A Conversation with Ivan Niven

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A widely known mathematical expositor, Ivan Niven is a respected number theorist who has worked primarily in the areas of diophantine approximations and questions of irrationality and transcendence of numbers. He has served as President of the Mathematical Association of America and as a member of the Council of the American Mathematical Society. In 1989 he received the MAA's Award for Distinguished Service to Mathematics.

One of the interviewers (Alexanderson) as an undergraduate at Oregon took courses from Professor Niven, specifically a sequence in applied mathematics, something well outside Niven's primary areas of interest. Those lectures were beautifully organized and delivered, as one would expect, and, though they did not convince this student to become an applied mathematician, they were highly influential in getting him to make up his mind between majoring in mathematics and majoring in French. After those courses the strong appeal of mathematics did not allow for competition.

The following conversation took place in Atlanta in January, 1988.

MP: Why don't we start with where you were born.

Niven: I was born in Vancouver, Canada, on October 25, 1915. I was one of three brothers—I'm the middle one— and we're all still alive and still have family reunions. Our parents are dead now, of course. I was educated in a school system that would be regarded as elitist nowadays. For example, if a student was good the school would promote him or her right past a grade. I was shoved past a couple of grades in elementary school and I don't recall missing anything. Everything seemed to fit in, so when people say you can eliminate a lot from the usual curriculum I believe that's so.

MP: At what age did you graduate from high school?

Niven: I was 14. I was going to be 15 in October. But there were three or four or five other students that young as well.

MP: How big was the school?

Niven: It was the biggest in Vancouver, John Oliver High School, with 1,600 students or so. It was an academic high school. There were in Vancouver at that time, academic high schools, commercial high schools, and a big technical high school. When you finished grade school you had to take government examinations to determine whether you could go on to the academic high school, which was the path to university. In the academic high schools of the province of British Columbia at that time there was a standard college preparatory curriculum,

Ivan Niven

essentially the same for all students. The underlying rationale for the system, which has a lot of merit, was that the educational leaders know better than the students about what they should study. In the academic high school I attended, the best students in each grade were assigned to the so-called "star class," in which all topics were studied in greater depth.

MP: Were there any clear-cut interests that you had discovered by the time you graduated from high school?

Niven: Well, I think that I was told I should have an interest in mathematics because it was easy for me.

Scottish Roots

MP: Were there any mathematicians already in your family?

Niven: No, my father was a machinist. I suspect that my mother might have been —well, I won't say might have been a mathematician, but she certainly could have got a bachelor's degree in mathematics with ease.

MP: So you got your mathematical talent from your mother?

Niven: It's quite possible.

MP: What were her roots?

Niven: Her father was a coachman in Scotland. Both of my parents came from Scotland, from the working classes. My father wound up in Vancouver as one of three fellows who owned a factory that made stoves and furnaces. None of his sons wanted to go into the factory because of more attractive opportunities elsewhere.

MP: So neither of your parents had a college education?

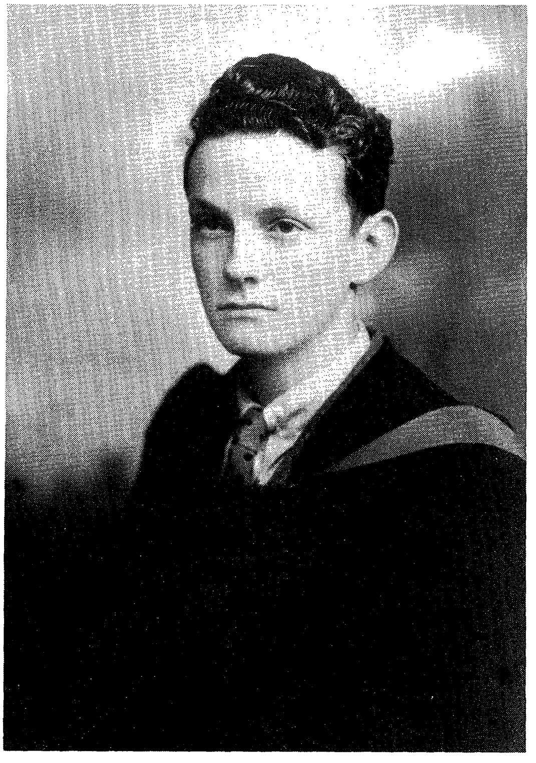
Niven: No, after completing the eighth grade, my father was expected to go to work to help support the family. This was in Scotland in the 1890's.

MP: But your parents were supportive of your educational efforts?

Niven: Oh, very much so. My mother had gone to high school for two or three years. I think my father was completely capable of having gone to high school and probably college—later in life he read serious material far above the eighth grade level—but higher education just wasn't in the cards in those times. Universities were not very accessible to the working classes in Great Britain before the First World War.

MP: What about your brothers?

Niven: Both of them also graduated from the University of British Columbia. My older brother went on to get a Ph.D. in biochemistry. My younger brother became a high school principal, after teaching mathematics. Mathematics was easy for all of us.



Niven, the young University of British Columbia graduate in 1934

MP: It raises questions, doesn't it, about the British class system? How much talent must have been lost when so many children of working class backgrounds did not get to go to the university!

Niven: Yes, I went over to Scotland in the 1950's and met a number of my cousins. I was surprised to see how many of them were accountants. They had not gone to university, but accounting was an avenue that was open to them. They were not just bookkeepers. Here we would call them certified public accountants, CPA's. There they called them chartered accountants, which means they had taken the official exam.

MP: So there was evidence of mathematical genes in the whole Niven clan.

Niven: I think so.

No fish story! Niven caught this beautiful salmon off Winchester Bay, Oregon.

Do Mathematicians Fish?

MP: Do you have any particularly vivid memories of growing up in Vancouver?

Niven: Oh yes. Vancouver was a great place to grow up because of all the water and the mountains, the ocean, the Fraser River. We could go down to the river or the ocean on our bicycles.

MP: Was it there that you developed your taste for fishing?

Niven: No, not really. I became a fisherman later, in Oregon.

MP: I was once told by someone—I'm sure on very little evidence—that mathematicians don't fish.

Niven: Well, when I was in Oregon it occurred to me one day that I could just as well be in the middle of Iowa or Nebraska. I wasn't skiing. I wasn't doing any of the things that people in Oregon can do that you can't do if you live in a flat country like the middle west. So I decided I would go fishing on the ocean. If the ocean is rough, I don't mind. I don't get seasick.

MP: At the University of British Columbia was there any special professor who was important for you?

Niven: Frederick Nowlan. He watched for the capable students and encouraged them. He would give out special problems, and if you solved those he would give you more special problems. When I was a junior, Nowlan was giving a course in the theory of equations. We were using a book by Dickson called *Elementary Theory of Equations*, I believe. There is a chapter in that book that gives a proof of the fundamental theorem of algebra. (It's one of the old proofs of Gauss.) You want to prove that a certain equation $f(z)=0$ has a solution, where z is a complex number. So you plug in $x+iy$ for z and then expand the thing and separate out the real and imaginary parts. That gives two equations, say $g(x,y)=0$ and $h(x,y)=0$. Now you have to establish that the graphs of these have an intersection point. As you can imagine this is pretty much a mess. Well, Gauss did it and Dickson reproduced it. Nowlan didn't take up that chapter in class, but he assigned to me the task of reading it. When I finished he met with me late one afternoon and got me to explain, without notes, the nature of that long proof. He would ask questions as I went along to make sure I understood what I was talking about. In the book the proof takes up seven pages. Well, it was rather fun to do that. It was Nowlan's way of encouraging you.

MP: What was his own field?

Niven: Algebra. The field of another professor, Buchanan, was analysis. Both Nowlan and Buchanan had taken Ph.D.'s at Chicago and they had sent quite a few students there. They would send only very good students. When I got to the University of Chicago I was told that the Department of Mathematics looked favorably on anybody British Columbia recommended because they could rely on the recommendations of Nowlan and Buchanan. I wrote my Master's thesis with F. S. Nowlan in matrix theory, and presented it at a meeting of the American Mathematical Society on June 18, 1936, held as part of the 75th anniversary celebrations of the University of Washington in Seattle. In that talk, I extended some work of M. M. Flood, a Ph.D. graduate of Princeton in 1935. Flood had solved a certain problem in "associated" matrix polynomials, giving necessary and sufficient conditions. Details on my talk are available in the *Bulletin of the A.M.S.* in 1936, pp. 486 and 601.

