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☛ **Early years**

Q. You were born in Iceland?

A. That's right. I am born in north Iceland, as a matter of fact. The town is called Akureyri. It was — as Iceland was at the time — rather primitive. It had around three thousand people, and yet it was the second largest town in Iceland: Iceland even now has only 300,000 people, total. And Akureyri was very isolated because there was in 1930 no passable road from Reykjavík, the capital, and the only communication was by boat or by horse. The farmers in that area brought their goods to Akureyri — milk and so on — and this was all by horse-drawn carts. So I remember very vividly horses in the town. At that time there were probably five or seven private cars in town. The owners were primarily medical doctors who needed them for their practice. There was a taxi station, too. So there were some decent roads in the town. The roads in the northern part of the country were mostly one-lane gravel roads.

Life in Akureyri was simple: no TV, of course, and the radio reception was rather poor. Sports and social life at all ages was quite active. The large fjord would be frozen over much of

the winter, providing infinite space for skating. As children we would skate for hours until we were exhausted, then lie down on the ice, look up in the black evening sky, and admire the dazzling display of northern lights in various colors.

My father was born in 1892 and my mother in 1900. Both of them lived in Reykjavík in the early part of their years. My mother lived in Reykjavík because her father was postmaster general there. My parents' families go way back. You see, in Iceland it is very easy to trace families, partly because there are so few people but in addition because there is available recent software called Íslendingabók,¹ which can trace how an arbitrary person is related to you. My mother happened to be of a very large family called Briem, which went way back to 1750 or so. Iceland does not have family names, except from old times. Now you can use an existing family name but you cannot introduce a new one. You have to use the patronymic system. That is, your last name comes from your father's first name: Helgason means son of Helgi, and so on.

My father became a medical doctor in Reykjavík, and he practiced for a couple of years in Iceland before he went abroad for specialization in ophthalmology in Germany, in Freiburg and Berlin. So he became an eye doctor and was offered the position in Akureyri by the surgeon general. At that time the number of eye doctors in Iceland was maybe two or three. He settled in Akureyri in 1926 and served until 1970 as the only eye doctor in north Iceland. This meant that in the summer he traveled to the various villages in the north and set up consultations, two or three days in each place, depending on the size. There was very often a medical doctor in these areas where he would be housed. Thus people didn't have to travel to Akureyri, but they could see him in these villages. This took him, I guess, a little over a month every year.

This travel was rather primitive: first a little by car, but then by boat and finally by horse. So he had his equipment on two horses, and he set up consultations in schools or little buildings where he could house his equipment. I was with him on one of these trips, I remember. It was to a village called Raufarhöfn: there was a herring factory in the village, and I and several gymnasium students got jobs there for the summer, mid-June through August. This was convenient because the herring season coincided with the school vacation. My father happened to be going to that place on the boat with me at the time, so I saw his setup.

He actually produced the glasses too; he ground the lenses. After the summer he would be preparing dozens and dozens of glasses for the people he had seen during the summer, and the

¹ Named after *The Book of Icelanders*, originally written by a twelfth-century priest.

whole family was involved in packing these things. We would send them by mail to people, by boat.

My father found glaucoma to be abnormally frequent in North Iceland and gave several talks on the radio about it. With no TV and only one radio station, this reached many people and may have made some difference.

The work at the herring factory in Raufarhöfn consisted of two six-hour shifts each day. It was a hard and messy job. I remember several hours of rowing a boat inside a huge herring oil tank while my partner stood in the boat spreading protective paint on the inside walls of the tank. We alternated positions occasionally. The smell was overpowering and falling out of the boat because of the slippery bottom meant certain death, since nobody could swim in that oil - he would simply sink. However, the pay was great, at least when the fishing was good. The last summer I worked there, the herring season lasted until mid-September. The bus that took us back to Akureyri got stuck in a snow bank during a blizzard, the motor died, and we spent the night in the bus. Neither cell phone nor radio communication there. Help came the following morning from the village of Húsavík. I was sixteen or seventeen at the time.

In earlier summers I had done the usual thing for boys of my age, did some light work on a farm, haymaking, attending to animals, sheep, goats, cows and horses. It was quite enjoyable.

❗ **A Scandinavian education**

Q. You went to gymnasium in Akureyri, which started in 1930 as the second gymnasium in the country.

A. Yes. Gymnasium is about equivalent to high school, although it goes normally until the age of nineteen or twenty. So the last classes are a little bit more advanced than an American high school. At that time, there were just two: one in Reykjavík and one in Akureyri. Since then there are several others. There was a lot of time spent on languages, because Icelandic is only used by Icelanders: it resembles Norwegian but has more complicated grammar. So we had classes in Danish, English, German, and French. If you chose the “language line,” as my brother did, you would also have Latin. Now this has changed, of course, because Latin has lost its usefulness. But the program was actually quite varied. This was a six-year school, so a bit

longer than high school. I had chemistry, astronomy, physics, and mathematics, in addition to several other subjects.

I think the chemistry was somewhat inadequate because the laboratory facility was rather limited. Physics was pretty good. It was modeled after the Danish system: we used Danish text books. The mathematics program was fine. Our final exam included finding the radius of a sphere inscribed in a regular dodecahedron. So this was a pretty good school, I would say.

The teacher that I benefited most from was actually an astronomer, but he had certain training in mathematics. He had a doctorate from Göttingen in Germany, which was a very prominent place, perhaps a center of mathematics in the world at the time. This would have been around 1930 or so. His name was Trausti Einarsson. He is no longer alive, but he was a highly respected astronomer and geologist. And he had this infectious respect for mathematics, which he conveyed to the students. He became a professor at the University of Iceland in Reykjavík.

Q. You were in the gymnasium during World War II. How did the war affect Iceland?

A. Exactly the war years, yes. From beginning to end. It affected Iceland in several ways. Iceland of course did not have any army. But after Germany invaded Denmark, England believed that Iceland might be next, so England decided to occupy Iceland. That was in 1940. So they moved in there with a certain amount of army and their equipment. We were very conscious of the army there in Akureyri, because they had a fairly large group there.

