

Remembering Atle Selberg, 1917–2007

*Dennis Hejhal, Coordinating Editor**

On August 6, 2007, Atle Selberg, one of the pre-eminent mathematicians of the twentieth century, passed away at his home in Princeton, NJ, at the age of ninety.

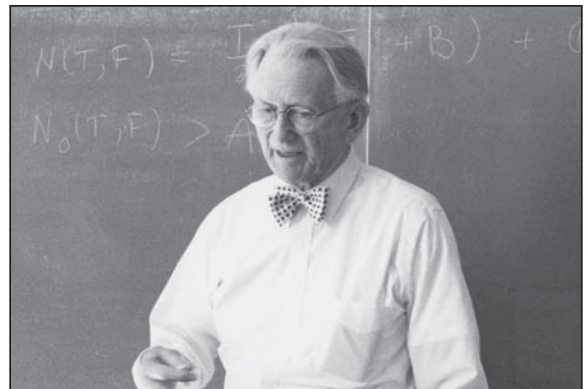
Born in Langesund, Norway, on June 14, 1917, Atle Selberg was the youngest of nine children in an academic family (his father held a doctorate in mathematics, and two of his brothers, Henrik and Sigmund, also became mathematics professors). He grew up near Bergen and then studied at the University of Oslo, earning his doctorate there in October 1943, a few weeks prior to the university being closed by German military authorities. Following a five-year research fellowship and encouraged by Carl L. Siegel, in 1947 (the newly married) Selberg moved to the U.S. and the Institute for Advanced Study in Princeton, where he was a member for one year. After spending the 1948–49 academic year at Syracuse as associate professor, Selberg returned to IAS as a permanent member and in 1951 was promoted to professor. He retired from IAS in 1987 but remained mathematically active for at least another decade.

Noted for his profound contributions to number theory, discrete groups, and automorphic forms, Selberg was honored with a Fields Medal in 1950, a Wolf Prize in 1986, and a special Abel Bicentennial Anniversary Prize in 2002.

The impact of Selberg's work can be seen from some of the many mathematical terms that bear his name: the Selberg trace formula, the Selberg sieve, the Selberg integral, the Selberg eigenvalue conjecture, and the Selberg zeta function.

Dennis Hejhal, coordinating editor for this article, is professor of mathematics at the University of Minnesota and Uppsala University, Sweden. His email address is hejhal@math.umn.edu.

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Atle Selberg at Columbia University, 1998.

During the course of his career—a career spanning more than six decades—he was variously a masterful problem solver, a creator of powerful and lasting tools, and a gifted theory builder. Depth, elegance, and simplicity of method were the hallmark of Selberg's style.

More detailed recent accounts of Selberg's life and work can be found in [3–6].

Curiously, in a 1989 interview [2], after first reminiscing how

the things that Siegel tended to do were usually things that seemed impossible. Also, after they were done, they seemed still almost impossible...

and then describing the daunting effect that Siegel's lectures sometimes had on younger people at IAS, Selberg went on to say:

Well, I knew of course that I could do *other* things. I couldn't do the kind of things that he did. I think the things I have done, really all of them, are things that—although sometimes there were technical details, and sometimes even a lot of calculation, in some of my

early work—the basic ideas were rather simple always, and could be explained in rather simple terms. I think in some ways, I probably have a rather simplistic mind, so that these are the only kind of ideas I can work with. I don't think that other people have had grave difficulties understanding my work.

There was a certain understated (distinctively Scandinavian) quality in Selberg that could be quite inspiring, particularly for younger mathematicians.

Those who got to know him soon came to realize something else. Blessed with an impressive memory, Selberg was generally able—even during his seventies and beyond—to reconstruct with minimal need for “any faded old notes” the essential details of proofs that he had worked out years, if not decades, earlier.¹ There was an ongoing vitality not only in Selberg's ideas but also in the man.

What follows are some recollections of Atle Selberg by mathematicians from a number of generations. Together with the interview in [1], it is hoped that they can give the reader a glimpse of the remarkable person that Atle Selberg truly was.

References

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- [2] B. DEVINE, Interview with Atle Selberg, unpublished, IAS Archives, 1989, 57 pp.
- [3] R. HEATH-BROWN, Obituary: Atle Selberg, *The Guardian*, 25 September 2007, p. 41.
- [4] D. HEJHAL and P. SARNAK, Some commentary on Atle Selberg's mathematics, *Bull. Amer. Math. Soc.* 45 (2008), 485–487.
- [5] In Memoriam: Atle Selberg, *The Institute Letter*, IAS, Fall 2007, pp. 6–7, and Summer 2008, p. 10. (See <http://www.ias.edu/about/publications/>) The IAS website also contains streaming videos from the Selberg Memorial (at: video.ias.edu/).
- [6] *Norsk Biografisk Leksikon*, vol. 8, Kunnskapsforlaget, Oslo, 2004, p. 161 ff. (See also the 1958 edition, vol. 13, H. Aschehoug, Oslo, p. 195 ff.)

G. D. Mostow

Atle Selberg was a mathematician whose accomplishments will be admired throughout the ages. Having enjoyed his friendship for nearly sixty years, I would like to describe how he was seen by his contemporaries.

¹One senses that the relative paucity of detailed working notes found either at home or in his office after his death (and linked, e.g., to specific results in lectures or published papers) is at least partially reflective of this fact.

G. D. Mostow is professor emeritus of mathematics at Yale University. His email address is mostow@math.yale.edu.

We differed in age by only six years, but to call myself a contemporary is a bit misleading, because in 1943 he had published more than ten articles and was receiving his Ph.D. at the University of Oslo, while I was a college undergraduate with no publications.

We both came to the Institute for Advanced Study in Princeton in mid-September of 1947, each of us being newly wed. I came with my wife, Evelyn; his wife, Hedvig, was delayed for several weeks because of immigration formalities.

I first heard of Atle Selberg from one of my colleagues at Fine Hall, where I had an office. My officemate spoke of Selberg with such awe that I can still remember his exact words, uttered in hushed tones, to this day: “There came out of the North a star who lit up for the first time a large piece of the Riemann Conjecture.” That mythic aura persisted throughout his career.

As it turned out, Atle became my neighbor. Evelyn and I were housed in one of the two apartments in the converted carriage house of Maxwell Manor; Hedi and Atle occupied the other apartment.

As is customary in Norway, Atle was reserved with strangers. There was a single narrow footpath leading from Fuld Hall to our building. Before Hedi's arrival in the U.S., Atle would avert his eyes as our paths crossed. Hedi's ebullience changed all that, and we gradually became good friends.

In one conversation Atle even poked fun at Norwegian reserve, telling the following anecdote. He was walking along the main street of Oslo when he encountered a cousin who stopped to talk to him. As they were conversing, the cousin's twin brother passed by. The twins acknowledged each other with only the tip of a hat. Atle asked his cousin, “Do you see your brother frequently?” The cousin replied, “We haven't seen each other for a year.”



Selberg in 1935, shortly before graduation from Gjøvik Gymnasium.



From a 1949 rotogravure section of a Norwegian newspaper, in an article on his elementary proof of the prime number theorem.

At that time, the Fuld Hall Common Room could hold all of the Institute members. After the daily tea the members of the School of Mathematics would often linger in the Common Room to talk shop and gossip. Among the Institute members, Atle had the reputation of answering questions on number theory immediately or, if he could not answer fully, he invariably got to the core of the problem at once.

His self-confidence was enviable. That may have been genetic, as the following story suggests. One day early in the summer of 1949, one of Atle's brothers was visiting him in Syracuse. They had been invited by Arthur Milgram to watch a doubles tennis match. Unfortunately, at the last minute one of the four players canceled. Milgram, seeing Atle's brother getting out of the car, ran over to him and asked anxiously, "Can you play tennis?" The reply, delivered deliberately with a Norwegian lilt, was: "Well, I don't know, because I have never tried." This is reminiscent of an exchange in a filmed interview with Nils Baas and Christian Skau where they posed the question: "Could you have summed an arithmetic progression at the same age that Gauss did?" Those who have seen that film will remember Atle's response: "Well, I don't know. No one asked me to."

Atle, who was a professor in the Syracuse math department, was the principal influence in my decision to join that department. As Atle and I looked through a list of the department members and their fields of research, I referred to him as a number theorist. He dissented, saying, "My mathematical interests center on function theory."

Atle not only was engaged in the deep problems of mathematics but also in the serious problems of the world. This may have been due in part to his experience with the Nazi occupation during World War II. For example, after World War II, Atle served for many years as a member of the Science Advisory Committee of the Belfer Graduate School of Yeshiva University in New York City. I believe that his impulse to serve was due to his desire to support an institution of a people that had gone through the Holocaust.

His friends and colleagues are proud to have known him.

Dennis Hejhal

During the Atle Selberg Memorial at IAS, one of the remarks I made at the start of my tribute was this:

If someone asked me to pick just one word to describe how Atle affected me over all these years, the word I'd select would be *inspiration*. In the 1970s, when I was at Columbia, Atle and I met quite regularly as I prepared volume one of my book on the trace formula. I remember how, after we got done

discussing math, I would always come back home feeling so inspired to work! I was twenty-six years old then. It's strange...but even thirty-some years later, whenever Atle and I discussed math, he still had that *same* effect on me. I'll miss that.

Atle was for me not only a wonderful mathematical colleague and mentor, but also a dear friend. There was a kind of synergy in our interactions, a kind of eclectic give-and-take, that Atle seemed to enjoy. In my lines here I'd like to expand on this a bit.