Chicago—Almost No Finals

MP: Was Chicago something of a culture shock after Vancouver?

Niven: Not really. I had already taken a master's degree, and at the University of British Columbia that was quite a solid degree. I had, for example, gone practically all the way through Whittaker and Watson's *Modern Analysis*. I had had a course in real variable theory and a course in complex variable theory. I had read Dickson's *Modern Algebraic Theories* and his *Algebren und ihre Zahlentheorie*, a translation of the English version, enlarged and improved. I had also read number theory, including some of Landau in German and also Hecke's *Algebraic Number Theory* in German. Foreign language exams were no problem because both at the

University of British Columbia and the University of Chicago, we read books in the original languages since there were no adequate textbooks in English in many subjects. The University of British Columbia followed pretty much the British system, with rather severe examinations. Before I went to Chicago in 1936 I had been told, "Now this is going to be the big time; you're going into the big leagues." So I worked very hard. Then I was a little bit disappointed because there were almost no final exams; in fact there was only one professor, the geometer E. P. Lane, who gave exams at the end of the term. The other faculty members would just lecture and then at the end they would say, "Fine, see you next term." It was the German system, brought in there by Maschke and Bolza.

MP: But both of them must have been long gone by the time you got there.

Niven: Yes, but their influence was still there. There were no qualifying exams, and no prelim exams. I took a final exam in mathematics, a French exam and a German exam. And I took exams at the ends of the quarters in Lane's courses in geometry. That's all.

MP: Who were some of the people you came in contact with during your first year?

Niven: Right away I took courses with [Leonard Eugene] Dickson, [Gilbert] Bliss and [A. Adrian] Albert, the algebraist. I remember that Albert gave me a paper by another author to read for class presentation. I found that the proof of one of the theorems was flawed. I thought, "I should tell Albert. Maybe this isn't the paper he wants to give me." Then I decided, "Oh no, Albert knows there is a flaw in the proof." So I figured that what he wanted me to do was straighten the proof out if I could or give a counterexample. It turned out that the theorem was correct and I could straighten out the proof. When I gave this talk in class, I first presented the paper just as it was, flaw and all. Then I drew attention to the incorrect argument, and went on to give an accurate proof. At the end of the class I handed Albert his reprint. He gave me a pat on the shoulder and said "Just fine. Just fine." Then I knew that I'd passed the test with Albert. A similar circumstance occurred with Dickson and with Bliss, in the sense that each of them asked me to study certain topics or to solve certain problems and then present them to the class.

Dickson—Bridge, Classes, and Work

MP: Which of the three was most colorful?

Niven: Probably the most colorful fellow was Dickson. He lived his life in the following way. He would come to the campus about 10:00 in the morning to play bridge with some cronies over in the Faculty Club. Then he would have lunch and again get back to bridge. Next he had this class from 2:30 to 3:30 four days a week. Then he went back to the Faculty Club. Theoretically he had an office hour but unless somebody was standing right there he immediately left. The Faculty Club—the Quadrangle Club—was just a block from Eckhart Hall. So he scooted over and played more bridge. Then he went home and had dinner with his wife.

A sketch of L. E. Dickson in 1920. Dickson was Niven's thesis advisor and "probably the most colorful fellow" at Chicago.

He came back in the evening about 7:30 or 8:00 to Eckhart Hall. We could always see his light on. He'd work there from 8:30 till about 1:30 or so and then go home. In the evening he turned out a tremendous amount of work, but in the daytime it was bridge and one class. That was his life.

MP: A kindly sort?

Niven: No, rather gruff, a very gruff person at that point in his life.

MP: Any examples of his gruffness?

Niven: If you went to see him to ask to write a Ph.D. dissertation with him, he would say, "I'm going to give you a preliminary problem that is shorter than a dissertation problem and if you can solve this within three months, then I'll talk about a dissertation problem." He had a file in his office of problems he had solved but didn't think important enough to publish. So he would give you one of these. He knew how to do it. Some students didn't like this at all. If you didn't solve that preliminary problem there was no second chance with Dickson. It was sudden death. If you failed, you had to ask some other professor for a dissertation problem. But I liked his system. "If you do this problem, then I'll take you on. If you can't, goodbye." I believe I was his last Ph.D.

MP: Was the Ph.D. exam then a defense of your thesis?

Niven: Partly. But if you wrote a dissertation, say, with Dickson, what was there to defend? After all the professor had signed the thesis and he would not have signed it if it was indefensible. All that they wanted was to make sure that I was the one who had written the thesis.

MP: Which they did with a few pointed questions?

Niven: Yes. My Ph.D. exam was scheduled one morning from 9:00 to 12:00. That's all, just three hours of oral examination. *Some* of it was on the thesis, but for the most part the questions were in the four required fields: abstract algebra, analysis, geometry/topology, and applied mathematics. There would be people appropriate to these areas on the committee. For instance, [Arthur] Compton came to my final exam and asked me some questions on the physics of radiation, especially X-rays and cosmic rays, and then went away. Everybody deferred to him, a Nobel laureate. All the faculty examiners in mathematics seemed to follow the same pattern in their questioning: start with an easy question and then raise the ante with successively harder questions until the candidate is having trouble. The examiners didn't allow you to linger on questions you could handle. As soon as they realized that you were on firm ground, they moved you on, promptly but politely, to a tougher question. On reaching a question that gave you trouble they asked you to explore it; how would you go about examining such a problem? Since I was spending so much time struggling with the top problem in each chain of difficulty, I thought I was doing rather badly. But I passed the exam, with hearty congratulations from all the examiners. I felt beaten to a pulp.

MP: But Dickson was apparently a good teacher for you. Why?

Niven: I think it was because he was challenging. I liked his mathematics, not only his number theory. Dickson gave a course in the Lie theory of differential equations when I was there. I took all his courses.

MP: But you were not that far away from some of the things that Albert was doing.

Niven: That's right. I could have been attracted to Albert. He was not a very good lecturer, but he was a gem in the rough, so to speak. Very quick, very direct, and very outspoken to the point that some people just didn't like him. I didn't mind that. I thought he was fine.

MP: But were you never attracted to Bliss and the calculus of variations?

Niven: No, I got the impression that it was a heavily worked field in need of some new approaches. The students at Chicago knew that Marston Morse and his group at Harvard were developing the calculus of variations in the large, but this was not in Bliss's program. Furthermore, my background in current work in algebra and number theory was stronger than in analysis. I knew a lot of *classical* analysis: real and complex variable theory, the theory of functions, as well as the special functions of mathematical physics along with their partial differential equations. But when I arrived in Chicago, I knew almost nothing of functional analysis, integral equations, Fourier analysis beyond simple trigonometric series, and modern theories of integration. I took courses in these topics from L. M. Graves, W. T. Reid and E. J. McShane (a visitor from Virginia).

Lawrence M. Graves and E. J. McShane, two of Niven's teachers at Chicago.

MP: How long were you at Chicago?

Niven: Two years. I came in 1936 and got my Ph.D. in 1938.

MP: So you were twenty-three when you got your Ph.D.?

Niven: Well, I was actually twenty-two.

MP: You certainly flew through.

You've solved a lot of problems since then. How do you solve problems? How do you attack them? Can you tell us your secret?

Niven: That's a difficult question. At an elementary level, Pólya's book *How to Solve It* gives a good explanation of ways of approaching problems, using analogy, auxiliary problems, symmetry if you're working in geometry, induction in algebraic and number theoretic situations, and so on through his whole catalog of devices. But the mystery remains, why some students can catch on so quickly, whereas others are much slower, and others again are frustrated. We can say it's due to individual differences, but that's a description, not an explanation. I had friends in college who were topflight students in areas like history and English, and who were quick and sharp in debate, but very slow at mathematics. Earlier than that, back in grade school, I recall a teacher asking the class to write the latitude and longitude of the antipodal point to Vancouver on the earth's surface, the point reached if we could drill a hole down to the earth's center and out the other side. We had been given the latitude and longitude of Vancouver, as well as many other examples. Some students had a terrible time with the question, especially the longitude part.

MP: Now that's a visual problem. But your field of number theory is one that a lot of people would say is not particularly visual. So how do you think about problems in number theory?