Now, that meant that Germany no longer considered Iceland neutral, and that was very costly to Iceland because Iceland had this unlimited trade of fishing with Britain. So there were constant shipments of fish to England from Iceland, and this was a very significant part of Iceland's economy. But by the end of the war, Germany had sunk just about all that fleet. The war had this effect in Iceland that there was considerable loss of life, mainly because of the sinking of the boats. This included many boats in normal travel from one town to another, as well as ordinary fishing boats. These were completely unprotected, you see. My mother's sister and her husband were returning from the United States at the end of the war, in 1945. Two weeks before they left he had defended his PhD thesis at Harvard in Medicine and she was a Pediatrician. The boat was in a convoy coming from New York, and it was quite close to Iceland when it was shot down by a U-boat. The whole family was wiped out: both parents and three children. It was very tragic. As a matter of fact, in percent of population, Iceland lost more lives

during the war than did the United States.

Iceland benefitted from the war in some ways though. After America entered the war, the British were essentially replaced by Americans. Various construction projects, including the airport in Keflavík, caused considerable boost to the economy. At that time, Iceland became able economically to start on the geothermal energy project, first in Reykjavík but gradually it spread over the whole country. This energy source, combined with plentiful hydroelectric power, has eliminated the use of coal and oil for heating.

Q. In 1945 the war ended, and you went to University in Iceland school of engineering.

A. I did, yes. That was a temporary move, because I intended to go to Denmark. Copenhagen was the traditional place. Of course prior to that, around 1930, Göttingen was also popular. But in those days it was out of the question to go to Germany. Göttingen actually did not suffer bombing during the war, so the university escaped more than many others. But the Hitler regime had decimated the mathematics faculty.

But I was told that the University of Copenhagen hadn't quite gotten started during that first year. The teaching had become a bit fragmentary during the last years of the war. In mathematics, a couple of the professors had to settle in Sweden. I think Harald Bohr had to settle there, as did his brother Niels Bohr.² Werner Fenchel was another of Jewish descent; he had also to go to Sweden. So I was told it would be more practical for me simply to study at the engineering school in Reykjavík, because, as I said, the science education in Iceland was very much modeled after the Danish one, including the use of the same textbooks. So this was a practical move, just to stay in Reykjavík one year.

During that time, my fellow engineering students spent a lot of time on technical drawing. Actually the professor was the father of Vigdís Finnbogadóttir, who later became president. He had very high standards. Nowadays that drawing has been computerized, but at that time, much of the students' time went to that. But I could skip it. The courses I took were in mathematics, physics and chemistry, and a small one in philosophy.

Q. In 1951, you won the University of Copenhagen's Gold Medal for an answer to a Mathematical Prize problem, to establish a Nevanlinna-type value distribution theory for analytic

² The Bohr's mother, Ellen Adler Bohr, was Jewish.

almost-periodic functions.

A. Yah, the University of Copenhagen had a tradition that goes back — well, I don't know if it is centuries, but very far back, and they still have this system: every year the university posts certain prize questions, not just in mathematics, but in all fields. In literature it may be a literature study about some specific author. There is one in theology and one in physics and so on. So it would generally be an essay about a specific topic. In mathematics it is usually a real research problem.

Now, in Copenhagen, I had plenty of time. You see, the degree there was the so-called *Cand. Mag.*³ This meant you studied mathematics, physics, chemistry and astronomy to a certain level so that you could with ease teach these subjects in gymnasium throughout Denmark. Then after the two or three first years, you specialized — and I specialized in mathematics — and that would be like a beginning of a graduate school. But the number of courses available was rather limited, which meant that I had lots of time on my own. I could have finished my degree much earlier than I did. But then I noticed one of these prize problems, because it was posted there in the university. So I decided to try that, mostly for the fun of it, also because there was plenty of time for me.

The system is that you hand in your response after a year: you have the calendar year for this. The only condition is that you had to be under the age of thirty, so technically anybody in the world is allowed to try. And you do it anonymously: the author's name is not on the paper, but rather a certain symbol. I used the letter "Aleph" Along with the paper is a closed envelope where the author's name and the symbol is included. If there is no reward for the paper, the envelope is not opened. So you had nothing to lose if you didn't win! [Laughs]

Q. How hard are these problems? How much did you work on it?

A. I worked on it for a whole year. Yah, it was a hard problem, at least for me, because I didn't really have the background needed, and since it was supposed to be handed in anonymously, I could not consult with anybody: I felt duty bound not to tell anyone. So I was on my own trying to get the background. It wasn't even clear what background I needed! So I think the first four months went on just finding out what the problem was about. But an indirect benefit, perhaps, was that I got used to working on my own without any guidance.

³ *Candidatus* (m.) or *Candidata* (f.) *magisterii*.

There were two subjects involved. One was a subject founded by a Finnish mathematician, Rolf Nevanlinna. There was a school of Finns that all went into that line, including one who later became a professor at Harvard, Lars Ahlfors, and who became a good friend later, after I got to MIT. Then there was the other side, the theory of Harald Bohr, who was a very prominent Danish mathematician, and he had started this almost-periodic function theory. The problem was to somehow combine these two theories. That is, to show that the Nevanlinna theory could be modified so that it fits the theory of almost-periodic functions and then prove the analogs of Nevanlinna's two main theorems as well as the defect relations. So this was what I did.

The problem isn't completely solved yet because I had to make a certain restriction on the functions considered. I did actually revise the paper later with the intention of pushing further, getting rid of this assumption. This was after I got to the States. But then I somewhat lost interest. I have published an excerpt of the paper in the proceedings of a memorial conference to Harald Bohr. I have been approached lately by two Russians about what remains of the problem, namely certain subtleties concerning the so-called defect relations. But almost-periodic functions is a rather limited subject, although it was intensively studied in Denmark during the '30s and '40s even. Harald Bohr developed the theory in the '20s, and it was very prominent for a while. But it has dried up a bit.

The theory was a kind of a natural extension of the theory of Fourier series, which is a very prominent subject in mathematics. Later on, one can say it has been to some extent absorbed in the theory of Fourier series on compact groups, except for the part that I was involved with, which was more related to Dirichlet series. I would say that this part is very much alive. In fact, analytic almost-periodic functions do constitute a very interesting generalization of Dirichlet series, in that the frequencies are quite arbitrary.

Rolf Nevanlinna had a strong connection to Switzerland. There used to be a Nevanlinna colloquium in Switzerland regularly, and he enters a little bit in the story of André Weil. Weil was in Finland during the second half of 1939, and he was arrested by the Finns during the Russian-Finnish war and was in danger of being executed as a spy. But Nevanlinna managed to have him deported to Sweden instead. André Weil wrote his memoirs published by Birkhäuser⁴ — not very large, but very interesting. There he relates the story. He had to go back to France

⁴ André Weil, *The apprenticeship of a mathematician*, translated from the French by Jennifer Gage (Basel, Boston: Birkhäuser, 1992).

and serve in the army, and he got into some difficulties, having been away when the war started.