I first met Atle early in 1974 after something of a zig-zag. As a graduate student at Stanford, I had already heard the name "Atle Selberg" spoken of with great respect by both George Pólya and Paul Cohen in connection with the zeta-function. Following my degree from Stanford in 1972, I moved cross-country to Harvard to continue my work in one complex variable with Lars Ahlfors (with whom I had already established contact as a high school student). Though my work at Harvard started out in complex analysis, my interests there soon began shifting more towards discontinuous groups and automorphic forms. In the spring of 1973 Ahlfors invited his old friend André Weil to Harvard for a colloquium. In a conversation with Weil the following day, one of the things I learned was that "the zeros of the zeta-function are *not* going to be understood by complex analysis!" Weil suggested that I study his papers on the explicit formula and focus more on Selberg's work on the trace formula. I did so starting that fall after familiarizing myself with Selberg's earlier papers on the zeta-function. Enrico Bombieri's visit to Harvard that semester motivated me further in all this.

In trying to understand things better, I decided to see if I couldn't compute the trace formula explicitly for a number of congruence subgroups of $SL(2, \mathbb{Z})$, Selberg's 1956 paper being vague on this point. My calculations got to be so messy that I began to wonder if *anyone* had ever done them before! I mentioned this to Ahlfors and asked him if he thought going to see Selberg might be a good idea. (A letter to Selberg several months earlier had not garnered any response.) Though Ahlfors had known Selberg for many years, he responded hesitantly, telling me, "I'm not so sure; Atle has a reputation for being rather reserved and difficult to talk to." At about the same time, Lipman Bers was trying to entice me to move to Columbia. Bers heard about my predicament from Ahlfors, and the next thing I knew, I got a phone call from Bers, who said, "Don't worry! I'm on very good terms with Atle. Let me give him a call for you." Bers must have said some magic words, because I soon found myself knocking on the door at Fuld Hall 112 and being warmly welcomed by Atle.

After moving to Columbia as associate professor in the summer of 1974, I generally got together with Atle every few weeks or so, usually on Friday mornings at 10:30 for about two hours, following which we'd eat lunch together in the IAS cafeteria. If the weather was good, we'd sometimes follow that with a walk in the Institute woods.

We talked about all kinds of things, mathematical and non. Atle tended to be quite open with me, and I responded with enthusiasm. It did not take me long to decide that I wanted to try to write a book on the trace formula for $SL(2, \mathbb{R})$. Atle reacted positively. A typical morning get-together would start with a progress report and discussion of my latest ideas, then gradually morph into "news reporter mode", where I'd ask questions and then sit scribbling notes based on Atle's responses or on what he sketched out for me at the blackboard. (I actually preferred sketches, because to really understand things, I felt it best to try to work out full details on my own. Though it was slow going at times, once or twice I uncovered snags in what Atle had asserted.)

Things went on like this for about a year and a half until I finished volume one of my book; they then continued, albeit with slightly modified focus and a bit less frequency, during the remainder of my years in New York.

Atle told me any number of things over lunch or on our walks which still stand out vividly in my mind. Once, while we were discussing the Riemann Nachlass, Atle went off on a tangent and commented to me that of all the mathematicians he had ever met or known, in terms of technical strength he was most impressed by Carl L. Siegel and Arne Beurling.

On another occasion Hermann Weyl's name came up. After telling me how greatly he admired him, he paused and explained that "with Weyl, this went beyond his mathematics. As Weyl aged, he became a better and better human being. He made a conscious effort at this." This latter point was evidently an important one for Atle: in the 1990s he told me the story for a second time.

After moving to Minnesota in 1978, I kept in regular touch with Atle, particularly via occasional visits to IAS. Besides being fun, such return visits helped spur the completion of the second volume of my book.

In the mid-1980s, Bombieri and I became interested in trying to use Minnesota's Cray-1 computer to study the distribution of the zeros of Epstein zeta-functions. Hedi Selberg, who had coded some of the very first experiments run on John von Neumann's "IAS Machine" in the early 1950s (e.g., with Kummer's Conjecture) and who sometimes employed supercomputers in her work at the Princeton Plasma Physics Lab, reacted enthusiastically. Atle was also supportive; I remember his early comment to me that in number theory there was



Archives of the Mathematisches Forschungsinstitut Oberwolfach.

Selberg, right, with André Weil in 1968.

a long tradition of theory and experiment stimulating each other and that a well-formulated conjecture (born partly in experiment) could well have greater impact on the development of mathematics than a rigorously proved theorem.

Atle continued to follow our work with interest. I was at IAS when Enrico and I started to get our first results. I remember with fondness the way Atle would occasionally greet me with an upbeat, "Anything new?" when I first encountered him in the morning.

I decided to speak about this work at ICM'86 and was very pleased when Atle, who was visiting Stanford that summer, came up to Berkeley to hear my lecture.

Atle had a strong sense of honesty as a person; also a certain dispassionate candor, particularly when it came to scientific matters. Over the years I came to appreciate Atle's old-school ways very much.

During 1990, a year I spent entirely at IAS, this appreciation was enhanced by an eye-opening conversation the two of us had in June. Around nine months earlier I had been mulling over possibly organizing a broad-based computer project in automorphic forms. One day Atle asked me about this, and I lamentingly commented that due to skeptical reactions from several key senior people (one tantamount to a "Well, what can you actually *prove?*"), my earlier enthusiasm had pretty much lapsed. Atle first responded by recalling his earlier words to me about well-formulated conjectures and experimental insights. He then looked at me and in an almost admonitional tone said, "You know, one should *question authority*." He repeated the phrase, saying that he had learned this early—in connection with Hardy and Littlewood's work on the zeta-function.



Selberg and his family (daughter Ingrid, son Lars, wife Hedi) near their house in Princeton in 1960 or 1961.

Atle's words struck me for several reasons, not the least being the fact that I remembered seeing a similarly worded bumper sticker on his son Lars's car in the late 1970s.

Suggestions from Atle were always things to be taken seriously. Ironically, on several subsequent occasions, Atle's advice to me (coupled with a little logical consistency) proved to be just what was needed to resolve various points of contention that arose between *us*. Later that year, for instance, after a comment that Atle made to me began to gnaw away at my sense of basic fairness, I decided to stop by his office the next morning. I still smile at the way Atle unwittingly facilitated matters after I began by "casually" asking him if he recalled Lars's old bumper sticker, and he serendipitously replied, "Even if it's no longer there, I hope he *still* believes it!" Things went well. Our exchange of views ended on an up-note, with Atle reminiscing about his formative days as a mathematician in Norway and telling me several stories that I doubt I would ever have heard otherwise. (On the phone a couple days later, Hedi commented in her unique way, "You learned something important about Atle.")

In 1994, when I started spending part of my time each year in Uppsala, Atle was pleased. He had spent several months in Uppsala in 1939 and liked Uppsala's historical atmosphere. Atle came for return visits to the university in 1995, 1997, and 1998. In 1996 he came for a shorter stay to attend my formal ("white tie and tails") professor's installation in the University Aula. Atle joked that

he was there to provide moral support, since my lecture needed to be in Swedish.

There was something about Atle being (almost) on his "home turf" that seemed to put him in an especially good mood. This made for *a lot* of memorable happenings. From a mathematical standpoint, perhaps the most significant occurred in 1997. Atle was visiting for two months but preferred not to give a lecture series. Our department's finances were tight, and the department head commented to me that if Atle's pay could somehow be classified as a stipend (tax-free in Sweden), we could pay Atle a *more respectable* amount. He joked that perhaps I could get Atle to agree to "study something" with me—or at least work on a common topic. I told Atle. He laughed, and we promptly agreed to study zeros of linear combinations of L -functions. To stay out of each other's way, he would work *on* the critical line, I would work *off*. Atle took his assignment seriously, telling me one afternoon that he had been up 'til 2 a.m. trying to rework several technical aspects of his Ph.D. thesis.²

A couple of months after leaving Uppsala, Atle finally succeeded—at age 80—in proving his beautiful positive proportion result for linear combinations.

Atle's visits to Uppsala were always a special treat for the department; he stirred things up, and people looked forward to having him around. Continuing a long-standing habit of Atle and Hedi's, I especially enjoyed going lap swimming with Atle several times a week for 45 minutes at our local aquatic center, after which we'd then normally enjoy a very relaxed lunch together in the center's restaurant.

During subsequent summertime visits to Princeton, I would occasionally³ meet Atle at 7 a.m. for a 45-minute swim in the Nassau Swim Club's outdoor pool near the IAS housing complex; lunch followed four hours later. Chilly conditions, e.g., after a heavy rain, never bothered Atle. As a schoolboy in Norway, he recounted, "we" swam down to a water temperature of 12°C (54°F); to be excused, the teacher, a former military man, insisted on a note *from one's father*. Though in the mid-1980s Atle still had a mean backstroke (once encountered first-hand thanks to a narrow lane!), his preference more recently was for an unusual-looking, slow, underwater sidestroke of his own design that he could keep up for long periods of time.

Atle inspired subtly...and in many ways. He was also fun. As I look back, I feel very fortunate

²It turns out the 2 a.m. was not without precedent; Lars and Ingrid Selberg recently told me that years earlier (during the 1960s and 1970s, at least) their father had a habit of working late in his study at home, very often to well past midnight.

³(and typically with only one eye open!)

to have gotten to know him the multifaceted way I did. The warm hospitality and many kindnesses shown to me over the years by Hedi, and later by Mickey Selberg, made a real difference in this.

Following his ninetieth birthday dinner, I stopped by to wish Atle well, and to tell him that I hoped to visit him again later that summer to discuss a bit of math. Very sadly, that hope would never come to pass.