Niven: I look for connections. I look for patterns. Also, for many years I have tried to see what lies behind the results in the literature. That's what Gauss was trying to do, I believe, when he came back to the Gaussian Reciprocity Law time after time, so that ultimately he had seven or eight different proofs. I suspect Gauss was trying to find the meaning behind the result. You recall the result, which concerns two congruences $x^2 \equiv p \pmod{q}$ and $x^2 \equiv q \pmod{p}$, where p and q are distinct odd primes. If both primes are of the form $4k + 3$, then one congruence is solvable and the other is not. But if one or both of the primes is of the form $4k + 1$, then either both congruences are solvable, or neither is. Vague as it sounds, this theorem opens the door to quick methods for solving numerical quadratic congruences. About three or four years ago I heard Basil Gordon give a talk with the title *Yet Another Proof of the Gaussian Reciprocity Law*. So we're still trying to understand what makes it work.

If you can see what lies behind something, you're getting a real grasp on it. Take the theorem of Pythagoras for example. To me, the idea behind this is that the areas of similar figures in the plane are proportional to the squares of the lengths of corresponding distances on the figures. Thus if you draw the altitude of a right triangle to the hypotenuse, there are three similar triangles to be seen, and the theorem is rather obvious. The same idea lies behind many other theorems in geometry.

To take another example, the idea lying behind the little Fermat theorem that $a^p \equiv a \pmod{p}$ holds for any integer a and any prime p, also lies behind the generalization of this by Euler to a general modulus, as well as to the Lagrange theorem that the order of an element of a group is a divisor of the order of the group. The idea is the pigeonhole principle, that if $n + 1$ pigeons fly into n holes, at least one hole contains two or more pigeons. Furthermore, that principle lies behind many other results in number theory and combinatorics. There are far fewer ideas and principles in mathematics than there are theorems. Accordingly, without slighting the theorems, I try also to see the ideas behind them. Even at the undergraduate level, as I progressed through college, the mathematics professors seemed not quite as clever as I had thought initially. I was slowly developing a more sophisticated view, as I believe all budding young mathematicians do.

"I'd Spent My Last Dollar"

MP: But when you were given a Ph.D. problem, based on what you've told us about Dickson so far, this must have been very difficult. You got stuck initially, certainly. So what did you do?

Niven: I tried every device and every idea I knew, as well as combinations of them. I read the available papers of the Indian S. S. Pillai, who had solved the Waring problem independently at the same time Dickson did it. Their work was based on some analytic estimates by the Russian, I. M. Vinogradov, who had improved on the Hardy-Littlewood arguments. So naturally I reviewed Vinogradov's results to see if Dickson and Pillai were getting the full strength of those results; they were, so there was nothing to be gained there. One way or another I did manage to solve the problem that Dickson had posed.

Let me say a little more about how we solve problems in mathematics. As you know there is the matter of finding the right problems to work on, and beyond that the creation of new theories. Jacques Hadamard attempts to explain these matters in his book *The Psychology of Invention in the Mathematical Field*. Hadamard had the credentials to write on that subject, since he, and [Charles] de la Vallée-Poussin independently, were the first to prove the famous prime number theorem.

Prime Number Theorem. One of the great achievements in number theory in the late nineteenth century was a proof of this famous theorem that gives some estimate on the frequency of the primes in the sequence of positive integers. It states that if $\pi(x)$ is the number of primes that do not exceed, x, then

$$\lim_{x \to \infty} \frac{\pi(x)}{x/\log x} = 1.$$

The theorem had been conjectured, but not proved, by Gauss somewhat earlier. New ideas had to be introduced to establish that result, so the work was truly creative. Many papers in the literature are derivative, in the sense that no really new ideas are introduced. This is not to denigrate these papers; known ideas have to be adapted and combined in novel ways, and that's not easy to do. Hadamard, like everybody else, gives necessary but not sufficient conditions for creativity. So did the great biologist Louis Pasteur, when he said that chance favors the prepared mind.

After I was finished at Chicago, by the way, I started meeting people from other universities, like Princeton, for example, where they were proud that they weren't given problems. They were supposed to find their own problems. Well, I thought these other people had a fancier, more sophisticated system. Then I discovered, on talking to more and more of these people, that they didn't really discover any problems. Mostly it had been something that was tossed out by the professor in a course. They picked it up and then talked to the professor about it. After all a student at that stage is not in a very good position to decide on a problem. You could find one, but how do you know that it hasn't been solved already in the literature? That's what the professor's there for. The professor knows the literature very, very well. In *Mathematical People*, Olga Taussky-Todd talks about Vienna and the fact that the professor just said, "Well, we'll work on class field theory," which was just in its beginnings at that time. So she read whatever she could and there wasn't much to read. She got more and more desperate because she couldn't find a problem. She had a hard time and I think it cost her a year. It isn't so much that it made her a year older. The cost of a year is financial. I got my Ph.D. in 1938 when money was in very short supply. When I finished my Ph.D., that was it: I'd spent my last dollar.

MP: Just for the record, what was a fellowship in those days?

Niven: A fellowship paid $600 at first and later $700, but they took $300 back in fees at the University of Chicago. So in effect I got $300 and $400 per year, the equivalent of almost $3000 to $4000 today. I did not especially want to work in cafeterias and things like that. My father, although he'd never been to university, didn't think you should have to do that kind of thing. He didn't know what college was exactly, but he said, "It is a full-time job, isn't it?" I told him that you're supposed to do two hours of work outside class for each hour in class. He said, "Well, you take 15 or 16 class hours and that's 48 hours in all. That's a good work week, sure. Well, I don't think you should have to work otherwise." He wanted us to work in summertime to help contribute to our education, but not in the course of the academic year.

MP: One last thing on your dissertation. Dickson gave you this sort of test problem to find out whether he would take you on as a student, and you were pretty sure he had already worked through the problem.

Niven: He told me that he had.

MP: That leads to an obvious question, of course: do you think he had worked through a lot of the dissertation problems that he presented?

Niven: I don't know. I do know that later when I gave dissertation problems I looked down the road a little bit to see if it was possible to solve them.

MP: For your dissertation you solved one of the last steps in the Waring problem.

Waring's Problem. This problem, of which there are several variations, asks in its most basic form whether there is a positive integer $g(k)$ corresponding to every positive integer k such that every positive integer n is a sum of at most $g(k)$ positive kth powers. The simplest case of interest is the theorem that every positive integer can be expressed as the sum of at most four squares. That such a $g(k)$ can be found was first proved by David Hilbert in 1909, but his proof did not indicate how $g(k)$ could be calculated for a given k.

Niven: No, I did that soon after my Ph.D. My dissertation was concerned with a variation of the Waring problem with different summands. It was sufficiently different that I had to do some mildly creative work to settle the questions. Then, following my Ph.D., I pursued the problem you mentioned, one of the last steps in the Waring problem. This work is mentioned in Newman's four volumes, *The World of Mathematics*—I'm in the index—in some remarks by E. T. Bell about international cooperation in mathematics. It brought me some genuine attention early in my career. For example, a professor of law drew the Newman reference to my notice.

MP: So Dickson's problem opened the way to additional problems. Some dissertation problems are dead ends. You solve the problem and you have nowhere to go.

Niven: Apart from the item I just mentioned my dissertation problem was a dead end for me. However, mathematicians have to break away sooner or later from the area of their dissertations. The postdoctoral year with Hans Rademacher at Pennsylvania helped me a great deal.

MP: Before we go on to Rademacher and Pennsylvania, let's continue with Dickson and Chicago.

Niven: Dickson had been an algebraist earlier on, but by the time I was in touch with him at Chicago, he wasn't doing algebra any more; he was doing just number theory. As I said earlier, mathematics and bridge were Dickson's life. He was at loose ends in retirement, when he dropped out of mathematics so as not to spoil a good record with weaker papers, "as a couple of my friends have done."

For his distinguished work in mathematics, Dickson was given an honorary doctorate by Harvard at its tercentenary celebration in 1936. I asked him if that was the greatest honor he had ever had, and he said no, the honor that had given him the most satisfaction had come many years earlier. Just before he was to present a paper at a meeting in Paris, the young Dickson was writing on the blackboard the statement of his principal result. One of the great French mathematicians, Camille Jordan if I remember correctly, came to the board and asked, "You have proved that?" Dickson said yes, he had. The old Frenchman shook his head and remarked wistfully, "I tried so hard to prove that."

"20-20-20"

MP: What makes number theory so attractive to you?

Niven: I have a theory that for a lot of people, certainly for myself, a number of areas of mathematics could have been attractive. This area was attractive to me because Dickson was there; that was an opportunity. If I'd gone to another school it could have been another subject and another professor. I liked analysis when I was a student. I liked the course in topology we had, but there was no possibility of taking a Ph.D. in topology at that time at Chicago. Once I started on number theory I had no special reason to shift fields.