Q. Is there a distinct tradition of Scandinavian mathematics, in any sense, a tendency to work in certain areas, perhaps analogous to the Italian school of algebraic geometry?

A. Well, there is nothing analogous to the Italian school in algebraic geometry, which had this weakness that it relied a little bit too much on intuition. In Finland, Sweden, Norway, Denmark there is a tradition primarily in analysis. There you don't have this problem of insufficient rigor, but it is certainly true that in these four countries, analysis has been very prominent. Norway stressed analytic number theory and Norway has of course these prominent stars in the history of mathematics, Niels Abel and Sophus Lie, but they were in somewhat different fields. Abel was short-lived, and his work was related to algebraic geometry, although strongly to analysis as well. And Lie founded this theory of so-called Lie groups.

This is a relatively small group that we are talking about, so one might say it grew up from the interests of very few people. In Finland, there was Lindelöf, the Nevanlinna brothers and Ahlfors who continued in that path, so there was a certain school formed in this area of classical complex analysis. So it was a natural development. Now, in Sweden, the first mathematicians — for example, Phragmén — were also involved in analysis. Mittag-Leffler was very influential in this regard. Sweden has tremendous analysts today, like Carleson, Gårding, and Hörmander. They are getting up in the years now, but their influence is enormous.

So that sort of explains Finland and Sweden. Norway had the number theorists Viggo Brun and Atle Selberg. Now, in mathematics in Denmark, there was for a long time a great emphasis on geometry. The principals were two people: Julius Petersen and Hieronymus Zeuthen. Zeuthen was of the Italian school in geometry, Petersen, a great geometer, and a problem solver, became later an important figure in graph theory. But the analysis, that started I suppose with Niels Nielsen and mainly Harald Bohr and Børge Jessen. This however was somewhat different from the mathematics in Finland and in Sweden: there was not very much contact there. But this is I think a product of those times, that certain special fields in mathematics would be tied to a very specific place or specific country, and I think it is simply that the communication between mathematicians in different countries was very limited. There would be an international congress and a Scandinavian congress every four years or so, and of course there would be journals. But this is nothing compared to the situation today, where

mathematicians are constantly flying to other conferences, especially during the summers, and communicating electronically. So the atmosphere has changed completely.

Q. You solved this research prize problem posed by the university . Were you already thinking of being a research mathematician?

A. No, somehow at that time I wasn't thinking much about the future: I didn't have any particular sights on doing research in mathematics. I suppose I assumed that I would go back to Iceland and take a position as a gymnasium teacher, because that seemed a very pleasant job at the time. I think the teaching load was quite high, although nobody thought about that. But, the teachers in the gymnasium in Akureyri, they were *extremely* respected people — oh, yah. In town, these were the people who were looked up to and they seemed to enjoy their job. So I felt this was just an agreeable position to have.

At that time I discounted the possibility of having some kind of a position or a job at the University in Denmark. It just didn't seem possible: there were very few positions and relatively little research activity. As I pointed out to you, the amount of math courses that were given was rather limited, and Denmark, in mathematics, took a while to recover after the war. Mathematics was only present at one university: the University of Copenhagen. There was another university, Aarhus, but there was no math department there at the time. The mathematics department in Aarhus came later through the visionary initiative of Svend Bundgaard. That meant that there were two math departments in Denmark, and that turned out to be *very* beneficial to both places. There was not a real competition, but somehow they both got revitalized in the process. But it wasn't until the '60s when that really took off. So now Danish mathematics is very versatile, very dynamic, but in those days it seems in retrospect to have been somewhat dormant, although the teaching was first rate.

❗ Princeton

Q. In 1952 you left Denmark and came to Princeton. What brought you here?

A. My parents told me about this possibility of a fellowship to go to the United States. So I

applied to something called the Institute for International Education, kind of a Fulbright.⁵ So I filled out an application there, and I had the choice to go to either Harvard or Princeton, but my professor, Børge Jessen, recommended Princeton, and it was a very wise choice because Princeton did not have such a structured graduate program. There were no exams in the graduate courses. No homework. Complete academic freedom. And I didn't take many courses there! As a matter of fact, the courses in Princeton were, at that time, somewhat specialized, in the sense that it was nothing like the spectrum we have now at MIT — and maybe now in Princeton too. But the compensation for that was that there were many seminars. In particular, students would often organize seminars on topics they wanted to study. Then there was the Institute for Advanced Study, which had regular lectures. I went to some of them, particularly to some given by Arne Beurling. So I was on my own as a graduate student, which suited me fine. It was a great time! But at one time there was a serious oral exam, called the "Generals" On my exam committee I had Artin, Lefschetz and Spencer.

I was only two years in Princeton, so when I finished my thesis there I really knew very little, quite ignorant about several fields in mathematics. At the time I didn't feel that way, but later I realized that two years was a little short. So I could have stayed a little longer perhaps, but when you finish your thesis you are supposed to leave! [Laughs]

Q. Salomon Bochner was your thesis advisor. How did that come about?

A. That was fairly natural because he was one of the major names in the theory of almost-periodic functions. He went to other things after he came to the States, but while he was in Germany, he was one of the founders of the theory. So I went to his lectures and somehow got acquainted with him. During the term when he was on leave I came across a problem in almost-periodic function theory. After Bochner came back I mentioned this to him. He was at first skeptical: "This problem has already been considered by many people." But he quickly agreed that it was far from being solved. Actually, the solution was quite easy, but it required methods which were only known after 1940. It was not directly related to what I had done in Denmark, but it was still within that field. I connected it to another theory, which had become very fashionable at the time, the so-called theory of Banach algebras. So my thesis was on Banach algebras and almost-periodic functions and contained some theorems on each of these two

⁵ The non-profit Institute for International Education, founded in 1919, administers some 250 programs, including the Fulbright.

topics.. Bochner was very accessible and supportive.

Q. What language did you speak to him in?

A. Oh, English. It seemed easier. I could read German, but I couldn't speak it. I had English for four years, German for just two. But neither language was comfortable for me. I hadn't spoken a word of English for five years, so it took some effort. But I had no problem understanding lectures: Mathematics is easy so far as vocabulary is concerned.