A few days prior to Atle's memorial at IAS, I came across a reprint of his in Norwegian from 1979 that I had simply put aside (not being able to read it at the time). It was a tribute in honor of Viggo Brun. Leafing through it, I found that I could now read large portions of it relatively easily. The way it closed struck me with some poignance as being eminently appropriate also for Atle. I paraphrase:

Atle Selberg som vi minnes i dag, var ikke bare en stor matematiker, men også stor som menneske. Hans minne gjør oss alle rikere.

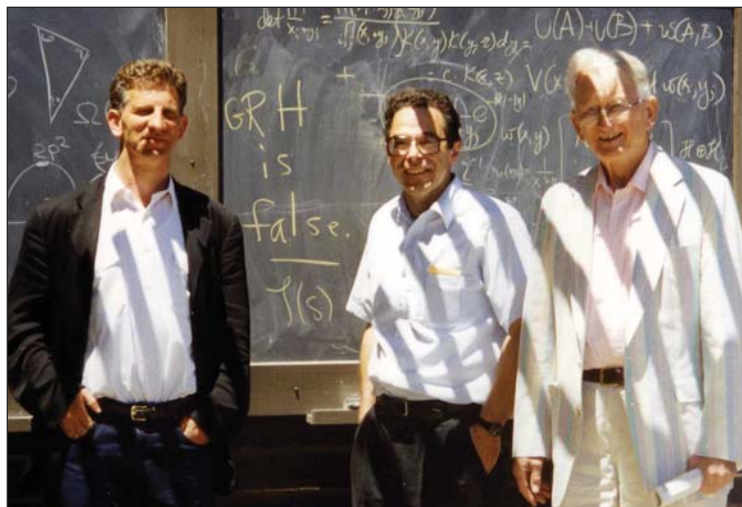
[Atle Selberg was indeed not only a great mathematician, but also great as a human being. And our memories of him do enrich us. In spirit.]

Peter Sarnak

In a recent *Bulletin of the American Mathematical Society* article (October 2008), Dennis Hejhal and I give a brief account of the impact of some of Selberg's mathematical contributions. What follows are some personal reminiscences about him.

I cannot remember exactly when I first heard the name "Atle Selberg", but it was certainly in South Africa when I was still an undergraduate. By the time I was a third-year graduate student at Stanford, his name was very familiar to me, and it was during this period (1979) that I first met him. My advisor, Paul Cohen, arranged for me to meet Selberg at the Institute, as I had been working on topics around the trace formula. Paul warned me that Selberg could be intimidating, and indeed, like most other mathematicians, I found that to be the case. Now that I think back on it, I understand better why. Given his stature in the field, one's first conversations with him (and for many perhaps also their last) were bound to be around some aspects of his work. This, coupled with his profound understanding of any issue related to what he had thought about in depth and his direct and matter-of-fact Norwegian style, was bound to leave one feeling somewhat intimidated. However, this image quickly changed for me and, I think, for others who

Peter Sarnak is professor of mathematics at Princeton University and the Institute for Advanced Study. His email address is sarnak@math.princeton.edu.



Peter Sarnak, Dennis Hejhal, and Atle Selberg in a humorous mood at MSRI in 1999. (Photo courtesy of D. Hejhal.)

were also fortunate to get to know Selberg. He was kind and generous, he had a keen sense of humor, and he valued his friendships deeply.

In my case I got to know him a little later when, in 1980, he came for the first of what was to be a number of long-term visits to Stanford. During each visit he gave a series of lectures on some aspect of his work. He and his wife, Hedi, enjoyed very much the hospitality of the mathematics department, in particular that of Paul Cohen, Ralph Phillips, and their families, and especially Isolde Field. Selberg's rating of the faculty club at Stanford as being the "best in the country" pleased Ralph Phillips, who was of a similar opinion, and they enjoyed eating lunch together there daily.

During this first visit, Selberg gave a series of lectures on the trace formula. He also served as one of the examiners on my thesis. I was quite worried that he would, in the Scandinavian tradition, take the position as the official "attacker" of the thesis, but after posing some probing questions, he seemed quite content. Immediately after the examination and on many later occasions, he offered some excellent general as well as some specific advice, much of which I carry with me to this day.

During this same visit I drove him up to Berkeley, where he had agreed to give a colloquium. The colloquium chair there had been pushing him to talk about sieves, a topic that he wasn't thinking about at the time, and he was quite irritated by being put in the position of not obliging. The topic Selberg chose for his colloquium is known today as the "Selberg Integral", but in the early 1980s it was hardly known, if at all. The colloquium hall was packed, as he didn't lecture often, and most people just came to see him. But in true Selberg style, he started the lecture by explaining that he was going to compute an integral that he had done in his youth. He offered no reason for choosing to talk

about this. It took some months after this event for people to digest the fact that his old paper contained the solution to conjectures of Dyson and MacDonald, which were “hot” topics at that time. It was in response to Bombieri and Dyson’s questions about related integrals that Selberg pointed out his old paper, and he took this occasion to make it better known. Most of the audience left the lecture perplexed about what they had witnessed, and Selberg seemed to enjoy that.

Photograph by Betty (Mickey) Selberg.



At the February 14, 2003, wedding of Atle and Mickey Selberg: left, Enrico Bombieri, best man; maid of honor Shirley Dwork (center); and Atle Selberg.

Paul Cohen and I had spent much of 1979 and 1980 studying Selberg’s work on harmonic analysis on locally symmetric spaces and filling in details of the many results which had simply been stated with only hints of the methods used to prove them. (Selberg later told me that he was very lazy by nature and that once he understood something, he found it very difficult to energize himself to write it up for publication. I used to be unsympathetic towards this attitude, but as I grow older I find myself being much more sympathetic.) In putting ourselves in his mindset as we supplied proofs of his results, we found ourselves explaining things to each other in Selberg’s accent and manner, so much so that it became a competition between us as to who “did a better Selberg”. Ten years later the opportunity arose for us to resolve this and to act out our Selberg imitations in front of him. The Stanford mathematics department gave me a farewell dinner on the occasion of my leaving for Princeton, and Selberg happened to be visiting at that time and he too was invited. In Paul Cohen’s speech (better called a roast) he proceeded to imitate me doing “a Selberg”. In my response, I had the chance to do the same. Selberg was amused by the two of us putting on strange Norwegian accents, but he didn’t seem too impressed. Here and in other circumstances (e.g., when fantasizing naively on some notorious unsolved mathematics problem!), he had the knack of sobering you up by making a key observation.

Once you got to know him, Selberg liked to talk and to tell stories from his experiences, mathematical and otherwise, and there were many. One which sticks in my mind is about his joint work with Chowla. (Selberg was particularly fond of Chowla, who is distinguished by being Selberg’s unique scientific coauthor and the one responsible for Selberg’s “Erdős number” being 2.) It was before the days of computers, and Chowla had noted that $L(\frac{1}{2}, \chi)$ must be nonnegative for a quadratic Dirichlet character, for otherwise the Riemann Hypothesis is false for this Dirichlet L -function. He wanted to compute these numbers for various χ ’s, such as the one corresponding to $\mathbb{Q}(\sqrt{-163})$, where he expected the value was small. Selberg provided an efficient method to compute these values using Eisenstein series, and Chowla went home that evening to do the calculation. The next day Chowla was waiting nervously for Selberg outside his office. He had found that for the specific character above, the value was negative. They quickly set out to redo the calculation and soon found the “error”. In describing his method, Selberg used Dirichlet’s notation for binary quadratic forms, $ax^2 + bxy + cy^2$ (as most of us do today), but Chowla was old-fashioned and used Gauss’s notation, $ax^2 + 2bxy + cy^2$. Once this inconsistency was clarified, the value turned up positive (it is 0.0685...). This was good news, of course and, even better, it led them quickly to their well-known “Chowla-Selberg” formula.

When I and my family moved to Princeton, Atle and Hedi were very welcoming, and we enjoyed many lunches, dinner parties, and outings together. During this period I would often use Atle to lure a desired speaker by saying that Atle would join us for dinner. Later, when Atle’s hearing deteriorated to the point where he was no longer comfortable sitting at a crowded table, he came less often to such events. However, even well into his eighties, if there was a special seminar, he would come and also join us for dinner, the most recent instance that I can recall being a few years ago when Dan Goldston came to speak about his joint breakthrough concerning the differences between consecutive primes.

After Hedi passed away in 1995, Atle made an effort to be more directly outgoing, but he was clearly very lonely. In 1998 Atle and I made a joint tour of China. We traveled together extensively by air, train, minibus, and taxi and were treated lavishly by our local hosts. Our days consisted of lecturing, listening to lectures by locals, discussions with young Chinese mathematicians about mathematics and their work, and a lot of touring and shopping (for Atle, this meant mainly acquiring new dragons for his already quite large collection). This was quite a “bonding” experience for the two of us. Our host in Beijing, Professor Pan, was very concerned that no harm would come to

Atle. In particular, he insisted that Atle retire to his room immediately after dinner, which on a number of occasions was as early as 7 p.m. Atle was quite frustrated by this and insisted that I come to his room for a drink (unknown to Professor Pan, Atle had stashed quite a bit of alcohol in unusual places). I could see Atle's disappointment with my lack of tolerance of any quantity of alcohol.

During this trip Atle mentioned a few times that on the way back to Princeton he planned to stop over in Palo Alto for a week or so. When I asked if he was lecturing at Stanford, he answered no, and I was quite perplexed by his breaking his trip home in this way. When I saw him next after China (which was three weeks later at a conference in Vienna on the Riemann Hypothesis), he explained to me with a twinkle in his eye and a broad smile that his stopover in Palo Alto was a success. His purpose was to propose to his and Hedi's longtime friend, Betty (Mickey) Faith, that she move to Princeton to be his partner. She was taken by surprise, but after thinking about it for a short time, she agreed. A few years later they were married. From that time on until his death, Mickey was always by his side, and Atle appeared to be constantly in a good mood.