When I was at the University of Chicago, the city was a big railway hub. Commercial air travel was in its infancy. The University of Chicago was very accessible; a meeting of the A.M.S. was held there every spring in those days. I heard many distinguished lecturers: G. H. Hardy, C. Carathéodory, Emil Artin, Norbert Wiener, Saunders Mac Lane, Tibor Radó, Bengt Stromgren from Denmark, Vijayaraghavan from India, and W. Hurewicz of dimension theory fame, to mention a few. Earle Raymond Hedrick came through town. G. Baley Price and A. S. Householder were summer visitors for extended periods. It was a great environment, second only to Princeton and the Institute.

Unless I've changed, the talks were more comprehensible than the talks today. That may be because mathematics has advanced and gotten more abstract. It's more difficult today to make clear what you're talking about. But it seemed to me in those days that people talked on a sort of 20-20-20 basis; they would spend about 20 minutes or one third of the time giving a general setting; then for about 20 minutes they would talk to the specialists in the field; and then the last 20 minutes they would talk about their own work, at which point it seemed sometimes they were talking just to themselves and God. But nowadays you often just get that last one-third. Sometimes you get the last two-thirds, but the general audience is not taken care of much at all in these times, except by certain lecturers like Paul Halmos, Peter Hilton, Peter Lax, Saunders Mac Lane and others.

MP: Did these visiting lecturers get instructions from Bliss?

Niven: He ran the seminar. I don't know if he made all the arrangements, but I think he must have made very clear to the visiting speakers that since the audience was very broad there should be a certain amount of clear exposition at the start. If that did not happen, Bliss would actually ask the speaker questions right away within the first five minutes. I can still remember his saying, "But wait, you've lost me already." In spite of the rather formal setting, Bliss would speak right up. Bliss was such a distinguished figure, a member of the National Academy and head of the department, he could get away with it. Department heads in those days were autocratic figures. His persistence was a great boon to the graduate students.

MP: Of all the mathematicians you heard in those days, who was the most exciting lecturer you ever encountered?

Niven: I would say Saunders Mac Lane or Emil Artin. It would be hard to decide between those two. Their lectures were of the sort that you went away feeling that you'd understood all that they'd said. Of course, if you tried to work out the details, especially of the last part, you'd find it didn't work out quite as easily as the way you thought you had understood it.

MP: I sometimes get that feeling after listening to Paul Halmos.

Niven: So do I. I didn't mention Halmos because he was not yet a famous lecturer. He took his Ph.D. in 1938 and so did I.

A Famous One-Page Proof

MP: What kinds of problems did you work on?

Niven: Let me mention two specific items that are easy to comprehend, in addition to my work on the Waring problem mentioned earlier. In the June 1947 issue of the *Bulletin of the A.M.S.*, I gave a one page proof that π is irrational. I had worked on this problem for a specific reason: in the first edition (1938) of what is now a great classic, *Introduction to the Theory of Numbers*, by G. H. Hardy and E. M. Wright, the authors made the observation that "There is no simple proof of the irrationality of π." I wondered why this should be so. Following the publication of my proof, Hardy and Wright replaced that observation with a simple proof in subsequent editions of their book.

Let me mention another work that drew a lot of attention. Consider any irrational number β and the sequence $S(\beta)$ of its multiples, $\beta, 2\beta, 3\beta, \dots$. Each of these multiples has a fractional part and an integral part; for example, the fractional part of 10π is $0.4159\dots$ and the integral part is 31. Herman Weyl had proved that the fractional parts of the numbers in the sequences $S(\beta)$ are uniformly distributed in the unit interval, meaning that each subinterval gets its expected proportional share in the limiting sense. For example, if the subinterval from $1/4$ to $1/3$ contains k of the fractional parts of the numbers $\beta, 2\beta, 3\beta, \dots, n\beta$ then the limit of k/n as n tends to infinity is $1/12$.

It occurred to me that the integral parts of the numbers in the sequence $S(\beta)$ must also be uniformly distributed in some sense. But in what sense? There was no definition of what it would mean that an infinite sequence of integers is uniformly distributed. I introduced the rather natural definition that a sequence of integers is uniformly distributed if each of the residue classes modulo m contains its expected proportionate share of the integers in the sequence, again in the limiting sense. Furthermore, it is required that this property should hold for every modulus

A SIMPLE PROOF THAT π IS IRRATIONAL

IVAN NIVEN

Let $\pi = a/b$, the quotient of positive integers. We define the polynomials

$$f(x) = \frac{x^n(a-bx)^n}{n!},$$

$$F(x) = f(x) - f^{(2)}(x) + f^{(4)}(x) - \cdots + (-1)^n f^{(2n)}(x),$$

the positive integer n being specified later. Since $n!f(x)$ has integral coefficients and terms in x of degree not less than n, $f(x)$ and its derivatives $f^{(j)}(x)$ have integral values for $x = 0$; also for $x = \pi = a/b$, since $f(x) = f(a/b - x)$. By elementary calculus we have

$$\frac{d}{dx}\{F'(x)\sin x - F(x)\cos x\} = F''(x)\sin x + F(x)\sin x = f(x)\sin x$$

and

$$(1) \qquad \int_0^\pi f(x)\sin x\,dx = [F'(x)\sin x - F(x)\cos x]_0^\pi = F(\pi) + F(0).$$

Now $F(\pi) + F(0)$ is an *integer*, since $f^{(j)}(\pi)$ and $f^{(j)}(0)$ are integers. But for $0 < x < \pi$,

$$0 < f(x)\sin x < \frac{\pi^n a^n}{n!},$$

so that the integral in (1) is *positive*, *but arbitrarily small* for n sufficiently large. Thus (1) is false, and so is our assumption that π is rational.

PURDUE UNIVERSITY

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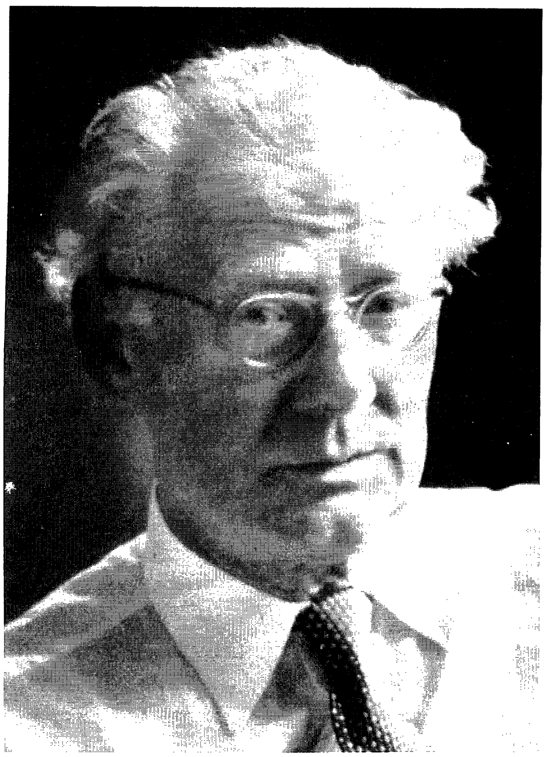
$m = 2, 3, 4, \ldots$. For example, for the modulus 2 this means that half the integers in the sequence are even, and half are odd, in the limiting sense.

In a paper in the January, 1961, issue of the *Transactions of the A.M.S.* I proved that the integral parts of the terms in the Weyl sequence $S(\beta)$ are uniformly distributed. Furthermore, this result is equivalent to the theorem of Weyl: each implies the other. I believe that this 1961 paper can be called seminal, because it stimulated a small stream of papers exploring other aspects of uniformly distributed sequences of integers.

Driving Lessons for Mrs. Rademacher

MP: Now to go back to the University of Pennsylvania and Rademacher—was he a nice guy?



Niven spent a postdoctoral year studying with Hans Rademacher at the University of Pennsylvania.