Q. Did you know his story at all?

A. A little bit. He came to the States, I think in the mid '30s.

Q. Yes. In April 1933, the Germans kicked everybody who was even a quarter Jewish out of the universities. Bochner was Jewish and Orthodox, so of course he had to leave, and he came to Princeton later that year.

A. Well! That was good, because many other German mathematicians had very great difficulty finding positions here. He was a big star, no question about that. There has not been much written about his life. There is an article by a colleague of his at Rice, where Bochner went after he retired from Princeton.⁶ That colleague was William Veech, who was a former student of Bochner's, and he wrote a little article about his life. He sent it to me, but I am not sure it has been published. Anthony Knapp has an article about him in the biographical memoirs of the National Academy of Sciences, 2004.

Bochner was inspiring to talk to, and he spoke freely about many things.⁷ I also liked the fact that although he talked to me about certain problems that he wanted me to get interested in, the fact that I was *not* interested in those problems but started on some other problems instead — he reacted positively to that and was very supportive. So I got into this habit of just following my own taste in the choice of topics to work on. This suited me better.

⁶ Bochner was at Princeton 1933-1968 and at Rice University 1968-1982.

⁷ To his work in analysis Bochner later added two popular books on the history of science, *The Role of Mathematics in the Rise of Science* (Princeton University Press, 1966) and *Ecllosion and Synthesis: Perspectives on the History of Knowledge* (New York, Benjamin, 1969).

Q. You met Arthur Mattuck at Princeton. Do you remember that?

A. Oh, yes. We were good friends there. Frank Peterson also. We were in classes together, as a matter of fact. Frank and I played tennis together, too. Arthur was of course in a different field: he was in algebraic geometry. Frank Peterson was again in another field, in topology. So mathematically, we were separate. But we all had dinner together in Procter Hall in Princeton. We wore black gowns, and at dinner time we talked about mathematics — it was largely shop talk throughout. This was a complete novelty to me, because I lived in a fifty-student dorm in Copenhagen, and there were no mathematicians there except me. They were mostly engineers, but somehow talking shop over meals was something that you just didn't do. In Princeton that was quite common among the mathematicians, but frowned upon by others, who even complained to the dean.

Frank and I were in one course together the very first term. That course was given by Ralph Fox. I happened to have met Fox in Copenhagen. He came there for a visit. So he knew that I was going to Princeton, and I contacted him when I got there. Anyway, this was a course in topology, in which I knew next to nothing. It was done partly by the so-called Moore method, where there is no text book; he simply handed out some notes on theorems that we, the students, were supposed to prove. Moore was a guy from Texas who is famous for that method of teaching.⁸ Moore went further in that he forbade the students to look at any sources. No books are allowed; you had to do it all on your own. Fox didn't take that attitude: he welcomed that we consulted the literature. And it was a kind of a seminar. Fox would say, "Is there anyone that has solved one of these problems here?" And then somebody would say "Yeah." So we would be usually occupied with listening to somebody giving a talk on a problem. But, as I say, there were no exams: there was complete academic freedom. So if you were not interested, or if you were lazy, you didn't have to do anything. But Frank was in the course, and Frank knew the subject already pretty well, so I would talk to him sometimes about topology.

Another person in that course was Gian-Carlo Rota. It was apparent to me that this guy would become quite a scholar; whenever I took a book or journal out of the library, his name was on the list of earlier borrowers. He was a senior, and he had to take this course for credit. He was

⁸ Topologist R. L. Moore (1882–1974) was most famous for his aphorism, "That student is taught the best who is told the least." He wrote his thesis under Oswald Veblen at the University of Chicago and taught mathematics at the University of Texas for 49 years. The "Moore method" of teaching de-emphasizes (or in Moore's case forbade) the use of textbooks in favor of teaching by the students themselves.

kind of humble, because he was a senior in a class with mostly graduate students, who by custom did not talk much to undergraduates. So I never talked to him at that time, and I didn't get to know Gian-Carlo until I came to MIT and we became close friends. However, he did an enormous amount of the work in that course, and he was very often the one who was called upon to talk: I would say that he probably gave more problem solutions in that class than anybody else. Later I asked him, "Why did you do that?" He said, "Because unlike you I had to take it for credit!"

! **Instructor at MIT**

Q. After finishing your PhD you came to MIT for the first time in 1954. What brought you here?

A. There were these instructorships that you could apply to, so I applied to MIT and I applied to Harvard, both of them. And then a friend of mine, Walter Baily, was driving up to Boston — it was probably early spring 1954 — and he said, "Why don't you come along? You can then take a look at MIT, where I studied." So I did. I liked it, so when I was offered the Moore Instructorship, I accepted right away.

Q. Presumably Bochner wrote you a letter of recommendation, especially since he had written a book with Ted Martin called *Several Complex Variables* — the first American book on the subject.

A. Oh, yah, Martin was the chairman then, so I'm sure that Bochner wrote to Martin. I think the procedure was simpler in those days, because I don't remember asking anyone for a recommendation letter except Bochner. Nowadays the custom is to have several different letters.

Of course, the number of graduate students during those years in the '50s at MIT was very small, and the offering of courses was relatively limited. But there was a big jump at the end of the '50s — the Sputnik atmosphere caused a lot of expansion. In earlier days, undergraduate seniors were I think usually encouraged to go elsewhere for their graduate school, because there wouldn't be enough new to offer them. But that changed in the '60s, and as a result, we have now some graduate students who were undergraduates at MIT.

Ted Martin was extremely successful in building up the department. Later chairmanships

have been rotating, usually for just a three-year period, so people tried to do their research at the same time. Whereas in Ted's time, he was a full-time, permanent chairman.⁹ Through his dedication he accomplished a lot for the department. Later he became Chairman of the Faculty for the whole Institute, which he was for several years. He was a talented administrator, and *extremely* tactful in dealing with people. I later served on several committees within the department, for example for hiring Moore Instructors, where he was a member. I remember that very well. He would always be willing to accept another point of view.

He had a monthly get-together at his house. He sent a card, "We will be home every Sunday during this month, and feel free to drop in," and people did. Nothing very formal, but it was a positive effort to make people feel comfortable. I admired him very much as a chairman. I think he really deserves tremendous credit for what he did for the math department at MIT. He showed up with his family at a celebration of fifty years of the Moore Instructorship, which he started.

Martin died just a couple of years ago.¹⁰ I remember there was an Institute-wide gathering at the Faculty Club, when he was retiring, and he gave a little speech there. He was telling a joke about another guy who was retiring, and who was asked, "What are you going to do now that you're retired?"