Atle Selberg stands as a towering mathematical figure whose works have allowed a number of mathematicians to achieve quite striking goals. I was fortunate to benefit not only from his mathematics but also from his excellent general advice, his inspiration, and, not least, his friendship. I will miss him.

John Friedlander

I first heard of Professor Selberg some years before I met him when, as a beginning number theory student, I attended a series of lectures (by J. Chalk) on the Selberg sieve.

By the time I finished my graduate studies, I had heard and read quite a bit more about his work, and I was thrilled when—no doubt partly because my supervisor, Chowla, was an old friend of his—he responded to a letter from me with an offer of my first academic position, as his assistant at IAS. Apart from bed and bath, that letter remained on my person for several weeks thereafter.

Only a few minutes after I first arrived at the Institute the following autumn (1972), I was introduced to Selberg by Deane Montgomery, with whom I had become acquainted during his stay at Penn State the previous spring. Selberg was at the time sitting in the Fuld Hall lounge reading the *New York Times*, the first of what seems a few hundred times I was to come upon him doing just that over the thirty-five years to follow. Fighting desperately to find conversation, I asked him if

John Friedlander is University Professor of Mathematics at the University of Toronto. His email address is frd1ndr@utsc.utoronto.ca.

there were many lectures given at the Institute. He responded that there were in his opinion far too many of them (within a few weeks I discovered to my surprise that I had begun to agree with him).

Perhaps a week later I went to his office to tell him about a problem I had just begun to look at. I had picked a rather difficult problem that had seen no progress for many years, and I had concocted a somewhat artificial modification of it which one could hope to attack. Selberg listened patiently, and after I finished he offered the opinion that it was often possible to modify an interesting and difficult problem, getting a question about which one could obtain some information. However, he said, before going to the trouble of obtaining this information, one should probably ask oneself whether this will then be information about something that anyone would want information about. As I left his office, I vowed to myself that in the future, even if I had nothing worthwhile to say about them, the problems he would see me working on would be subject to no such criticism. Within a few days I had an idea to tell him for what seemed a reasonable attack on a much-studied and difficult problem. Selberg from that day on always offered me his time, his attention, and his encouragement.

These were just my first glimpses of the crusty sense of humor and the bit of feigned conceit that cloaked the kindness and modesty lying beneath the surface, and not so far beneath as to be that difficult to uncover.

During my second year at IAS there were a number of junior visitors interested in analytic number theory, and Selberg decided to run a weekly seminar. He gave the first lecture, and afterwards each of us took a turn. Each week at the end of the lecture he'd ask for a volunteer for the next week. After we'd each given one, he gave a second one. A few of us then did the same. Gradually, however, we realized that when there was no volunteer, Selberg would pull some manuscript from his desk and give a lecture on a piece of work that nobody had ever seen before. After that there were very few volunteers. Much of this work did appear years later in the second volume of his *Collected Papers*.

The job market was very tough. At the first AMS Employment Register, in January 1972 in Las Vegas, there had been over 1,400 interview requests for a position at a branch campus of one of the less illustrious state universities, a number I was told by their chairman while sharing a cab to the airport. After my two years at the Institute, I had a succession of temporary positions. Selberg was writing letters for me almost every year for about a decade. After I finally received tenure I was told by one of the members of my tenure committee that Selberg had said in his letter about me: "He's a pretty good mathematician, but are you guys sure you want to put up with his pipe for the

rest of your lives?" I guess he just wanted to write letters for another ten years!

Up until 1981 I was a very heavy pipe smoker. At that time it was already frowned upon, but not the anathema it has since become. During one of my trips back to Princeton in the late 1970s, Selberg had invited me to stay at his home. When I arrived, he greeted me at his front door with the words: "Hedi says you are not to smoke in the house." (I remember that it was his late wife, Hedi, who taught me that he was to be called "Atle" and not "Professor Selberg".) I said that this was fine with me, but I hoped he wouldn't mind if I went out for a walk and a smoke after dinner. When the time came for me to go out, he absolutely insisted on accompanying me throughout and did so despite the quite cold weather and a strong wind which, flouting my best efforts to position myself, seemed determined to blow the smoke directly in his face.

There was an interesting story that took place about the same time. I know of it only second-hand (not third), but I believe it completely, and it very accurately depicts the protagonists. A young number theorist spending a year at the Institute had been trying to evaluate a certain integral and, finding it beyond him, was going from person to person asking for help. Having no success after several attempts, he spied Chowla at teatime and showed it to him. Chowla's response was, "For five dollars I can get you the answer in half an hour." Half an hour later they met again, and Chowla showed him the answer. The postdoc paid the five dollars and said, "That's amazing, Professor Chowla. How did you do it?" "It was easy," said Chowla. "I showed it to Selberg and asked 'How do you do this?' Selberg looked at it and went 'Hmmm, oh yes' and wrote down the answer."

There are many other stories about Selberg that one can tell, and perhaps in an article like this more than one person will tell the same story. Some of the ones I remember best are the ones where he exhibits the humorous comment that on the surface sounds a bit immodest but really isn't, ... well maybe it is to those who wanted to see it that way. A good example came on an afternoon bus excursion during the Durham meeting (July 1979). The tour leader, having given us the requisite free time to tour Durham Castle, boarded the bus and, seeing it still half-empty, called out facetiously: "Is everybody here?" The response from Selberg: "Everybody who counts!"

In early 1996, after a several-months stay at IAS and just before my departure, Atle took me to dinner at the Institute dining room. We talked about my work with Henryk Iwaniec on primes that are the sum of a square and a fourth power, the details of which were maybe twenty percent complete but of which, because we already had a full sketch of the argument, I was fearlessly confident (not my usual posture). Atle cautioned me

that often it is the details that are one's undoing, and yet I felt that in spite of his always careful nature, he seemed to believe in my optimism. I then started to reminisce about how proud I was of having been his assistant. This, of course, was too much for him to take: "What time period was that?" he asked. "Oh, no wonder you didn't mind; my children were grown up by then. A few years earlier and you would have had to baby-sit. Your memories would be rather less positive."

The last time I saw Atle was at the dinner for his ninetieth birthday, to which a few of us took him at the Princeton University Faculty Club. He was already somewhat unwell, but I don't think any of us had a clue just how serious this was going to turn out to be.

Atle was a really wonderful teacher and friend to me in many ways. I sort of knew that I would be quite sad when he passed away. Yet, when he did die, I was really surprised at the extent to which this was the case. And still is.

Dorian Goldfeld

I first met Selberg at the 1969 conference on number theory at Stony Brook, but because I was very young and there were so many famous number theorists there, I spoke only a few words with him at that time. My long and wonderful association with him really began in 1973, when I had a teaching position at Tel Aviv University and was living in Jerusalem. I would go to the Hebrew University on occasion. I was utterly shocked when one day I saw Atle sitting at a desk in an office at the Hebrew University. He told me he came to Israel every few years because his wife had family there and that he would be there for one term, going to his office three times a week. At the time there were almost no number theorists in Israel, and I felt quite isolated with very little hope of obtaining a good job in the United States.

Seizing the opportunity, I visited Selberg as often as possible, and we spent most of the time talking about mathematics. I seemed to be the only one spending serious time with him in Jerusalem. He taught me automorphic forms, the trace formula, and how to see the essence of a proof by looking for elegance and simplicity. Just before he left Israel for Princeton, he asked me what I was planning to do the year after. I told him I had no prospects other than staying at Tel Aviv University, and he suggested that I apply to be a member of the Institute for Advanced Study for the next year instead. I therefore returned to the United States and became Selberg's assistant at the Institute, where I continued to have the extraordinary opportunity to interact with one of the greatest

Dorian Goldfeld is professor of mathematics at Columbia University. His email address is goldfeld@columbia.edu.

mathematical minds of our age. Selberg turned my career around and put me on the path to success. That year Bombieri, who was also visiting the Institute, offered me the chance to go to Pisa to work with him for two years. While in Italy, I learned of the Vaughn Foundation, whose main purpose was to fund research on Fermat's Last Theorem (FLT). With a letter from Bombieri, I soon obtained a grant from the Vaughn Foundation.

In 1981 I was at MIT, and James Vaughn suggested that it might be worthwhile to have a meeting on mathematical work relating to FLT. Now at that time no self-respecting mathematician would publicly admit that they were working on famous problems such as the Riemann Hypothesis or FLT. However, I was fairly certain that they were working on them and thus thought it would be a good idea to get people out of the closet about it. In an attempt to do so, I approached Barry Mazur and asked him if he'd like to organize such a meeting. He immediately agreed and said that many other people were also interested. In the end, Harold Edwards, Nick Katz, Neal Koblitz, Barry Mazur, Andrew Wiles, and I were the organizers. I invited Selberg to the conference, and he promptly accepted the invitation. He told me he had always been interested in FLT.

The FLT conference, generously funded by the Vaughn Foundation, was held at MIT's Endicott House, a magnificent 1930s mansion built in the style of a French manor on a secluded 25-acre estate. We arranged for the Selbergs to have one of the best rooms at Endicott House. I fondly remember shooting darts and chatting with Atle every evening after dinner at Endicott House.

