from a one-dimensional representation, then the induced representation has the same L-function as the original one, and Artin's conjecture is true. Beyond these cases, the holomorphy of $L(\rho, s)$ for non-monomial and irreducible ρ was a mystery.

In a letter to Weil dated 1967, Langlands proposed, among other things, a very strong heuristic reason for the Artin conjecture: $L(\rho, s)$ should be **equal** to the L-series $L(\pi, s)$ of a cuspidal automorphic representation π of $\mathbf{GL}(n)$ over F, and the L-series $L(\pi, s)$ of such an automorphic representation is known to be entire. In the monomial case, an idele class character is tantamount to an automorphic representation for $\mathbf{GL}(1)$ and H. Jacquet and Langlands (following Hecke) produced such a π for $n = 2$ and monomial ρ. The general conjecture of Langlands came to be known as the strong Artin conjecture.

Irreducible non-monomial ρ of dimension 2 have image in $\mathbf{PGL}(2, \mathbf{C})$ isomorphic to a permutation group that is also the symmetry group of a regular polyhedron: A_4 (tetrahedral), S_4 (octahedral), or A_5 (icosahedral). The icosahedral case seemed out of reach (and in fact, the case of odd Galois representations over $\mathbf{Q}$ wasn't resolved until 30 years later), but the cases with solvable galois groups seemed approachable. Indeed, in those cases the Galois group has a subgroup of index 3 corresponding to a cubic extension K of the ground field F, and the restriction of ρ to K becomes monomial, so there is a unique cuspidal automorphic representation Π of $\mathbf{GL}(2)$ over K with $L(\Pi, s) = L(\rho_K, s)$.

Thus one looks for a process—called base change—associating to an automorphic representation Π of $\mathbf{GL}(2)$ over F an automorphic representation π_K of $\mathbf{GL}(2)$ over K, which would correspond to restricting ρ to $\mathrm{Gal}(L/K)$. It was hoped that this would allow π to be constructed from Π.

At a conference in Michigan in 1975, H. Saito and T. Shintani proposed such a construction for cyclic F'/F, using a so-called twisted trace formula for $\mathbf{GL}(2)$. Langlands then "caught fire" (in Jacquet's words) and soon a preprint *Base change for* $\mathbf{GL}(2)$ appeared. For a cyclic extension F' of F, the base change π'_F of an automorphic representation π of $\mathbf{GL}(2)$ over F is an automorphic representation of $\mathbf{GL}(2)$ over F' which is invariant under the natural action of the cyclic group $\Gamma = \mathrm{Gal}(F'/F)$. Any Γ-invariant cuspidal automorphic representation π of $\mathbf{GL}(2)$ over F' is a base change of some cuspidal π, and the other possibilities are the twists of π by idele class characters corresponding to characters of Γ. The proof of Artin's conjecture for tetrahedral ρ follows from this and a clever construction of S. Gelbart and Jacquet.

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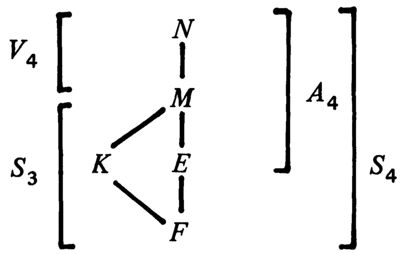

Figure 4. Field diagram for the octahedral case.

For octahedral ρ over an arbitrary number field F, one can start with the tetrahedral representation obtained by restricting Π to the quadratic field E over F. However, the techniques used in the tetrahedral case are not sufficient to determine a corresponding representation over F.

Jerry solved this puzzle by using both the cubic extension K/F (which is not Galois in the octahedral case) and the quadratic extension E/F. He used work of H. Jacquet, I. Piatetskii-Shapiro, and J. Shalika that had shown how to construct a base change associating to an automorphic representation τ of $\mathbf{GL}(2)$ over F an automorphic representation τ_K of $\mathbf{GL}(2)$ over the non-Galois cubic field K. They did not use the techniques of Gelbart and Jacquet, but rather analytic properties of the so-called Rankin-Selberg L-series.

Jerry's crucial "lemma" is that there is only one choice of $\pi = \pi_1$ or π_2 whose base change to K is the automorphic representation corresponding to ρ_K. His next main theorem is that $L(\pi, s) = L(\rho, s)$, requiring the the Euler factors in the definitions be compared term by term. So $L(\rho, s)$ is indeed entire! The idea is elegant, and the details are in Jerry's very readable note.

It is still not known how to go significantly beyond dimensions 2 or 3 for Artin's conjecture using only automorphic techniques.

Another conjecture which can be viewed as part of the broad Langlands program, but had independent and earlier origins variously attributed to G. Shimura, Y. Taniyama, and A. Weil, is often called the modularity conjecture. Roughly, it asserts that elliptic curves over the rational numbers are in bijection with certain modular forms of weight 2.