"Oh I think I'll finish my book."

"Oh, I didn't know you were writing a book!"

"Oh, no, I'm not writing one, I'm *reading* one."

That's the kind of joke that Ted Martin would say.

Q. Arthur Mattuck remembers that you decided to study Harish-Chandra's work, and that you worked very hard on it at that time.

A. Yes, that's right. When I was in Princeton I did somehow find out about Harish-Chandra's work. Bochner never talked about it — it was very far from his work. I saw it in the literature. It was unrelated to my thesis, but I was led to it because, as I told you, I got interested in Banach algebras and their relation to abstract harmonic analysis, so it was natural to get involved in

⁹ William Ted Martin served for 21 years as head the department (1947–68), the third-longest term in the department's history, after John D. Runkle (1865–1902) and Henry Walter Tyler (1902–1930). After the Martin era the headship alternated between pure and applied faculty. Of the seven heads since then, none have served longer than a decade.

¹⁰ William Ted Martin died on May 30, 2004, at the age of 92.

Harish-Chandra's work. But that was on a very high level, so the first thing I had to study on my own was Lie group theory. Once in the Spring 1954 Harish-Chandra was invited to give a lecture to the physicists. On the faculty were the two physicists Bargman and Wigner, who were among the pioneers of representation theory. I was very happy to go but there were I think no other mathematicians in the audience.

Q. How did you do with the lecture? Did you understand what he was talking about?

A. Yes, I understood it, because he really wanted to be understood by the physicists. And Wigner, he wasn't afraid of asking simple-minded questions. He was a modest man, and might be asking questions where he knew the answer very well but pretended not to, just so that the students would then benefit from the answers. It is not very usual that a professor would play that role. But I think he did! Bargmann was there too; he wrote a very important paper in the field, and so did Wigner.

I studied Lie theory on my own in Princeton and I continued after I got to MIT.

Lie group theory has nowadays become a lot easier. In one year's course you can go much further than you could in the '50s, because the whole thing has become more streamlined. At the same time, the subject has grown enormously, so reaching the frontier is still quite difficult. Lie groups and representation theory, which is an outgrowth of Lie theory, is a strong subject at MIT now. But as I said, this was not in a course given during those two years I was here, when the department and the variety of courses was much smaller. So I would say that even after those two years at MIT, I didn't really have any serious insight in it, partly because nobody else in the area was involved in the subject. Nowadays students have a much easier time of it.

Q. It seems that every serious research mathematician, at some formative point in his or her career, has to tackle something really difficult in order to push the frontier out further. What did it take for you to master Harish-Chandra's ideas? How did you work on it?

A. I started systematically reading his papers. But it did not come so easily to master Harish-Chandra's work, because although I had gotten some facility with Lie group theory, Harish-Chandra's work was way beyond that, too. The background for reading his papers was really rather inaccessible. There were some notes from Hermann Weyl at the Institute, which Harish-

Chandra always quoted and probably had mastered: Harish-Chandra had been at the Institute, so he had absorbed some of this from Hermann Weyl. While Weyl was a spirited writer, I did not always find him all that clear.

Then there was Élie Cartan's work. But his work was, for one thing, relatively little understood, in spite of its great importance. He was one of the great mathematicians of the period, but his papers were quite a challenge. Hermann Weyl, in reviewing a book by Cartan from 1937 writes: "Cartan is undoubtedly the greatest living master in differential geometry... I must admit that I found the book like most of Cartan's papers, hard reading"

Geometry has always appealed to me very much. It's just that global differential geometry, at the time I was interested, was rather inaccessible. It is an old subject, but it was rather quiet until the mid-'40s or so. It has now become a very popular subject, but it took a while.

Chevalley's book came out in '46.¹¹ He was a professor at Princeton University at the time. His book was very useful, in that it clarified a lot of material that was obscure. It's a book on Lie groups, but it had considerable effect in differential geometry also. Chevalley's book and *Seminaire Sophus Lie* in France, 1955, were the principal background sources for me. Later, in 1956, appeared a nice little book by Nomizu, *Lie Groups and Differential Geometry*. It did not go very far — eighty pages or so — but it was very useful to me. However I did not really get seriously involved until I went to Chicago for two years, '57 to '59.

In some ways I was still in graduate school at MIT. I only taught courses that did not take much time. Ted Martin did allow me to give one graduate course, and that required much more preparation. It was called Functional Analysis. That is a very large subject, so I just simply picked out subjects that I liked to talk about.

I picked up a lot of things from communication with other mathematicians. I got acquainted with John Nash right away when I came here, and I was, of course, very impressed with him. I knew of his great work in Riemannian geometry, and he also would often talk about what fields in mathematics were worthwhile and what fields were not. He had plenty of opinions about that, which I took seriously, though with some grain of salt. We were both bachelors, and neither of us liked to cook, so we went to restaurants all the time. Arthur Mattuck was often with us. He was an instructor at Harvard the first year I was at MIT and moved to MIT the following

¹¹ Claude Chevalley. *Theory of Lie Groups, I*. Princeton University Press, 1946.

year.

Nash actually started the MIT colloquium. There was a Harvard colloquium, which met once a week on Thursdays. Then Nash felt MIT should have one too, so it was set up on Wednesdays. After every colloquium, at Harvard and MIT, there would be a party, and there you could talk mathematics with everybody — and you did! And you picked up a *lot* of information that way. I didn't have a car, so I usually relied on Nash or Arthur for transport. So it was very different from what things are now. I mean the community was smaller, but there would be, as I say, a mathematics party twice a week! So it was kind of a graduate school for me.

Q. Did your contact with Nash continue in later years?

A. No. My period knowing Nash was '54 to '56. After that I saw him very little. Nash had become sick, back in '59. I saw him later when I was at the Institute in Princeton in '65. He was not involved in mathematics in any way, and he was under care at some clinic in Princeton. I remember running into him at the library once, we got into a conversation, and I said, "Well, why don't you come for afternoon coffee one day." So he did, but it was very sad. Somehow one didn't know what to do. He was not happy at that clinic, although he talked about sessions with his doctor being productive in some ways. But I didn't follow that up. He was still quite ill at the time.

I didn't see him again until '98. Then he was completely different, and he had recovered a lot. He was a little subdued compared to his older days and didn't quite have the same sense of humor as I remembered. But otherwise okay.