A few years after the FLT meeting, I received a call from Hedi Selberg. She told me that Atle was soon going to be seventy years old and that they wanted to do a conference in his honor in Oslo. She reminded me how much they enjoyed the Endicott House conference, and she wanted to know if I was interested in organizing a meeting for Atle in 1987. I was extremely honored that Atle would think of me for organizing such an event, and I immediately accepted her offer. I called James Vaughn, who said his foundation could provide support for the conference. Later I found out that the Selbergs had also contacted Karl Aubert and Enrico Bombieri for organizing this event. The Selberg seventieth birthday conference was probably the most successful one I have ever attended; in the end, it was funded by various Norwegian councils and societies, as well as Landon Clay and the James Vaughn Jr. Foundation Trust.

Years later, when Selberg was in his late seventies, I asked him if he'd like to give a talk at the Columbia Number Theory Seminar. He looked at me with his boyish smile and said people at his age don't give talks. In the next few years I brought the question up a couple of times, but he didn't



Oslo, 1987: Selberg with family after being named Knight Commander with Star of the Royal Order of Saint Olav by the king of Norway.

seem interested in speaking about mathematics anymore, so I thought I'd give up asking. Then one day he came to me and said he had something to talk about. I was surprised and delighted when I heard this. It was one of his last mathematical talks, and the room was packed as he explained how linear combinations of L -functions will have a positive proportion of their zeros on the critical line. His result is probably the best theorem ever proved by a mathematician over eighty!

I feel extraordinarily fortunate in having such a long and close personal friendship with Atle Selberg. Much of my own work in mathematics was inspired by what I learned from him over the years. He was a remarkable man in many ways, and I shall cherish our time together for the rest of my life.

Samuel J. Patterson

My first encounter with Atle Selberg was a virtual one. In 1972, during my first year as a research student in Cambridge, I had been studying Joe Lehner's *Discontinuous Groups and Automorphic Functions*. In the final chapter of that book there is a brief introduction to the theory of automorphic functions of several variables. Joe Lehner wrote at the end of the introduction to that chapter: "But the reader cannot fail to improve himself [sic] if he consults the following publications." The list included Selberg's "Indian" paper. Shortly afterwards I consulted Volume 20 of the *Journal of the Indian Mathematical Society* to make a copy of Max Deuring's paper therein and, remembering

Samuel J. Patterson is professor of mathematics at the Mathematisches Institut der Georg-August Universität Göttingen. His email address is sjpg@uni-math.gwdg.de.

Lehner's injunction, made a copy of Selberg's paper as well. I took it home and looked at it the same evening. This was my epiphany. Although it was to be some time before I understood it in detail, I knew this was where I wanted to work. One thing that fascinated me from the beginning was the theory of Eisenstein series. In the "Indian" paper, Selberg only asserts the analytic continuation of these functions without any hint of a proof. I was intrigued and found a little later the brief sketch of a proof in his Stockholm address. At that time there was apparently no one in the United Kingdom who knew anything about these matters, and I spent several months reconstructing Selberg's proof in the case of Fuchsian groups. It was difficult, but I learnt a great deal from the effort, and because of it I count Selberg as one of my teachers.

A little later Selberg came to Cambridge—I think it must have been in 1973—and I saw him for the first time. Two things remain in my memory here. First, I was overawed by him. I was a mere research student at the time, and I did not have the nerve to go up and speak to him. I cannot recall anyone else who had this effect quite so strongly on me. Later I got to know him a little, and he was both courteous and kind. A few years ago I learnt that Selberg in his turn was overawed by Carl Ludwig Siegel, who really could be very intimidating, and this made me feel much better about it.⁴

The second thing was that he was quite different in appearance from what I had expected. The "Indian" paper is written in a somewhat old-fashioned style and becomes very concrete. I had not expected its author to be a very elegantly and fastidiously dressed gentleman. He would be my choice for the "Best-Dressed Mathematician". His two lectures then were on the arithmeticity of groups operating on products of the upper half-plane and on Siegel's conjecture about irregular primes. The lectures were unlike any others. He stood at the front, facing the audience and speaking slowly with his gentle Norwegian accent. He only rarely wrote anything on the board, and consequently, despite the relaxed diction, it was very hard to take notes. I have often regretted that neither of the Cambridge talks ever appeared in print.

I had much more contact with him around 1980. First, he was a central figure in the Durham conference of 1979, organized by Christopher Hooley and Heini Halberstam. From shortly after that conference up to February 1981, I was a Benjamin Peirce Lecturer at Harvard. Dorian Goldfeld had invited Selberg in 1979 to MIT to lecture over a term on Eisenstein series, so I saw him both there and on other occasions in Princeton. He was very helpful whenever I had questions. What always impressed me was his remarkable memory for what he had

⁴When I spent my first year in Göttingen (1974–75) Siegel was sometimes to be seen, but I considered it to be out of the question to even think of talking to him.

done. (This did not seem to always work, as noted, for instance, in the footnote on p. 152 of Vol. 2 of his *Collected Papers*.) I still have somewhere the notes of his explanation to me of his version of the Shimura Correspondence (he had apparently discovered it somewhat earlier than Shimura but did not set much store by it). This method was rediscovered later by Winfried Kohnen and Don Zagier and is quite different from the method that Shimura presented at the Antwerp conference.

Selberg had a very developed persona as a country gentleman. I found that I could imagine him in an Irish country house, at least if he were not quite so well dressed. He was very keen on talking about matters other than mathematics, for example, about the natural world. He also adopted the appropriate political stance. With the intention of shocking me, I suspect, he once spoke about his time in the Norwegian Army and said that "a cannon is a very beautiful piece of equipment—at least, if you are on the right side of it." One has to remember that he lived through the German occupation of Norway and that, therefore, his early years of maturity were lived in much more difficult circumstances than most of us have had to cope with.

Because of the German occupation of Norway, Selberg was isolated in his early twenties. He had worked very much on his own. He describes his early years in his notes "Reflections around the Ramanujan centenary" (pp. 695–706 of the *Collected Papers*, Vol. 1). Selberg liked to tell one how few books he owned; this may have been so, but he was very well read. He seems to have done his reading mainly in libraries, starting with that of his father. It is then no surprise that he knew the works of the great Scandinavian mathematicians well. Of Abel he spoke with highest praise; admittedly, no one who has read Abel can do anything else. He also knew the work of Ivar Fredholm well. One can see how the theory of Eisenstein series arose when one compares his first version, the one in the Göttingen notes, with the work of Fredholm. The second proof of the analytic continuation of Eisenstein series, the one sketched in the Stockholm address, was more in the Hilbert mold and was based on an idea of Walter Roelcke. There have been various variations since then; Selberg spoke several times in later years about one particularly simple proof (cf. pp. 629–631 of the *Collected Papers*, Vol. 1).

The theory of Eisenstein series seems to me to be one of Selberg's most important achievements. It has become part of our way of thinking about automorphic forms, and it has led to many insights, of which perhaps the best-known example is Langlands' idea of L -groups. Also the many generalized Rankin–Selberg integral representations of L -functions would be impossible without this concept. Most importantly, it offered analytic number theory new methods for proving the analytic

continuation of interesting functions, methods not based on the Poisson Summation Formula.

On the subject of mathematics, one topic on which Selberg's opinion was frequently sought was the Riemann Hypothesis. During the later 1980s I had been thinking about Kleinian groups and also the Riemann zeta-function (trying to come to terms with H. Matsumoto's purported proof of the Riemann Hypothesis). While talking about the latter at the conference in Tel Aviv on the occasion of Ilya Piatetskii-Shapiro's seventieth birthday, I ventured, incautiously, the opinion that the Riemann Hypothesis would mean that we were living in the best of all possible worlds but that it was not given to us mortals to know whether this was the case. Selberg came up to me afterwards, somewhat irritated, and said that whereas it was clear that the everyday world was by *no* means the best possible, in matters of real importance, that is, the Riemann Hypothesis, he was convinced that we were living in the best of all possible worlds. It should be added here that his views on this were not at all immutable despite the thought that he had put into it; for example, on the last page of his article on "The zeta-function and the Riemann Hypothesis" (*Collected Papers*, Vol. 1, pp. 341–354) he takes a thoroughly skeptical stance.

There was another side to Selberg that rather surprised me. Once, in the 1980s, when I was visiting the Institute, he invited a large number of us to a party. Around 11 p.m. people started muttering about babysitters and so on. Selberg (and Hedi) were rather upset; he said that in earlier days, in the 1950s, things really only got underway around midnight, when they would roll up the carpets to dance. I never experienced Selberg as a socialite, but I suspect that he really was one.

With regard to his house, a phrase he once used has remained embedded in my memory. The first time I was invited there, he explained very carefully how to get there. I could not miss it, for "it has a *very red door*." The house did indeed have a very red door.

Selberg writes in a note at the end of the second volume of his *Collected Papers* that writing did not come easily to him. He told me that when he became a permanent member of the Institute, his first thought was that he would never have to worry about publishing papers in journals again. Indeed, almost all of his papers afterwards are in conference proceedings, even if these appeared in journals. It clearly needed a bit of pressure from an editor to get the paper out of him. He had then much more time than Hedi, who was a researcher at the Princeton University Plasma Physics Laboratory. Despite the image as the mildly conservative country gentleman he affected, it was Selberg who was in charge of the house and children. This domesticity was very uncharacteristic of his generation. The persona he

cultivated of the urbane country gentleman was only one part of the story.

Roger Heath-Brown

I first met Selberg in 1978, I think, when he came to give a lecture in Cambridge. I had finished my Ph.D. and was just starting a Junior Research Fellowship at Trinity College. By that stage I was well aware of his wonderful achievements in so many of the areas on which I had worked: on the zeros of the zeta-function, on the elementary proof of the prime number theorem, and on sieves. They were awe-inspiring pieces of work which I had studied in detail. I do not remember the title of his talk, but the subject was the zeros of the zeta-function. Naturally, I was very much looking forward to hearing the great man speak!