Niven: Quite a nice guy, more friendly than Dickson. Also, whereas Dickson was a specialized intellectual, Rademacher had a strong interest in literature, science, art, and music. He was formal and aloof in the standard European professorial style of the pre-World War II era, but all in all a kind and generous man. Rademacher did not have a very high opinion of American culture and American education. He did not say that directly but, working with him and being around him a lot, you realized how he felt about these things. And I got the impression pretty early on that he did not think much of the American Ph.D. degree, so whenever an opportunity arose to point out to him that a proof of his was unnecessarily long, I did that. I found that Rademacher did not know a great deal of abstract algebra. He knew what a group was and a ring, integral domain, a field, and so on, but he hadn't really immersed himself in it. I noticed occasionally that you could use a little abstract algebra to shorten his proofs in his lectures on function theory, so I would draw this to his attention. Afterwards he started behaving differently toward me, although that might have been just in my own mind.

MP: Do you think he might have found what you did embarrassing?

Niven: Well, yes, to some extent. When I'd start to explain something to him, I'd have it written out on a sheet of paper for him and he'd say, "Oh, thank you" and he would take it away. He didn't want me to explain. Then he'd study it and a couple days later he'd say, "That's very good, that's very good. I've incorporated it in my notes. I'll do it that way."

MP: Was he entirely of this world?

Niven: No. Physical activity and physical things, apart from works of art, were of secondary importance. He couldn't drive a car. Now not all of the European intellectuals were like him. But of the Europeans I met in those days, almost none knew how to drive a car. Rademacher owned a car but somebody else drove it for him. I offered to teach him how to drive and he said, "Oh, thank you. Let me think about that." The next day he said to me, "You offered to teach me how to drive. Well, I don't care to drive, but would you teach Mrs. Rademacher?" So I taught her how to drive.

MP: It sounds as if you got along pretty well with him, if he was willing to have you teach his wife how to drive.

Now when you went to Pennsylvania we were still in the depths of the depression. Jobs were not very plentiful in those years. You were also aware that European mathematicians, very distinguished European mathematicians, were already hitting our shores, which could only make your job prospects worse.

Niven: Yes, right. When I went to Illinois, for example, Reinhold Baer and Olaf Helmer were already there, two emigrés.

MP: You had an offer at the University of Illinois when you chose to take the postdoctoral appointment at Penn in 1938. Was the job at Illinois kept open for you from 1938 to 1939?

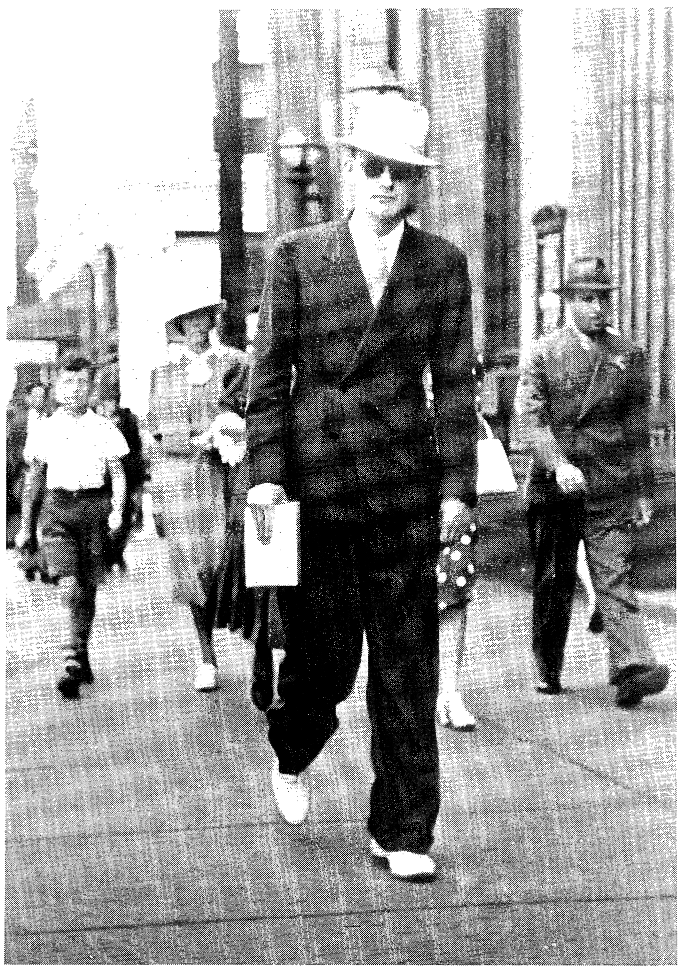
Niven: I don't know if that special position was kept open. When I said "No" to the job in 1938 I wrote in a very, very apologetic way saying that it was a magnificent opportunity but I really felt that a postdoctoral year would help me a good deal with my work. But I kept in touch with the department head at Illinois.

MP: You had to be a pretty confident guy to do that, to turn down a job in the middle of the depression for another year of graduate studies.

Niven: Well, I guess I was fairly confident. In 1938 when I had the offer from Illinois, I also had an offer from Reed College in Portland. So I had reason to feel fairly confident. On the other hand, Professor E. P. Lane of Chicago recommended to me in 1938 that I should take the Illinois job rather than the postdoctoral fellowship on the grounds that all the benefits of the latter could be lost at once if in 1939 I had to take a job at a school with an inadequate library and a poor atmosphere for research. That scared me, but I ran the risk.

Experiences of the Emigrés

MP: You were confident you would get a job even with the flow of immigrants into the United States?



A white hat and white shoes were part of a very dapper Niven in 1939 on the streets of Vancouver.

Niven: I was confident that in 1939, after my postdoctoral year, I would find a position in some college or university, but not necessarily a position at as good a school as the University of Illinois in Urbana. This was the risk. As you say, the emigrés were flowing into the United States from Europe, dozens of world-class mathematicians like Courant, Max Dehn, von Neumann, Pólya, Szegő, Rademacher, Herman Weyl, and others. They were tough competition. I assumed, somewhat incorrectly, that they were virtually no competition at all because they would want professorial positions at high salaries, whereas I would be looking for an instructorship at an entry level salary. What I didn't realize was that the emigrés coming in from Europe had to take whatever jobs they could get, at whatever salary. The department heads had very few professorial salaries at hand for the hiring process. They had more instructors' salaries, and not too many of them, because the depression was still disconcertingly present.

Max Dehn took a post at $1200 for the academic year in 1941 at Idaho State College in Pocatello, Idaho. He had fled Germany by going east. He came on the Trans-Siberian Railroad to the Pacific. I've never been able to find out how it was that Max Dehn made contact with a place out in the sand and sagebrush, Idaho State in Pocatello, but he did. Now the department heads did not like to hire people at $1,200 or even at $1,800, which was the going rate for beginning faculty members. They didn't like to do that for the simple reason that it would embarrass them since the person obviously was deserving of more.

When I went to the University of Pennsylvania in '38-'39, Rademacher was an assistant professor, a position he had held there since 1934. He had been a full professor in Germany, but he lost his job because of anti-Hitler activity. Another university in the United States tried to lure Rademacher away from Pennsylvania with an offer of a full professorship in the spring of 1939. This episode taught me a lot about the internal financial operations of universities. To meet the outside offer, which carried a whopping raise from an assistant professor's salary to that of a full professor, Pennsylvania would have had to give Rademacher almost all of the raise money that had been allotted to the entire Department of Mathematics. This didn't sit too well with the other members of the Department. After much pulling and hauling the Administration of the University managed to turn up some other money, so Rademacher stayed there with a promotion to a full professorship, and the other faculty members got their raises too.

MP: When you were at Pennsylvania, number theory was Rademacher. When you went to Illinois in 1939, who was number theory?

Niven: Well I guess you'd have to say that Carmichael was number theory, but Carmichael by then was dean of the graduate school so he wasn't around very much. In fact, Carmichael invited me to share his office. He had a big, nice corner office. I hardly ever saw him in it.

MP: You must have some interesting recollections of G. A. Miller, too.

Niven: I knew him fairly well because he lived not far from where I lived the first year, so often I would walk over to campus with him and back home at noon for lunch. He was wealthy, but nobody knew it. He had married a Busey.

MP: Of Busey National Bank?

Niven: Yes. The story was that he used \$30,000 or so of his wife's money to invest in the stock market. That was quite a bit of money in the depression days, because a full professor in those days normally made only \$4,000 to \$5,000 a year. I understand he never bought stock that paid dividends, because it obviously wasn't depressed enough in his opinion. He spread his money around and bought lots of equities. Some of his stuff became worthless paper, but what came back came back very handsomely. He left essentially \$1,000,000 to the University of Illinois.

MP: We have him to thank for the *Illinois Journal of Mathematics* and what may be the second best mathematics library in the country.