Q. Did other figures stand out from your instructorship at MIT?

A. One of the people I remember from the first years was Wiener. I visited his course once in while, out of curiosity. It was interesting. We did not talk mathematics, but he did talk to me about his stay in Denmark. He told me that he learned to speak Danish and said a few sentences to me in Danish, but he said, "Oh, but I never could master Icelandic."

He came to the common room quite often, where all these graduate students were milling around. He wanted to mix with them a little bit. I played chess with him a few times. I was no good chess player, but I usually won over him. Chess is usually a rather competitive game. But for Wiener, no! He did it strictly for the fun of it. So we would play a game, and after I won he

would say, “Well, you win, let’s try another — let’s change sides.” I found it refreshing: here is this great mathematician, who probably had a considerable competitive streak in him; yet he took chess as strictly a game, nothing more. Philip Franklin however was a clever chess player. I was no match for him. He even looked a bit like Lasker, the chess champion.

‣ **Princeton, Columbia, Chicago**

Q. The MIT mathematics department had a rule that essentially prevented them from hiring instructors, except in very rare instances. You moved around quite a bit in the next few years, but you implied that you were able to continue your work in mastering Lie group theory.

A. Yes. After two years at MIT, I got a kind of a lectureship in Princeton, and there I worked on applications of Lie theory to differential equations. For example I generalized Ásgeirsson’s mean value theorem to Riemannian homogeneous spaces.

Q. Then from Princeton you went to Chicago.

A. Princeton was not set to be a one-year job, but I assumed it was just that. Then I got this offer from Chicago for two years, so I jumped on that.

The chairman was Saunders Mac Lane, so he essentially hired me. He was wonderful, a great guy. Mathematically we had nothing in common, really: he was in abstract algebra. But he was a very enterprising chairman. He had actually invited me to a summer session in ’55: they had a special summer on functional analysis, of which I learned a lot from there. He was visiting MIT, and I think he was looking for younger people to invite to that summer in Chicago. He had some funding for it, so I went there. George Mackey was there. Also Kaplansky and Halmos. And I met Irving Segal there. At the time he was giving a course on group theory in quantum mechanics. So this was a very productive summer.

I met Grothendieck there. He was becoming a star at the time. He had not yet entered algebraic geometry with full steam, but he just delighted in talking about mathematics. I remember we both stayed at the International House at the University of Chicago, and we had our meals there. Once after dinner he said, “Would you like to take a little walk and talk about mathematics?” Sure! Delighted. And I could pick up whatever topic I wanted to talk about, because he knew everything. We discussed a paper by Godement that I had been reading and he

knew very well.¹² I was also writing a paper, and I was telling him what was in it. And he gave some good suggestions. He was not preaching to me at all: he just *enjoyed* talking about mathematics.

This was before he entered algebraic geometry and changed that subject. He had written his thesis, a very strong one, in topological vector spaces. At the time he worked with Laurent Schwartz and Jean Dieudonné. But then he changed fields completely.

Q. Mathematics is such a highly specialized field. How do mathematicians who are “talking shop” bridge the gaps between their different interests?

A. The various branches of mathematics in the last, let’s say, sixty or seventy years, are much less isolated than they used to be. To some extent it was Bourbaki’s project to make the communication easier between different fields. That was of course not the only project, but they certainly made a big contribution in that direction. For example in the subject of topology, they certainly had a role in making that subject useful for the general mathematician. This is, I think an important feature of modern mathematics, that with some effort you *can* relate to people in other fields. For an example, take representation theory and differential geometry: these would be very distinct fields if you go back a hundred years. But nowadays they are related: Representation theory is an outgrowth of Lie group theory. Lie group theory relates to differential geometry. So they are connected in a significant way.

Q. Did you return to MIT after your two years at Chicago?

A. No, I was one year at Columbia in between. I shared an office with Harish-Chandra. At that time I was teaching a course on Lie groups, and I began my book on the subject. He himself gave a course on [Carl Ludwig] Siegel’s work on quadratic forms, but I followed that too. He was very pleasant and enjoyed discussing mathematics.

At that time he had never taught Lie groups at Columbia; he had been there quite a few years, but somehow never lectured there about his own work or even the background to it. That would have involved quite a bit of effort for him, because this material was available mainly from Weyl’s seminar notes from the Institute, but he probably realized that it would be so much

¹² [Roger Godement was a student of Henri Cartan and member of Bourbaki.](#) He wrote a very enthusiastic review of Harish-Chandra’s first paper on infinite dimensional representations, a paper that was later awarded the Cole prize.

work to make this comprehensible to graduate students that he had never done it. In fact, Weyl had been lecturing on material where lots of stuff was taken for granted because Cartan had done it.

‣ **Steele Prize material**

Q. Seems like this demonstrates a common pattern of mathematical progress. Weyl was based on Cartan, Harish-Chandra on Weyl, but it sounds like many gaps remained to be filled in — and that such spadework is motivated not by the discussions of the experts, but by the need to place it before students.

You published *Differential Geometry and Symmetric Spaces* in 1962 — quite an early stage in an academic career to take time off to write a research level textbook. The bet paid off, you might say, as the book and its revision were two of the three books for which you later won the Steele Prize, in August, 1988.¹³ How did you develop the material for that first book, and what made you decide to write it?

A. It was a project that I became committed to in Chicago: I already started planning it there in detail. Chicago was on the quarter system, so I gave a quarter course on topics in Lie theory. It was very small, very few people were there, but it was okay. I remember discussing the project with Chern, who was a major mover in the field of differential geometry. He was very encouraging.

We had a seminar in Chicago that went on probably a whole quarter. I was getting interested in not just Lie groups but symmetric spaces, an outgrowth of Lie group theory, which was the title of the seminar. Joe Wolf,¹⁴ who later became a big contributor in the subject, was a graduate student, but he was the one that instigated that seminar. Participants were Chern, Spanier, Palais,¹⁵ Rinehart,¹⁶ Lashof¹⁷ and me, and a couple of graduate students. So that was

¹³ *Differential Geometry and Symmetric Spaces*; *Differential Geometry, Lie Groups, and Symmetric Spaces*; and *Groups and Geometric Analysis* (Academic Press, 1962, 1978, and 1984, respectively).