In the 1980s, it became gradually clear, through the influence of Serre and Mazur, that it might be useful to approach modularity by looking at modular representations of Galois groups (i.e., with coefficients in finite fields). The idea was to associate automorphic representations to their deformations to characteristic 0 (an ℓ-adic representation rather than a complex one). The goal was then to apply this to the modularity conjecture.

In 1985, Frey, using a curve of Hellegouarch, showed that if there was a nontrivial solution to the equation

Figure 5. Karl Rudnick on the left and Jerry on the right, in Texas in 2022.

$a^p + b^p = c^p$, for $p > 3$, then modularity was likely to be false for a specific elliptic curve, built out of a, b, c. In 1985 J-P. Serre showed that modularity would indeed be false in that context if a certain precise statement about modular forms, arising out of his general conjectures on modular Galois representations, was true. This so-called "epsilon-conjecture" was proved in 1986 by K. Ribet. All of a sudden, modularity implied Fermat's last theorem!

Much of this emerged at, or near the time of, a year-long program at MSRI in algebraic number theory. This was a hot topic for the participants, of which Jerry was one. The recent successes of Langlands, Tunnell, and others were in the back of people's minds, and at first this made this attack on modularity seem promising. People knew that one had to start with modular representations for which at least one deformation had an associated automorphic representation. The group $\mathbf{PGL}(2, \mathbf{F}_2)$ is S_3, but this isn't usable in this context. However, $\mathbf{PGL}(2, \mathbf{F}_3)$ is isomorphic to S_4, and Langlands–Tunnell implies that for suitable Galois representations into $\mathbf{GL}(2, \mathbf{F}_3)$, one can indeed get an associated automorphic representation. This seemed to be exciting for a while, but no one got anywhere, and many began to feel that modularity was still far off in the future.

Fortunately, Andrew Wiles did not share that opinion! It took seven years of his hard work (and help from Richard Taylor), to prove modularity for the needed curves. One of the curious features of that proof was that at one point it required a delicate dance between the primes $p = 3$ and 5. By a strange twist of fate, the key fact needed for $p = 3$ was . . . the Langlands–Tunnell theorem.

Richard Jones

The squeal of brakes split the air followed by a loud "whomp" and then silence. It was day 15 of our planned cross-country bike trip from the Atlantic coast at Saint Augustine, Florida, to our alma mater in Claremont, California, where we planned to celebrate the 50th anniversary of our graduation from Harvey Mudd College. After our lunch stop, the five riders in our jaunt had become stretched out along our planned route for the afternoon. Jerry Tunnell, my longtime friend and former college roommate, was in the lead. I was trailing him by 100 yards or so and the other group members were somewhere behind me.

I quickly focused my attention on the road in front of me and was astonished to see a semi-truck trailer jackknifed across both lanes of traffic stopped a few yards in front of me. I immediately slammed on my brakes thinking that the truck must have somehow collided with a vehicle in the oncoming lane.

Wondering if Jerry had seen the accident, I dismounted and hurried around the truck to see the collision.

However, I was surprised not to see any signs of another vehicle.

As I pondered this the driver of the semi appeared on the tarmac next to me.

"I saw it," she exclaimed, pointing down the road. "That other truck hit that guy."

I immediately focused down the highway and for the first time saw Jerry. He was lying still on the tarmac a few hundred feet away.

As I hurried to him, I was heartened to see that he was lying on his side, as if resting, and that there were almost no visible signs of injuries, just a few scratches on his limbs. His bike helmet was still on and looked fully intact. This raised my hopes that perhaps he was only shaken up.

However, when I reached him he was unconscious. After calling 911, I knelt next to him and tried to rouse him. I began rubbing his back and calling his name. Another person soon joined me and began searching for a pulse while also saying his name. After a few minutes, however, the other man shook his head and stood up. Slowly it began to dawn on me that my earlier optimism had been misplaced. By the time the other members of our group began arriving on the scene, I knew that it didn't look good for Jerry.

Richard Jones had a long and distinguished career at the State Department, serving as ambassador to Israel, Kuwait, Lebanon, and Kazakhstan, and held a variety of positions (including deputy executive director of the International Energy Agency) under Presidents Clinton, Bush, and Obama.

An EMT soon arrived and began assessing Jerry's condition. After a few minutes, I was disheartened, but not surprised, to hear him calling the hospital and saying that Jerry was a "probable DOA." A short time later an ambulance arrived, but all that they could do was confirm the EMT's initial assessment with an EEG. I touched Jerry one more time, but this time his body was cold. I knew that our friend and colleague for more than 50 years, the inspiration for our cross-country expedition, was dead.

In the days and now weeks since that tragic afternoon, I have struggled to come to terms with Jerry's loss.

I have found that the best way to short circuit these bouts of self flagellation is to recall the good times Jerry and I had together over the years starting with our undergraduate careers at Harvey Mudd College (HMC) where we met on the first day of the Claremont Colleges' orientation week for new students in September 1968. We quickly found that we had something in common. Jerry was from a small town in New Mexico, and I had an older brother working in another small town in New Mexico. This was the beginning of our friendship that lasted more than 50 years.