A few months beforehand, my interest in the zeta-function had led me to examine Levinson's work on the proportion of zeros on the critical line. In the 1940s Selberg had shown, by a very natural route, that a positive proportion of the nontrivial zeros of the zeta-function must lie on the critical line. Indeed, since the method detected sign changes, the zeros found by Selberg were necessarily of odd order. I therefore asked myself what one might say about the zeros produced by Levinson's method and discovered to my surprise that, in effect, they were all simple. I was pleased with this observation and had written to Montgomery about it but had not told people in Cambridge.

So imagine my surprise—my horror—when Selberg's lecture described exactly the same deduction from Levinson's analysis. In later years I heard tales of Selberg's unpublished manuscripts, of how an unfortunate student might lecture on his prized result in front of Selberg only to be told, "I made the same observation in 1943, but decided not to publish it."

After the talk I was given the task of walking Selberg from the lecture theatre back to college for dinner. Somehow I plucked up the courage to broach the subject of my own work on Levinson's method and simple zeros. Selberg was very kind and insisted I should go ahead with publication.

And so, for me, there was a happy ending. For this kindness and for all his other gifts to mathematics, I will remember him.

K. M. Tsang

It was in the spring term of 1981 that Professor Atle Selberg gave a series of lectures on

Roger Heath-Brown is professor of pure mathematics at the Mathematical Institute, Oxford University. His email address is rhb@maths.ox.ac.uk.

Kai-Man Tsang is professor and chairman of the Department of Mathematics at the University of Hong Kong. His email address is kmtsang@maths.hku.hk.



On the Great Wall of China, 1998. Kai-Man Tsang (with sunglasses) is just behind Selberg.

sieve methods at the Institute for Advanced Study. The very first lecture had attracted a large crowd to F119 of Fuld Hall. The number of listeners then quickly shrank to a steady state of five or six after the first two lectures, and I was proudly amongst the few that persisted till the end of the whole lecture series.

But I have to confess that I had difficulties in following closely Selberg's lectures and that my understanding of his beautiful sieve theory was superficial. However, as a graduate student at Princeton University I was keen on exploring whatever new things that I found interesting. Another reason I went to these lectures was that I wished to meet the famous speaker, Professor Atle Selberg. A couple of times after his lectures, I gathered my courage and was brave enough to talk to him and to raise some questions. I never expected that he would be so kind as to lead me into his office and even give me an offprint of his Stony Brook lectures, which he was doing in his lecture series. These were my first encounters with Professor Selberg, who later became my teacher and friend.

One afternoon in November of 1981, a few weeks after I passed my general examination at Princeton University, I went to see Professor Selberg in his office without making an appointment. I expressed to him my interest in analytic number theory, in particular on sieves and the theory of the Riemann zeta-function, and asked if he would be willing to be my thesis adviser. To my surprise, he agreed on the spot. I guess my attendance at his lectures earlier had played a role. That marked the beginning of our long association, which, for me, will last forever.

From that time onwards, Professor Selberg kept every Friday afternoon reserved for me. In his office he went through with me many of the things that he had done in the past, including mainly sieve methods and the theory of the Riemann zeta-function. But we also frequently digressed to other problems that interested both of us. He wrote on the boards in his office and patiently explained every detail. This was not his typical style of lecturing. He had

evidently put in extra effort to accommodate my pace. In addition, he also organized his materials in such a way that would allow me to see clearly the flow of his simple, elegant ideas. I still remember vividly the way he constructed what is now called the Beurling function, an analytic function of order one which approximates the characteristic function of an interval.

Our weekly meetings had no preset finishing time. If we finished around 3:30–4:00 p.m., we would go together to the tea and cookies in the common room. He would then sit down to read the *New York Times* and work on the crossword puzzles. Sometimes, when we finished a bit late, he would bring me to the cafeteria for coffee. This coffee time offered me excellent opportunities to talk to him and to learn from him about things other than mathematics. We talked leisurely and freely on anything of common interest, ranging from wildlife (one of his favorite topics) to U.S. politics and the environment. Sometimes we also shared stories about his family and mine. To a young man from the Orient and with a totally different cultural background, the views and experience of this knowledgeable old man were inspiring and refreshing. On some festive occasions, he and Hedi (his first wife) would invite me to join their family gatherings. They tried to enrich my social life in Princeton as well.

The generous support of Professor Selberg kept going even after I completed my doctoral study at Princeton University. He offered me the opportunity of staying on at the Institute for Advanced Study by appointing me to be his assistant (though, in actual practice, just the reverse was true). Even after I returned to Hong Kong in 1985 to reunite with my family and to teach there, his help continued. He arranged for me to be invited to conferences, first to the Oslo conference which celebrated his seventieth birthday and then to several others following that. He also invited me for shorter visits at IAS in later years. In the summer of 1990 my wife and I visited them again at IAS. At that time my wife had just gotten pregnant, and she was experiencing all kinds of typical and atypical discomforts. Hedi helped to allay her fear and gave her valuable advice. We still have the book on pregnancy which she gave us.

Professor Selberg had much interest in visiting Mainland China, and his first trip to the Far East was to Hong Kong in 1993 to attend a conference our department organized. That was a great honor for our whole university. Later, in May 1998, he visited us in Hong Kong again. This time he stayed for a whole month and gave altogether eight lectures. He then continued on to visit three places in Mainland China, namely, Beijing, Shandong, and Xian. He was still enjoying good health and even climbed the Great Wall under his own steam at the age of eighty-one.

I got to know Professor Selberg in the later part of his life. During this period I could see that he had been very generous with his ideas and expertise to whoever came to talk to him. Many young mathematicians, including Amit Ghosh, Brian Conrey, Heng Huat Chan, etc., had benefited enormously from his ideas and help. But I can boldly conclude that among all of them, I am the most privileged, for I had been with Selberg for one afternoon each week for more than three years. Besides learning mathematics from him and seeing him do mathematics on the spot, his rich and unique experience of life has enlightened and inspired me all the way through my development as a professional mathematician. I once accepted an offer of a position from an institution, and then, for some reason, I wished to change my mind. I went to see him and asked how I could make up some kind of excuse for withdrawing. But he told me, "Things are best explained by the truth." I remembered this valuable advice verbatim and have kept it as my motto ever since.

Many mathematical friends of mine have been curious about how I got to be Selberg's student. In response, I simply told them the facts: "I went to see him, expressed my interests in the subject, and then asked." I don't know whether anyone else ever approached him on the same question, but I do think he would have been willing to take as his student anyone who had strong interests in the subject. Under his supervision I was given absolute freedom to develop and do whatever I was interested in. He seldom asked me what I was doing or checked my progress. While I enjoyed this high degree of freedom, I also felt the pressure. I knew that I was on my own and that I had to be self-driven.

From time to time he would be consulted by mathematicians concerning new results they had obtained. Sometimes his comment was, "I obtained the same (or even more) in 1941 and so on..." Such a remark to some mathematicians may sound a little embarrassing, but to me this amounts to a great compliment. In my reminiscences, it was in only a couple of instances that I was able to obtain something on a par with what Selberg had done in the early 1940s.

I once asked Selberg whether he believed in luck. He replied, "Luck certainly plays a not insignificant role in one's life." With this I fully concur. I have been most fortunate to have had Professor Selberg as a teacher and friend in my life.

Wenzhi Luo

Atle Selberg left us with an immense scientific legacy. He will be remembered as one of the chief

Wenzhi Luo is professor of mathematics at The Ohio State University. His email address is wluo@math.ohio-state.edu.

architects of the twentieth century who shaped the global outlook of modern mathematics. His profound, monumental works on number theory and automorphic forms are a continuing source of inspiration. He discovered and established grand theories in mathematics and settled some of the most challenging problems. Simplicity and elegance are the hallmarks of Selberg's work, for which I have deep admiration.

Atle was a true genius and gentleman, with classic style and exquisite taste and sense of humor. I have vivid and fond recollections of the academic year 1993-94, when I was at IAS, and the period 1995-99, when I was a junior faculty member at Princeton University and was able to have more personal contact with him. Talking to Atle was a real pleasure for me. He had lunch regularly in the Institute dining hall, and I often joined him there. We talked about a wide range of topics. His views had extraordinary insight and depth, and his conversation was dotted throughout with wit and wisdom. He liked to talk about C. L. Siegel and his friendship with the late Chinese mathematicians L. K. Hua and J. R. Chen.

In the fall of 1997 I mentioned to Selberg that Professor Chengbiao Pan hoped to invite him and Peter Sarnak to visit Peking University at a convenient time. Atle expressed great interest in such a trip. Thus, in June 1998, he and Peter visited and lectured at Beijing, as well as in Jinan and Xian. Atle seemed to enjoy this trip a lot, and after his return, he vividly described to me his pleasant, joyful experiences in China.

In Atle's Fuld Hall study I was fortunate to be able to learn some of his theories and methods directly from him. I particularly remember how, at that time, I was using his mollifier method to study the zero density of L -functions and was stuck with something. One day I spoke to Atle about this. To my surprise, the next day I found a large envelope addressed to me from Atle. In it, there was a three-page sketch of a simple and beautiful approach to overcome the difficulty. This idea turned out to be the exact key to solving my problem!

Brian Conrey

Selberg was a giant among mathematicians. We used to refer to him reverentially as "the king". I first met him when I came to the Institute for Advanced Study for a year beginning in the fall of 1982. Amit Ghosh introduced me. Amit and he were good friends, and I think that helped pave the way for me.