¹⁴ Joseph Wolf received his Ph.D. from Chicago in 1959 under S.–S. Chern for a dissertation entitled “*On the Manifolds Covered by a Given Compact, Connected Riemannian Homogeneous Manifolds.*”

¹⁵ Richard Palais had received his Ph.D. from Harvard in 1956 from advisors Andrew Gleason and George Mackey. His dissertation was entitled “*A Global Formulation of the Lie Theory of Transformation Groups.*” He was an Instructor at the University of Chicago from 1956–58 and spent most of his career at Brandeis.

¹⁶ Bruce Rinehart was a Ph.D from Princeton 1956 with Donald Spencer as adviser. Skip material inside next parenthesis.

¹⁶

¹⁷ Richard Lashof had received his Ph.D. from Columbia for a dissertation entitled “*Topological Group Extensions and Lie Algebras of Locally Compact Groups*” and spent most of his career at the University of Chicago.

quite stimulating. But it was unsystematic. People picked out what they could talk about, and some notes materialized. It was not something where one lecture was a continuation of the previous one. You jumped around at will.

I gave a more systematic course in Lie groups for a whole year at Columbia. That was on a lower level, but still at graduate level, and at the same time I was working on this book: I would say one-third of the book, the differential geometry part, was done that year. Then I continued after I came to MIT: the first year, 1960, I gave an undergraduate course on differential geometry. I had the notes already written out, and I simply tested out the first chapter of my book on the students.

The book was in two parts. Differential geometry was the first half of it and some of the second part, but the main topic was symmetric spaces, which was a natural continuation. The first part dealt with things that were classical, but where the proofs were rather hard to dig out. So in some ways the first part was more difficult and a little frustrating, because the main results were known, but the proofs were often either not rigorous or were in Élie Cartan's difficult style.

Now, Ambrose was a serious expert at MIT in differential geometry at the time. He had also studied Cartan's work very much, and a book by Bishop and Crittenden was in part based on his lectures and his collaboration with Singer. But his taste was a little different. His differential geometry was more in the direction of fiber bundles, which included lots of machinery that I did not want to include, because it wasn't quite needed for what I had in mind. Fiber bundles came about in topology, but they fit into differential geometry in a very natural way. But my main aim was an exposition of these so-called symmetric spaces, and there this machinery of fiber bundles wasn't really necessary.

Writing an advanced text book at that age was a little risky, because that doesn't weigh very highly in the job market. I wrote some short papers in the fifties and a long one in 1959, that is true. The last one was related to the book without overlapping it. Of course, it was an advanced book, containing some new results, and I hoped it would be a useful one. But still I thought, better get it done quickly! So I was working very hard on it. I finished it in 1961 just before my son was born. My wife Artie typed it all on ditto masters, and our hands were always blue from the ink when making corrections. A later book of mine, in 1984, just went to the printer handwritten. That was a useful shortcut.

During that year '60 – '61, I was a kind of a hermit: I didn't go anywhere, in spite of the

place being full of old friends! Arthur Mattuck and Frank Peterson and Gian-Carlo Rota, they were all colleagues. It was very nice. I don't think there were parties twice a week at that time — at least, maybe there were, but I didn't go to them twice a week. I was busy. But it was worth it, because the book became reasonably useful. I revised it thoroughly back in '78,¹⁸ for example taking into account very important contributions by my colleague Victor Kac. He had not published this material in full before and had not been able to take it out of Russia so he had to reconstruct it from memory.

Q. Writing is not the main thing that mathematicians are trained to do. What is good mathematical writing? What do you try to do in your books?

A. Well, clarity is the main thing for me. When you read some old mathematics, let's say Cartan's work, you are completely baffled by the seeming informality of his proofs, which came from his experience, combined with uncanny geometric insight. You realize: here's some really good stuff, but you see also that it *does* require to be explained better. So clarity becomes an overriding issue.

Then you want to do something more: you want to somehow say something that ties the various theorems together in a natural way. I don't know how well I do that, although I try. In the 1962 book I wanted to get quickly to the second part, so the first part is a bit condensed. So the writing is nothing to brag about. But I think it's clear, at any rate.

Q. August, 1988, must have been a good month for the MIT mathematics department, as both you and Gian-Carlo Rota won the Steele Prize that year. Do you remember that?

A. I do, I do [laughs]. There was a centennial meeting of the American Mathematical Society, so as a result there was a lot of historical talks, and it attracted a lot of people. It's in Providence, only an hour drive from Cambridge, so I just drove down there, and I gave Gian-Carlo a ride back, because he was also at the meeting. So I asked him, when I was about to take him home, "Are you going back there tomorrow?"

"Yah, I am going back there tomorrow, I — I have to go, because I am getting the Steele Prize."

I said, "That's curious, because I am, too!"

¹⁸ *Differential Geometry, Lie Groups, and Symmetric Spaces* (Academic Press, 1978).

They tell you in writing, but they keep it confidential, so nobody knows until the time. Neither of us knew that the other had received it. So I said, “Well, look, we are near to a very nice restaurant, Le Bocage on Huron Avenue in Cambridge, why don’t we celebrate?” So we had a fancy dinner together, right there.

! Radon transforms

Q. You did a lot of work on Radon transforms.

A. Yah, that became a hobby of mine. That was an interest that developed from a book by a mathematician called Fritz John.¹⁹ He died a few years ago. That book of his came out in ’55 and I got acquainted with it partly because an Icelandic mathematician, Leifur Ásgeirsson, who knew John very well, lent me the galley proofs. It starts with an account of a work by Radon — who was an Austrian mathematician, at the time, although I think he was born in Germany²⁰ — who did this work back in 1917.

Q. Radon’s 1917 paper was about reconstructing a function from its various different line integrals.

A. Exactly. More generally, the explicit reconstruction of a function from its hyperplane integrals. This paper was relatively little known because it was published in a very obscure journal. But Fritz John knew about it and used it as a kind of a foundation for this book, and I found this combination of geometry and analysis absolutely fascinating. I was amazed that this was so little known. This has changed drastically in recent years.

In my paper in 1959, I have a generalization of Radon’s inversion formula to Riemannian manifolds of constant curvature. That was my first inroad. Then I had this seminar on it at MIT. At that time there wasn’t much material available, and the seminar was rather short. Then I proved another theorem in the subject and gave that in a course in the Fall term of 1963. I always remember that because when I just finished the class where I lectured on that theorem, now called the **support theorem**, I heard that President Kennedy had been assassinated. A colleague

¹⁹ Fritz John, *Plane Waves and Spherical Means Applied to Partial Differential Equations*. New York, Interscience Publishers, Inc., 1955.