Niven: That's right, although the library was already quite good even when I was at Illinois. The reason was that at the end of every biennium the money that had not been spent would revert to the state. So instead it would be turned over to the library and the library would buy books. Well, the Mathematics Department was on its toes on this and it would have its lists of books right there. The library had to act fast because everything had to be spent before July 1 when the new biennium started. So Illinois had a splendid library, but the G. A. Miller Trust helped to make it even better.

MP: How many years were you at Illinois?

Niven: Just three.

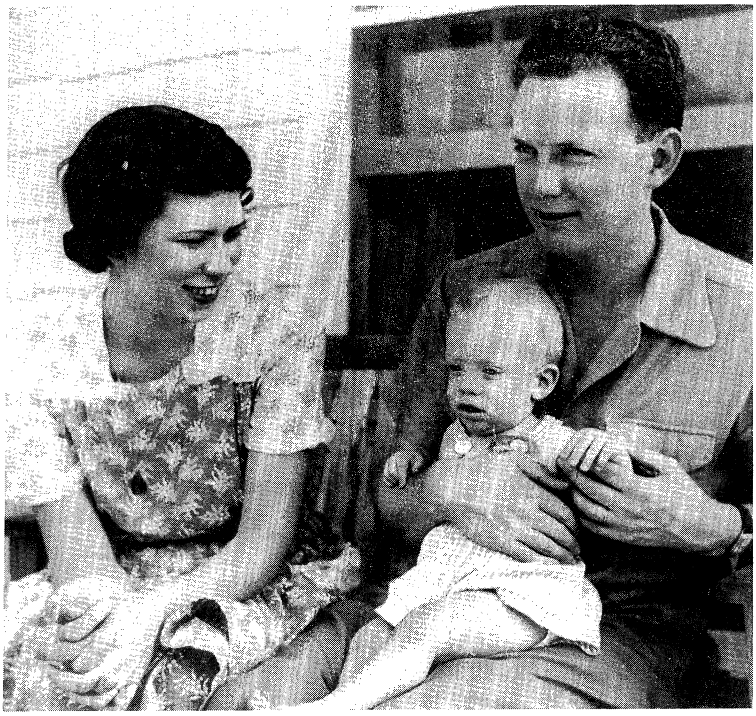
Learning Erdese at Purdue

MP: When had you married?

Niven: Betty and I were married in September, 1939. After our son Scott was born, I had a need for a larger income. My salary at Illinois had increased from \$2000 in my first year to just \$2150 in my third year. About that time Purdue University decided to change mathematics from a service department to a full-fledged research department, so that mathematics would be a subject in its own right as well as an important topic for the engineering students. In 1942 Purdue invited me to come and participate in this metamorphosis. It was an exciting opportunity.

MP: And Purdue offered you more money than Illinois?

Niven: Purdue offered a substantial raise, as well as a promise of an assistant professorship after one year there. The policy at Illinois, as told to me by the department head, A. B. Coble, when I asked him what the criteria were for advancement, was that salary raises would come very slowly unless you got an outside offer. Furthermore, although Illinois was willing to raise my salary, as to



Betty, Ivan, and baby Scott in 1942

matching the Purdue offer of a promotion the next year (which would be five years after my Ph.D.) Coble said that was very unlikely because he had a firm policy that every assistant professor in his department should have a salary of at least $3000 per year. In the absence of an outside offer, I would have moved from $2150 to $3000, and thus to an assistant professorship, in perhaps another ten years or so at the rate I was going.

It was not hard to understand why there were so many faculty members at Illinois who were totally cynical about the expectations for their careers. After all, in 1939 there were only ten faculty members with the rank of assistant professor or higher among the total group of 28 regular faculty members, all of whom had earned Ph.D.'s from reputable institutions. The other eighteen members of the Department at Illinois had not yet achieved the rank of assistant professor, although some were old enough to be grandfathers.

Illinois lost some able young mathematicians in the subsequent two or three years: Richard Duffin, an applied mathematician now a member of the National Academy of Sciences; Ralph Fox, later a professor at Princeton; and Paul Halmos, later a professor at Chicago and then Michigan.

Nine new people came to Purdue in 1942, for example Nathan Fine, later at Pennsylvania; Michael Golomb, the only one of the nine who stayed at Purdue; Maxwell Reade, now at Michigan; Leon Alaoglu, who went into industrial work; Arthur Stone and Dorothy Maharam Stone, now at Rochester; George Whitehead, a member of the National Academy, now at M.I.T.; and Gail Young, later a president of the MAA. Dorothy Maharam was not actually one of the nine, because a nepotism rule blocked her employment at that time. Because of the capabilities of the nine new instructors, the mathematical power at that rank was quite obviously greater than at all three professorial ranks put together. Purdue was upside down.

MP: So you guys were really the core of this new research-oriented department. Was that part of the attraction? To build essentially a new department?

Niven: Yes. There were a few strong mathematicians in the professorial ranks, Cornelius Lanczos for example, but very few. There was concern on the part of the nine newcomers that Purdue wanted to strengthen the department by hiring only at the rank of instructor. Why not some distribution through the ranks?

MP: One can foresee the possibility of pretty intense competition in this group. Not an entirely healthy situation.

Niven: It could have caused a serious power struggle, but it didn't. For one thing, the situation in the United States of more mathematicians than jobs, which prevailed for at least twelve long years from 1931 to 1942, was reversed by the summer of 1943, with more jobs than mathematicians. The slave market for positions was gone; if anything, the department heads were now the slaves. War work had changed the picture. Furthermore, within a year or two most of the nine newcomers regarded Purdue as just a way station along the road, pleasant enough but perhaps not a permanent location. The nine of us got along famously, with lots of seminars throughout the week, and lots of weekend parties.

MP: Lots of parties but how much number theory?

Niven: Doug Olds was there in number theory, and Fritz Kiokemeister, of Johnson and Kiokemeister. Paul Erdős would come around quite often, because he had several close friends who always made him welcome. Purdue gave him a research grant of $1000 one year. (Multiply any dollar amounts of those times by a factor of eight or nine to translate them into today's money.) It was a great year, but the Administration at Purdue would not renew the grant because Erdős was absent so much, about one third or one half of the time, lecturing at other universities. The mathematics group argued for renewal to no avail: absences be hanged, look at the joint papers that were coming out, with Erdős as one of the authors.

MP: But who covered his courses?

Niven: He had a research grant. He never taught a course at Purdue. Doug Olds was greatly impressed with Erdős and asked him if he would come as a guest lecturer in his calculus class. So Erdős talked about the beauties of calculus and the wonders of this and that and the fundamental theorem and so on. He finished in about half an hour and Olds said to the class, "Do you have any questions?" Somebody put his hand up and said, "I'm having a very hard time with this problem: $\int \sec^3 x\,dx$." Erdős went to the board and said, "The secant, that's one over cosine. . . ." He fooled around with it and poked it this way and poked it that way and nothing came out. Since he was not able immediately to evaluate $\int \sec^3 x\,dx$, the students were not very impressed with Erdős, this world famous mathematician.

"Shnahv-plahv"

MP: I seem to recall that you speak fairly fluent Erdese.

Niven: Several of us learned the language Erdese from Erdős. It was invented one day at lunch when Erdős looked at the menu and said, "Ah, peeneh appleh oopsheedeh dohven sahkeh." He was saying "pineapple upside down cake." And that's what Erdese is, a spoken language in which English words are pronounced using Hungarian phonetics. Thus "icicles" comes out as "ees-ees-lesh," and "snow-plow" as "shnahv-plahv." People were mystified by this foreign language we were speaking. But there is an even better story about "snow-plow" from Mark Kac. He would constantly ask his colleagues at Cornell about the proper pronunciation of words in English. On one occasion Kac asked (in effect) whether "snow-plow" rhymed with "how-how" or with "blow-blow". The colleague replied that it rhymed with "blow-how." Kac said later that he could never remember whether it rhymed with "blow-how" or "how-blow."

MP: In that group of nine that you named, there weren't any real emigrés, were there?

Niven: One, Michael Golomb, and he's still at Purdue.

MP: Did the presence of the many emigré mathematicians make much of a difference to your mathematical life at that point?

Niven: The immigrants tended to have more general and advanced seminars than the Americans did. They would get lots of people participating.

MP: So from your point of view the influence of the refugees on your life was positive?

Niven: Yes, definitely.

MP: Even though they did represent honest-to-God competition in the marketplace?