A highlight of that year was when Amit, Dan Goldston, and I took Selberg and Hedi to dinner one night at a fancy French restaurant in New Hope. I was very nervous driving, since the safety

Brian Conrey is executive director of the American Institute of Mathematics. His email address is conrey@aimath.org.

of one of the most brilliant mathematicians of the century was in my hands! The dinner was a success, and upon returning to Princeton, the Selbergs invited us into their home, where they showed us Selberg's Fields Medal and his collection of seashells. I returned to the Institute in 1987–88 and again in 1990–91 and enjoyed the hospitality of the Selbergs on numerous occasions. They were very friendly to the young number theorists.

After Amit and I went to Oklahoma State in 1983, Amit decided to invite number theorists from around the world to a conference in Stillwater so that they could see our new home. The first thing he did was to invite Selberg. After Selberg generously agreed to attend, Amit had no trouble organizing a most amazing conference with many of the top number theorists. At the banquet, Selberg stood up to give a toast and said that it was the best organized conference he had ever been to.

One example of Selberg's brilliant mathematical intuition that was especially exciting for me was when I first heard Selberg's lecture at Amalfi in September of 1989 (delivered by Bombieri), in which he laid out his set of axioms for a class of Dirichlet series, providing a precise list of what ingredients should imply the Riemann Hypothesis and generally describing with a simple analytic formulation what an L -function is.

Ghosh and I wrote the first follow-up paper to Selberg's preprint, and in that we named the class the "Selberg Class". What is striking about Selberg's formulation is that he got exactly the right set of axioms (down to the—at first sight mysterious— $\theta < 1/2$) to provide an analytic framework for future work. I have just heard that Kaczorowski and Perelli have established that the only elements in the Selberg Class of degree smaller than two are the Riemann zeta-function and Dirichlet L -functions associated with primitive characters. This is a beautiful theorem that I think Selberg would like to have seen. (He once told me that it would be very difficult to prove such a result.) I think it is accurate to say that this result simply would not have happened if not for Selberg setting things in motion with his axioms.

Another way in which Selberg had a profound influence on my own career was in helping to get the American Institute of Mathematics (AIM) off the ground. The initial AIM event was a workshop in Seattle called "In Celebration of the Centenary of the Proof of the Prime Number Theorem: A Symposium on the Riemann Hypothesis". After getting AIM's permission to sponsor such a meeting, the first thing I did was to call Selberg, and he kindly agreed to attend. Selberg gave the opening lecture; it was an expository lecture on events leading up to the proof of the prime number theorem. Speaking to a packed lecture hall of three hundred or so, he argued that Riemann must have known that the zeta-function did not vanish on the 1-line (a fact

equivalent to the Prime Number Theorem) but that since he was interested in an exact formula for the prime counting function, he did not write down the proof! At the end of the lecture, Selberg received a standing ovation—the only time I've ever witnessed that after a math lecture. After the meeting I was offered the directorship of AIM, which in part I attribute to the success of the Seattle conference.

I have fond memories of when Selberg and Mickey came to visit AIM in Palo Alto during the summers of 1999 and 2000. He loved to play games at lunchtime, especially bocce, and was fiercely competitive, winning more often than not. I especially cherish a barbecue at my home in Morgan Hill. After dinner we walked with our wine glasses over to the newly created golf course that will contain AIM's future home, and about fifteen of us drove golf carts around (Mickey drove Selberg) to see the various ponds and gardens on a tour led by AIM's founder, John Fry.

Selberg was always very generous to me personally, professionally, and mathematically. He was my hero and a friend; we all miss him.

Amit Ghosh

My first exposure to Selberg's mathematics came in 1977, when, as an undergraduate, I attempted to understand Titchmarsh's book on the zeta-function. I soon learned that Selberg was one of the living legends, and I hoped someday to study his papers. About two years later I was fortunate, as a graduate student, to attend the Durham Conference in Analytic Number Theory. My advisor, Heini Halberstam, was a coorganizer. Selberg was in attendance and made a striking figure, dressed in a bold bright maroon jacket, but I did not have it in me to approach him.

After the conference Halberstam informed me that Selberg said he was able to improve upon one of his results from a 1946 paper on the distribution of zeros of the zeta-function. Halberstam suggested that perhaps I should see if I could come up with a proof of my own, and a few months later I succeeded. While I was quite pleased, I was not at all sure what Selberg would think. A letter arrived soon after with the comment, "He has done a quite good job"; I thought that perhaps Selberg did not think much of my effort. I followed Halberstam to the University of Illinois in 1980 and applied for various postdoctoral positions the following year. When the Institute (IAS) called with a two-year offer, it then became clear to me that I must have made some impression on Selberg after all. This was how I met him.

Before meeting Selberg, I was familiar with his reputation of being very hard to impress, and I

Amit Ghosh is professor of mathematics at Oklahoma State University. His email address is ghosh@math.okstate.edu.

was told that, in all probability, if one had an idea, Selberg probably had had a better one thirty years earlier but did not wish to publish it. Armed with this baggage, my confidence level was not high when I arrived at IAS in 1981. However, my small success with his 1946 paper indicated to me (with my youthful ego) that even Selberg was capable of missing a result every now and then.

My first meeting was an hour-long conversation. Technically speaking, I did most of the listening and had to learn quite quickly how to converse with him: if he ever paused after a sentence, you waited, and if he looked at you, then he was done and your turn could begin (years later we dropped this system). In those two years at IAS, though I developed a rapport with Selberg which I would not call a friendship, I did notice that I was more at ease with him than were my friends and colleagues (perhaps we joked with each other more). With hindsight, I know that my generation had a much easier time interacting with Selberg, as he had mellowed a great deal with age.

In the academic year 1982–83, a group of number theorists was at IAS, and some of us began collaborating. As the end of the year approached, it was time to move on to permanent positions elsewhere. At this time, Brian Conrey and I were working on the zeta-function, and we decided to move to Oklahoma State University as part of a group of four number theorists hired by William Jaco. We wanted to bring the department to the attention of our fellow mathematicians, so we decided to hold a conference in the summer of 1984. To make this conference attractive to attend on such short notice, I asked Selberg if he would attend and if I could use his name when I approached other mathematicians. I must admit I did not expect him to agree, but he did so there and then.⁵ This was the kind of support he gave the younger generation.

After I left IAS my contact with Selberg dwindled, except during the summer months, when I made a habit of spending my summers at IAS as a visitor (at that time IAS had a summer visitor program). We would meet for lunch and talk mainly about mathematics, interspersed with history of all kinds.

At this stage I must mention Hedvig (Hedi) Selberg. To me she was a formidable presence, and I was always concerned that I might somehow offend her. While Selberg laughed at my jokes, Hedi just smiled (one never quite knew exactly what that meant). It was obvious that they were extremely close, and as the summers went by, I came to realize that it was she who made some of my summer visits to IAS possible. She told me that

⁵As the conference approached, I reminded him that the summer would be his sixty-seventh year and so he would be class number one. His response was, "It would be much better to make the next one," namely, 163.

my company was good for Selberg, as he did not interact much with other younger members at IAS. (I recall encouraging Selberg to sit with others at lunch, but he said that he didn't think they would be interested.) Soon we were having dinners out and later with my wife, Priscilla. These dinners revealed to me the personal sides of the Selbergs, and he became Atle to me. I remember a birthday party for Atle to which Hedi invited several of us who were at IAS in the summer of 1988. There was a very cheerful mood, and some of us were "discussing" various political issues. Atle joined in, and soon we seemed to be arguing on opposite sides (it was very humorous). The next day, concerned that I might have offended Atle, I was surprised to find him thinking the same toward me. I think we developed a friendship without really thinking about it. My summers at IAS came to an end in 1995 as the program was discontinued.

That last summer was a turning point in our lives. Priscilla and I met the Selbergs many times for lunch. Hedi encouraged us to see the Kirov Ballet in New York City, which we did. Before we could see her again, she was dead. This unexpected event was a great shock, yet I could only think of what it was going to do to Atle. At the memorial service a few weeks later, we heard lovely memories of Hedi. But when Atle got up to speak, it was not possible to control one's emotions. I did not know when Atle would come back to lunch at IAS, but one day he appeared and joined me. He broke down, and we talked about Hedi and his life with her. We continued to meet periodically for lunch the rest of that summer. When it was time to say goodbye, I wondered how he would endure living in that house alone. It was good that he had some trips planned, but I was concerned that his loss was going to be too hard on him.

I did not go to Princeton the next summer when our son Armand was born, but I did keep in touch with Atle. When we returned to Princeton (not IAS) in the summer of 1997, Atle had a twinkle in his eye. He had been spending some time in California, and when he returned, we decided to go out for dinner. He had not yet met Armand and wanted to stop at a toy store; after quite a while he decided on a turtle hand-puppet (Atle had a very keen interest in botany and zoology). Along the way, he told me of a new love in his life and was happy to tell Priscilla and me of "glorious days". We were so happy for him, and my worries for him disappeared. Thus began a new phase in our friendship in the coming years.

While I saw him less frequently each summer (we were now returning with our daughter, Saskia, as well as Armand), our conversations grew more personal in nature. As we got older and as he got more ailments, he invariably would wonder how much longer he would last. But Atle was not one to brood. He dealt with any setbacks with an



Crossing a fjord with Mickey Selberg, 2000.

infectious sense of humor that I loved. We talked of his childhood, his children and grandchildren, and we talked of friendships. He said that almost all his friends were really Hedi's friends, that he had difficulty making connections, and that he thought he had some autistic tendencies (we laughed, as clearly every mathematician appears to have some autistic tendencies).