One night during the orientation week Jerry and I went down to a mixer event at one of the other Claremont colleges with some other HMC freshmen. Jerry and another HMCer were energetically discussing a problem on a test they had taken that day to skip first semester freshman calculus. Jerry was sure of his solution and laid it out on a napkin for the other student who had not been able to solve it.

Evidently, Jerry's solution was correct; he passed the test. His reward was to be able to take an introductory course on complex variables a year earlier than most of the rest of our class, including me. Considering that Jerry's small high school in New Mexico had not offered a calculus course and he had studied it on his own over the summer, this was an impressive achievement.

As that first year progressed students' reputations gradually began to be solidified based on their performance in various areas. (Freshmen at HMC all took the same curriculum. The possibility of replacing first semester calculus with complex variables being the notable exception.) Jerry gained a reputation for being a good problem solver whose solutions were invariably correct. Other students began coming to him for help on their homework and he seemed to enjoy explaining the material to them, although once he complained to me that he had a hard time empathizing with those who could not understand points that were obvious to him.

Once, in response to my question as to why he was so good at problem solving, Jerry directed my attention to a small book on his bookshelf. It was *How to Solve It* by

G. Polya, which he evidently had acquired in high school, or perhaps when he had attended an NSF-sponsored summer program in chemistry. Among the many lessons in Polya's book is that success in solving one challenging problem builds confidence and leads to more problem-solving success. Another is that to really understand a subject area students should create and solve their own problems.

Jerry certainly took these nuggets to heart. He returned from summer vacation for sophomore year having completely worked through the more than 700 pages of Kreider, Kuller, Ostberg, and Perkins's *An Introduction to Linear Analysis*. This allowed him to skip another course and get further ahead of the rest of us. At some point that year, he proudly displayed his understanding of boundary value problems to me by devising (and solving) a problem involving the heating of a pepperoni pizza, our snack of choice for watching late night TV. His growing devotion to the study of mathematics was also evinced by his habit of bringing whatever math text he was reading with him when invited to go to the movies or some other social event, just in case things got boring.

Although we roomed with each other starting in sophomore year, we didn't actually spend much time discussing math, since we shared only one math class while at HMC. I rarely needed his help with my problems and he never needed my help with his! In fact, our academic discussions focused on Russian (which we took together for three years), or on some of the common core classes.

After graduation, we went our separate ways for graduate study. He started at Harvard in the fall of 1972, and after a gap year I entered a PhD program at UW Madison in 1973. Despite the lack of e-mail in those days we managed to stay in touch. In summer 1974, I finished a cross-country bike trip in Boston and Jerry rode his bike out from Somerville, where he was living, to meet me on the last day of my ride and guide me to his apartment. Although we had frequently ridden bikes together to and from Russian class, this was the first time that I rode with Jerry on the open road for recreation. I was pleasantly surprised that he had no difficulties in keeping pace with me on the ride into Boston.

Jerry also came to visit me and my wife in Madison on more than one occasion in the years that followed. A few years later, he visited us in northern Virginia by bike from Princeton. At that point I was already working at the US State Department. Later, when my wife and I were posted to Paris in the early 1980s, he came more than once to visit us during our three years there. On one of these visits I remember asking him about his proof of a 2000-year-old conjecture. [See David Rohrlich's contribution.] However, he said he really didn't want to discuss it, explaining that every time he was invited to a conference or to visit another mathematician he was always asked to talk about this proof and he was just sick of it. He found his subsequent work much more interesting but nobody wanted to hear about it!

After Paris we were transferred to Tunis in North Africa and, recently married, Jerry and Marlene visited us there, where we thoroughly enjoyed getting lost together on a desert road trip when our newly published map directed us to a road that had yet to be completed! Such good times continued almost annually, usually either in New Mexico or in New Jersey, for the next 40 years. During this period our friendship grew even deeper as we matured and faced life's many challenges of career and family.

Jerry was always good-natured and affable, even though he could be stubborn when he wanted to be. Sometimes I would badger him about not retiring or attending HMC's quinquennial class reunions. He quipped that he would work until he died as it was easy for him, and he was good at teaching. He promised that he would attend a reunion but not until the 50th, and it had to be by bicycle from the East Coast. This was the genesis of our ill-fated cross-country trip.

He was probably my closest friend in college, as close as a brother. He was exasperating at times, but I loved him all the same and knew we would always be close. The world is poorer for his departure from it. He will certainly be missed as a great mathematician, but he will be missed more by those of us who knew him as a true friend.

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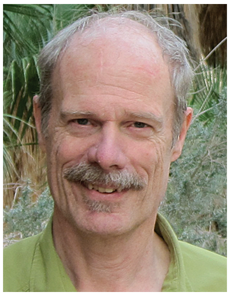

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