Niven: There were positive as well as negative aspects. Emigrés like Lipman Bers, Sammy Eilenberg, Michael Golomb, Mark Kac and Stan Ulam, who were just a little older than I, offered powerful competition to the young American mathematicians. In my case I was a beneficiary of their presence. Besides the influence of Hans Rademacher and Reinhold Baer which I have mentioned, I wrote papers with Eilenberg and with Erdős, in fact several with Erdős. (Of course I also wrote a dozen or so papers with American mathematicians, too.) But I was a foreigner myself, a Canadian until 1942.

MP: But not quite a refugee.

Niven: No, not a refugee.

MP: You're pretty positive about this whole refugee phenomenon. As I understand it, however, there were people who did not feel so positive.

Niven: The eminent American mathematician George David Birkhoff made the oft-quoted observation that he didn't want young American research mathematicians to become hewers of wood and drawers of water, meaning, of course, that he didn't want them to have to go out and teach in Podunk College. It has to be remembered that in the late 1930's there were only 18 or so American institutions offering Ph.D. work in mathematics in a serious way. In most other schools there was not a supportive atmosphere for research. A lot of schools called themselves "teaching universities," with teaching loads that had been raised from 15 up to 18 hours a week during the depression years of the 1930's. Even at the University of Illinois, a prominent research institution producing many Ph.D.'s in mathematics, my load was four courses, 15 hours a week, when I went there in 1939. G. D. Birkhoff was genuinely concerned, I believe, for the welfare of young American mathematicians at the outset of their careers, that they should be able to pursue their research work if they had the talent.

Birkhoff died in 1944 so he didn't live to see the virtual disappearance of this potential source of conflict over research appointments. Toward the end of World War II and later, many of the teaching universities changed their character by strengthening their graduate and research programs. In addition, many universities were created or grew remarkably in size and scholarly ambitions, such as the state universities of New York and the branches of the University of California. In fact, had it not been for the presence of the European emigrés along with the students they had developed here, it would have been difficult to staff the emerging graduate programs in mathematics.

MP: Did you ever think of going back to Canada?

Niven: Yes, I did, but I didn't really want to go back. When I visited the University in Vancouver in the early years after my Ph.D., I was still in a student-faculty relationship with the professors, whereas at Illinois I was treated like a colleague. So I declined an offer at that time. Then in the mid-1950's I was asked again if I wanted to return. It was an attractive opportunity, but by that time Betty and I were busy with our work in Oregon, and quite attached to Eugene.

MP: When did you become a U.S. citizen?

Betty, Ivan, and son Scott, who also earned a doctorate in mathematics

Niven: In Illinois, in the spring of 1942. Reinhold Baer and his wife went with me to the swearing-in session at a Federal courthouse to find out what was involved.

The Attraction of Oregon

MP: What attracted you to Oregon?

Niven: That's an easy question in the sense that I grew up in the Northwest and I was used to a moderate climate. I did not like the midwestern climate. There was no air conditioning back in those days and those midwestern summers were tough. To me, it was like Calcutta. Betty and I had been on a trip out to the West Coast and she rather liked it. So in the spring of 1947, at Betty's suggestion, I wrote to a whole string of department heads up and down the coast. I said I'd like to get acquainted. But I was in no big hurry since I had a tenured position at Purdue. Invitations to stop by and have coffee or lunch came in promptly from all the

Ivan, Betty, and son Scott in 1947

schools except Oregon, where they had started checking into my background and my record. A fortnight or so later they offered me a position. At that point I checked into *their* background and *their* record, and then I accepted the post.

MP: How big was the department at Oregon then?

Niven: There were about seven or eight full-time faculty members. The engineering school was over at Corvallis so the University of Oregon in Eugene had a smaller department. When I went there, however, I didn't burn any bridges. There'd been a financial problem in Oregon in 1932 during the depression and they had decided that since Oregon and Oregon State were just 40 miles apart and there was a lot of duplication of courses they would consolidate them. Journalism would be in Eugene, sciences would be at Corvallis, and so on. So from 1932 until the second world war Oregon had had only freshman and sophomore courses in mathematics, physics, chemistry, biology, and geology. During the war the policymakers came to their senses and realized it was absurd to have a university without any science. In any event I didn't burn any bridges. I kept up good relations at Purdue. But everything worked out OK at Oregon so I stayed.

MP: Was it at Oregon that you started to write more popular things?

Niven: Yes, but not for several years. I went to Oregon in '47 and the first book I wrote didn't come out until 1956. That was the Carus Monograph on irrational numbers [Washington, Mathematical Association of America, 1956]. I wrote the number theory text with Zuckerman in 1960. Then I wrote a couple of those books for the New Mathematical Library [*Numbers*: *Rational and Irrational* and *The Mathematics of Choice*, Washington, Mathematical Association of America, 1961 and 1965]. Lipman Bers and Paul Halmos had approached me at the International Congress in Edinburgh in 1958 and told me about the plans for starting the New Math Library. They were on the committee inaugurating it. They wanted to know if I would write volume number one, and I did. The reviews on the Carus monograph had been favorable. And then some publisher came into my office and said, "Would you write a book for us?" You know how it is; one thing leads to another if you're successful.

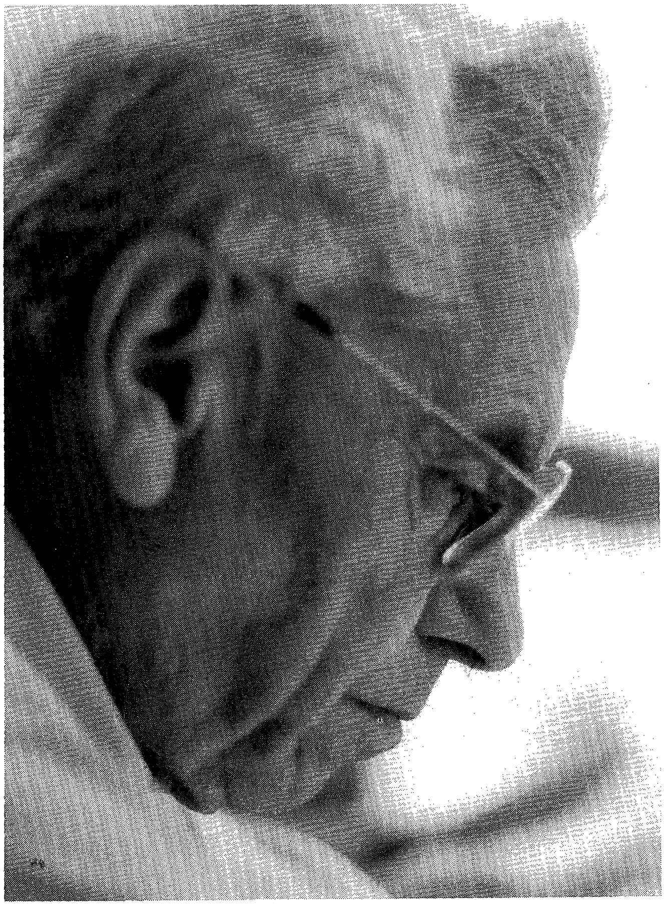
Erdős—Taking Problems by the Jugular

But let me tell you about writing with Erdős. I've written five papers with him. The way he works—at least the way he worked with me on those papers—is that we would have a specific kind of problem that we wanted to solve. We didn't necessarily know the answer, but we knew that there ought to be some kind of a theorem or set of theorems. So we would discuss it and make some conjecture. Then we would talk about how we could possibly prove that. There might be intermediate steps, lemmas and stuff like that on the way to proving it. We'd map out a possible way of getting to that final answer, if there was a final answer. This is the thing that interested me about Erdős. We would have, say, six steps leading to a final answer, all hypothetical. Well, my natural inclination would have been to try first to prove the first step, then to prove the second step and so on. Erdős would say, "What is likely to be the hardest step to prove? Let's go for that." He'd take it by the jugular, so to speak. If we couldn't handle that hardest step, we would junk our plan and reconsider our conjecture. If we were successful, we'd ask, "Now what's the next hardest step?" And we'd do that. Sometimes we'd get the three hardest steps done and he'd look at the thing and say, "Well, that's that problem done. The rest is easy. You finish it off and write it up." That was his way of working and it was very instructive to me. The idea of starting at the beginning, going on till the end and then stopping, that is the ordinary way of doing things, not Erdős's way. He wanted to tackle what appeared to be the most difficult aspect of the conjecture.

MP: You wrote five papers with him so five times it worked. How many times did the system fail?