In the last three years of his life, it was difficult for him to come for lunch. We would meet at his office for a bit of a chat. In 2006 I told him that I was thinking of spending my sabbatical in Princeton and that I could see him more. He said, "That would be very nice." I told him of a formula I was working on and wrote it on his blackboard; it was still there a year later when he died (we had come full circle). For the summer of 2007 my family and I were fortunate to rent a house across from the Institute lawn leading to Atle's house. I had high hopes of seeing much more of him and I told him so when we got there. He said he would be more mobile after some minor surgery and told me of a party at IAS commemorating his ninetieth birthday. I did not hear from him, but a few weeks later Peter Sarnak contacted me on behalf of Atle to tell me that Atle was not well enough to see me. I asked to visit him and then did so with Peter. He was in high spirits and thought he would recover, as did we. As I left he said, "We will spend more time next summer." He died two weeks later.

Lennart Carleson

I first saw Atle on August 30, 1950. The occasion was the award of the Fields Medals and the place

Lennart Carleson is professor emeritus at Uppsala University, the Royal Institute of Technology, and UCLA. His email address is carleson@math.kth.se.

was the Saunders Theater at Harvard during the International Congress. I was twenty-two and had just defended my thesis at Uppsala University in Sweden. My advisor, Arne Beurling, was an invited speaker, and this was my first trip abroad. The work of Atle, especially his elementary proof of the prime number theorem, impressed me very much. In contrast, it took me many more years to mature enough to appreciate the work of the other prizewinner, Laurent Schwartz.

Since that time I've always considered Atle a colleague of my teacher and regarded him with respect and admiration. He appreciated Beurling very much and actively promoted Beurling's move to IAS. They were not very close and never worked together (as far as I know), but they shared a common view of many aspects of life, in particular, of what constitutes good mathematics: hard basic problems, simplicity, and beauty.

Through the years Atle and I tended to meet mainly during my visits to IAS. Atle also spent two long periods at the Mittag-Leffler Institute. He and his first wife, Hedi, were always most helpful and generous to me and my family. We had a special relation to Hedi, as she was, so to say, almost Swedish, having lived and worked in Sweden for several years following the end of the war. I especially remember the 1977-78 academic year. The Mittag-Leffler Institute had then organized a special year in analytic number theory, and Atle spent four months there. He gave an extended series of lectures on sieve methods. It was an exciting year, with long-term visits by Paul Cohen, Enrico Bombieri, and a host of other noted mathematicians. The Riemann Hypothesis was constantly on the agenda with occasional sparkles of hope. Atle's presence was, in a way, a catalyst. I frankly wonder if ever a more serious effort to find a proof has taken place in conjunction with a scientific institute's annual program.

Paul Malliavin

I first met Atle Selberg in September of 1954 at the Institute in Princeton. Having presented Selberg's elementary proof of the prime number theorem at the University of Paris some months earlier, it was only natural that, while at IAS, I followed closely his 1955 winter course, in which he presented his famous trace formula for the first time. The audience was, as I remember, not very large. In addition to myself, it generally included Leon Ehrenpreis, Friedrich Mautner, Josephine Mitchell, her husband, Lowell Schoenfeld, and a few graduate students from Princeton University (when the snow was not too high).

Paul Malliavin is professor emeritus at the Université Pierre et Marie Curie. His email address is paul.malliavin@upmc.fr.

During the 1960–61 academic year I was invited back to IAS by Arne Beurling in connection with our common work on harmonic analysis. I had, however, a subsidiary plan to try to develop certain Tauberian-type theorems in a number-theoretic setting. On the suggestion of Arne Beurling and Jean Leray, I asked to be Atle Selberg's assistant for the year. As a result of this side program, in 1961 I published a paper in *Acta Mathematica* in which I show that the remainder term for Beurling's generalized primes has order-of-magnitude structurally similar to I. M. Vinogradov's remainder term valid in the case of ordinary primes. From this result I became persuaded that progress in classical prime distribution theory would not depend merely on some improvement in Tauberian methodology.

During 1960–61 I also discussed with Selberg some perspectives concerning progress on the Lindelöf Hypothesis. I remember that Selberg was quite convinced that a proof of the Lindelöf Hypothesis would be a decisive step toward the Riemann Hypothesis.

From these memories of the 1950s and early 1960s, Atle Selberg stands for me as a man of the utmost scientific integrity.

Nils A. Baas

I met Atle for the first time in 1972 when I went to the Institute for Advanced Study as a young member. We soon became very good friends, and my wife, Vivian, and I had a very close friendship with Hedi and Atle and also later with Mickey.

I discussed all kinds of subjects with Atle. In mathematics, it was actually an advantage for me to be in a different field, since he would then explain his thoughts in a more elementary way. Atle did not like fat books and big papers. He told me that he considered himself an amateur compared to some of his colleagues. He wanted to work on his own, penetrating problems by his own thinking and at his own pace. His knowledge outside of mathematics was extremely impressive—in botany, zoology, history, geography, and many other subjects as well—and his memory was hard to beat.

Atle had very high standards both in his professional work and in other activities. He had very clear and well-founded opinions, and when you asked him about something, you could always be certain to get his honest opinion without any unnecessary wrapping. His honesty was uncompromisable. When he, with great clarity, expressed his opinions, it was as if he was carving the message with capital letters in Norwegian granite; and when he, in his characteristic manner, raised his hand, there was no room for debate. Hedi once

Nils A. Baas is professor of mathematics at the Norwegian University of Science and Technology. His email address is baas@math.ntnu.no.



Atle Selberg in his IAS office, 2005. (Photograph courtesy of N. A. Baas.)

told Vivian and me: “There is one thing that Atle is incapable of—lying!” What a wonderful statement from a spouse.

Atle was very relaxed with respect to his own time. Once Vivian met him in the morning at the Princeton Shopping Center, and he explained why he was there: “I do the shopping, because Hedi has a job.” Atle visited our home many times in Trondheim, and he loved to be served halibut and Norwegian layer cake “heavily soaked with sherry.”

Atle was a true Norwegian. He loved Norway—the nature, the language, and the culture. He spent sixty years in the U.S., but I sometimes felt that his mind never left Norway. He visited Norway regularly, and he was a great inspiration for younger Norwegian mathematicians. We were all very proud of Atle being a Norwegian.

I remember an episode from the summer of 1974. I was a member of the Institute for Advanced Study, and we were staying for the summer. Atle was just about to leave for Norway in order to visit his brother, Sigmund, in his cabin by the beautiful Hardangerfjord. We were sitting in the common room in Fuld Hall chatting just after teatime. The temperature was in the high nineties, so was the humidity, and outside it rained cats and dogs. Atle looked out the window and commented in his characteristically cryptic way: “Actually, I prefer the rain in Norway.” To me this sounded like the deepest declaration of love for his home country that anyone could give.

After a lecture in Trondheim in the 1980s, he gave a list of problems he felt were important. I asked him then: If the Good Lord would offer him a solution to one of them, which one would he choose? “Without any doubt, the Riemann Hypothesis,” he said. “I would not like to have the complete solution, but a small clue would be nice.”

About the Cover

Atle Selberg and automorphic forms

The cover shows a framed image of a Maass form for $SL(2, \mathbb{Z})$ hanging on the wall of the office of Atle Selberg at the Institute for Advanced Study. Dennis Hejhal and Peter Sarnak, contributors to the Selberg memorial article in this issue, write:

“The theory of Eisenstein series lies at the center of the modern theory of automorphic forms. They are a fundamental tool in the theory, and understanding them has uncovered many of the deeper truths in the subject. Selberg’s very original investigation of their meromorphic continuation marked the beginning of the modern theory. In the case of the classical modular group, their closer analysis—combined with the trace formula—enabled Selberg to establish the existence of the simplest building blocks of the theory, viz., nonholomorphic cusp forms. Known also as ‘Maass waveforms’, such forms are, in the present setting, simply nonconstant eigenfunctions of the non-Euclidean Laplacian on the familiar triangular-shaped fundamental domain for the modular group. The picture on the cover is a color-coded topography plot of such a cusp form having eigenvalue approximately 15,700. It was computed numerically by Hejhal in 1992, and was one of the few decorations that Selberg placed on the walls in his office. (Very similar patterns are found with Eisenstein series.)”

—Bill Casselman, *Graphics Editor*
(notices-covers@ams.org)



Photograph by Bill Casselman.

When alone, Atle and I always spoke Norwegian. In connection with the celebration of his ninetieth birthday, Atle initially did not want any reception. Peter Sarnak and I tried to persuade him during a lunch at the Institute, but our attempt was inconclusive. A bit later I went home to Atle and suggested that a reception in his honor would be nice. First he nodded willingly, but then he switched to English and objected to the plans. I managed to get the conversation back into Norwegian, and then he agreed swiftly. Language is indeed a subtle thing.

Over the years we often discussed Norwegian literature, especially poetry. We both liked the Norwegian poet Tor Jonsson, and I remember showing one of his poems to Atle. To me, it offers a symbolic characterization of Atle’s life and work. I think it would be in Atle’s spirit to present it in Norwegian, and I offer a nonpoetic translation as well:

Å DIKTE

Å dikte er å vera
det vesle some ein vart
og sleppe kvite fuglar ut
i nattesvart

Å leva er å vera
det store som ein er
og stå i einsleg undring
og høyre fuglar flyge inn
frå ukjend verd.

TO COMPOSE

To compose is to be
the little you were created as
and let white birds out
in the dark night.

To live is to be
the great thing that you are
and stand alone and wonder
and hear birds fly in
from unknown worlds.

Atle was indeed a lonely wonderer and a great human being. With Atle gone, the world has lost one of its greatest mathematicians, Norway one of her greatest sons, and we have all lost a good and dear friend. But his spirit and work will continue to live in us and in generations to come!

Editor’s note: Unless otherwise noted, all photographs in this article are courtesy of the Selberg family.