²⁰ Johann Radon (1887–1956) was born in an area of Bohemia that is now in the Czech Republic and lived in both Germany and Austria.

told me about it as I was returning to my office. Most people remember what they were doing when Kennedy died, and so do I .

This subject Radon transforms became popular because of the application in medicine through CAT scanning, which started around '63 with Cormack's idea: in usual X-ray picture taking, you use X-rays just in one direction. But Cormack discovered if we use X rays in all directions through some inhomogeneous material, you can get full determination of the variable density of the material through an explicit inversion formula. Hounsfield computerized this. So the two of them together made CAT scanning possible.²¹

The support theorem that I mentioned fits in here. There was an AMS meeting in Boulder in '63, and that's when I stated it first. Cormack actually found out that I had worked on this subject, and he wrote me a letter concerning the support theorem and its medical applications, of which I had no idea. I was just interested in the theorem for mathematical reasons. There was nothing of that nature in John's book, but the practical application comes about in the following way: The support theorem tells you that if there is some part — say the heart — that you wish to avoid sending X-rays through, you can still determine the density outside that critical area by only using X-rays that avoid that area. So that is medically useful.

Q. You've been dealing with this for some time. You wrote papers on it as early as 1959, as you said. You published a book on it in 1980.²² And then in 1990 and '94, you and David Vogan got grants to continue researching this.

A. That's right. David Vogan always seems to have the answer when I consult him on something involving Lie groups. Victor Guillemin is a big Radon transform expert. He came, I think, in the sixties. I think his Ph.D. is from the early sixties, maybe.²³

Q. So the interest in this is driven by these practical applications, X-rays, tomography, and there were some geological uses as well.

A. Yes, there is a theoretical side to it, and then an applied side. Now, I have only thought about the pure side, I have to admit that. And I have combined that with my interest in symmetric spaces. So there is a Radon transform on symmetric spaces, which is a kind of my specialty. But

²¹ Allan M. Cormack and Godfrey N. Hounsfield shared the 1979 Nobel prize in physiology or medicine.

²² Sigurdur Helgason. *The Radon Transform (Progress In Mathematics)*. Birkhäuser Boston, 1980.

²³ Victor Guillemin received his Ph.D. from Harvard University in 1962.

I have listened to lectures from the applied side, because there are conferences where both sides appear, so there is very often at AMS meetings a session on Radon transforms and tomography. There are several people that have interests on both sides. A very prominent one is Todd Quinto at Tufts. He is a Ph.D. from MIT.²⁴ He is a student of Victor Guillemin's, although we talked a lot when he was writing his thesis, so I know him well; and he is active in both sides of the subject. He is now, I think, a chairman at Tufts. Another professor at Tufts, Fulton Gonzalez, is very active in the field. He wrote his thesis with me in 1984, entitled "Radon Transforms on Grassmann Manifolds."

Thus it's an attractive subject, because it combines geometry and analysis, and in the case of symmetric spaces, combines this with Lie group theory. Thus it is a meeting ground of many subjects; it's very appealing. So I have in fact two books related to this in the works. They are both extensions of earlier books. One is called *Geometric Analysis on Symmetric Spaces*, which includes chapters on the Radon transform on symmetric spaces; but then I have another one in the works, *Integral Geometry and Radon Transforms*, which is a continuation of the one that you mentioned. It's a new edition of it, updated and less technical, so it's meant for a wider audience.

! Mathematics in Iceland

Q. You did not do many administrative things in the department, am I right?

A. Yes, that is right, I have just served on various Ad Hoc and standing committees and was chairman of the Graduate Committee for seven years, sometime in the eighties. Phyllis Ruby was a great help to me at that time. She was the first graduate administrator and with George Thomas set up the Graduate Office in the form it is today. She served in the department for forty-one years, and was a helpful friend of the graduate students.

Arthur Mattuck has truly done remarkable service for the department, both as department head but also with his effective organization of the undergraduate courses and setting up the Undergraduate Office.

Q. I gather that there were more graduate students from Iceland around the time that you were

²⁴ E. Todd Quinto wrote his thesis, "On the Locality and Invertibility of Radon Transforms," under Victor Guillemin (1978). He was interim mathematics department head (2007–2008) at Tufts University.

graduate chairman. [Laughs]

A. There were two. That has nothing to do with me. One of them, Gísli Másson, was appointed by Nesmith Ankeny, who was my predecessor. And the other one, Einar Steingrímsson, I think I recused myself in that case. Their fields were different from mine (algebra and combinatorics, respectively) and neither took any classes with me. After them came Kári Ragnarsson, who wrote a thesis in algebraic topology. I had him in several courses. Altogether there have been four PhDs from Iceland at the department in past years and at present one graduate student. There is also one faculty member in the electrical engineering department who was born in Iceland.

Q. Has mathematics in Iceland become better since you left?

A. There was essentially just one mathematician in Iceland when I came to the States: that was Leifur Ásgeirsson. Leifur was a good friend of mine, and I did some work on subjects that interested him, just because he told me about them. Since then, there has been a math department at the University of Iceland so there are several people there now and the program leads to a good Bachelor's degree. But the department is still quite small. There are several Icelandic mathematicians in this country, actually. And as mentioned one is coming as a graduate student this year.

Q. Do you retain contact with people in Iceland?

A. Oh, yes. I do. I go back there most summers, I just was there in June. It's not very far — same distance as to California, because you fly directly from Boston now. I have, of course, several relatives that I keep in touch with. There are reunions where we go and see old gymnasium classmates, every ten years certainly, even now. What is left of the class is actually only about fifty percent, but they get together quite regularly. But then I have good contact with the mathematicians, too, although there is nobody there that works directly in my field.

They had an international conference there a year ago, which attracted people in my area.²⁵ David Vogan and a couple of Icelandic mathematicians did a great job in organizing it. It dealt with three topics: harmonic analysis, Radon transforms, and representation theory. So it was all Lie group-oriented. And it was not too large for Iceland to organize, maybe at most

²⁵ “International Conference on Integral Geometry, Harmonic Analysis and Representation Theory” in honor of Sigurdur Helgason on the occasion of his 80th birthday (Reykjavik, Iceland, August, 2007).

ninety participants, so very easy to manage. The unpredictable Icelandic weather cooperated nicely, so country sightseeing during half a day was quite successful. The Icelandic landscape is unusual in its absence of trees, providing a view towards infinity in all directions.

[end]