Niven: It always worked for us. The plan would sometimes go awry but then we'd start all over again. We were always successful in getting through to something.

MP: Ivan, we've talked a lot about your education, teaching, research, and writing. Now let's get to your work with the MAA. You're regarded by most people who spend any time around you as a consummate diplomat. You always know the right thing to say at the right time. People like that are often tapped for administrative positions and the Association has very wisely elected you to several



Erdős thinking about his favorite subject. Photo by George Csicsery.

offices including the presidency. You've also been on the advisory council of the president of the University at Oregon, which is an important kind of advisory council, not a rubber stamp operation. I notice that at the University of Oregon you've been very successful. But you have resisted the department chairmanship and deanships and other sorts of things. How do you explain that?

Niven: I have been asked if I was interested in being considered for some administrative post or other, including the deanship at Washington as well as at Oregon, but I have always said, "No, thanks."

MP: Why?

Niven: In my early days I regarded myself primarily as a research mathematician and university teacher. I did research for about 25 years after my Ph.D. and then started to do more expository writing. Since then I've done little things in mathematics, but nothing very big. But I just wasn't interested in moving into administrative work. I found out by being on the advisory council of the university that administrators spend a lot of time with oddball faculty members who pester the hell out of administrators. In the course of my years I hardly ever went to see the dean about my work or my problems. But a dean spends a hell of a lot of time troubleshooting with some of the worst of the cases. There is also the problem of trying to get rid of people who are doing a poor job. Well, it's important work, but I just didn't want to do it. I guess maybe it was selfishness on my part. I would have made more money from time to time going that avenue, but although I didn't write any books that were bestsellers or anything like that, when you've written several books and you've got a little money from each of them—the Niven-Zuckerman thing has been pretty good because it's gone to a fifth edition—you don't do so badly. Oh, I should say too with respect to financial questions: I had National Science Foundation contracts and Office of Naval Research contracts for many many years in a row in the summertime, so I could add to my salary that way. I didn't need to solve any financial problems by going into a deanship. I had my own agenda, which was mathematics and expository writing.

MP: But you are a superb problem solver and not just in the mathematical sense. With some of the things that have happened in the MAA Board of Governors you're the one who steps in, smooths the ruffled feathers, and comes up with the compromise that takes care of everything. You would have made a great dean, or president, or whatever.

Niven: Well it's very kind of you to make those remarks. Yes, from time to time in faculty meetings at the University of Oregon when we had a deadlocked situation, I would sit and think hard: how can we get out of this mess? Then I would propose a compromise.

MP: Can you remember being up against the wall with a situation in mathematics? If so, how did you work your way out of it?

Niven: I reconciled myself early on to the fact that there would be problems that would be too hard for me to do. So I drew back and did what I could do. You test your limits. Early on I spent quite a bit of time one year trying to settle the question of whether Euler's constant is irrational or rational. That's still not known. I thought I'd take a flyer at it. And so I looked at Euler's constant, the limit of $\sum_{i=1}^{n} 1/i - \log n$ as n tends to ∞. I got all the formulations that I could, expressing things in terms of an integral. You see, I had done that work on the irrationality of π, so I thought maybe I could do something about Euler's constant. But I couldn't, much as I would have wanted to do it and much as the problem bugged me. I don't think it scared me, it just bugged me. I resolved, "Now I've got to stop looking at that." I think there have been at least four other problems which buffaloed me and which I never have been able to solve.

MP: How many decades will it be till the Euler constant problem is cracked? Any estimates?

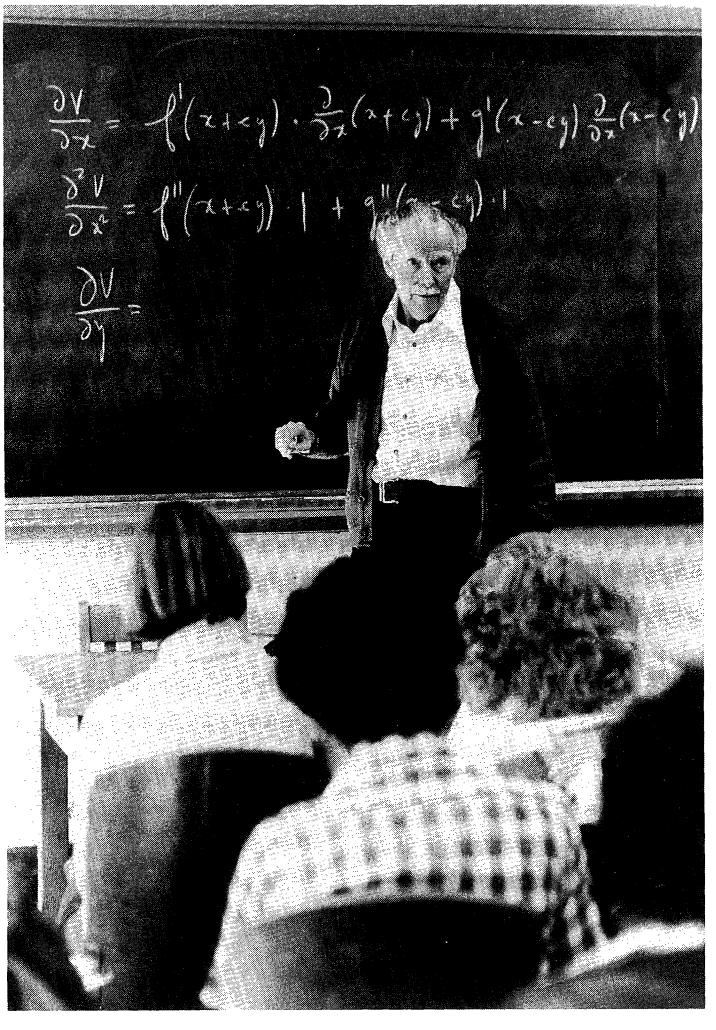
Niven: I would expect it to be solved within the next thirty years or so, because new methods have come along. And there are more mathematicians to work on it. It'll probably be a young person. Mathematics is a young person's game.

MP: Do you really believe that?

Niven: Well, I don't want to. But the record is clear: the famous unsolved problems that have been settled over the last forty or fifty years have usually been conquered by young mathematicians. However, there is no universal law. The Bieberbach conjecture was settled by Louis de Branges at Purdue, when he was not so young.

Mature Mathematicians Should Not Get Psyched Out

MP: He was a mature mathematician.



Niven loves to teach.

Niven: Mature people should not be psyched out of doing anything more in mathematics by interpreting the "young person's game" concept as an absolute rule. I was in my late forties when the ideas about the uniform distribution of sequences of integers started to flow in my head. I suspect that one difficulty as you get older is that you know too much. You have all these methods at your disposal, and you try all of them with all the combinations and variations you can think of. And they all fail. You just start running down the old tracks and nothing works. Pasteur's famous dictum that "chance favors the prepared mind" is a half-truth, no, make that a ninety percent truth.

MP: Of course, de Branges's proof of the Bieberbach conjecture, was not really a new method. He took something from Loewner here, a bit of Askey there, and so on. He juxtaposed them all and they worked. So it's still possible for an older person to solve a famous old problem.

Niven: Yes, and Weierstraß did some powerful mathematics in his later years. So we can all hope to be a Weierstraß. Nevertheless, a mathematician who is publishing fewer and fewer papers later in life might as well relax and enjoy whatever fame he or she has, be it great or modest. In any case I can relax and enjoy it because I can still write expository papers in my seventies. I just finished, jointly with Tom Apostol, an article on number theory for the *Encyclopaedia Britannica*. And Hugh Montgomery joined me (H. S. Zuckerman died over a decade ago) in writing the fifth edition of the well-received book on number theory. I have enjoyed working with many capable mathematicians.

"Did you know?"

There is a familiar use of the accent in mathematics to form new variables or other new signs for whatever purpose; thus x', read "x prime." Further signs are gained by doubling and trebling the accent; thus x'' and x''', which latter-day mathematicians and philosophers I have known persist in reading "x double prime" and "x triple prime." I even hear students and colleagues oblivious of origins speak of the accents themselves as "primes." My mathematics professors back in college read x'' and x''' rather as "x second" and "x third," in conformity with origins. The Latin for the three was "x primum," "x secundum," and "x tertium." Hence the otherwise puzzling "prime."

Quine, W. V., *Quiddities, an Intermittently Philosophical Dictionary*, Belknap Press, 1987, p. 125.