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**IAS Oral History Project
Interview Transcript**

**Atle Selberg
Interviewed by Betsy Devine
June 11 and 15, 1989**

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** Atle Selberg*
SIGNATURE OF NARRATOR

Betsy Devine
SIGNATURE OF INTERVIEWER

Date:

July 24, 1990

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[First interview, recorded on June 11, 1989]

Devine ...Atle Selberg,¹ of the Institute for Advanced Study. This is June 11, 1989, and my name is Betsy Devine. The Institute has a project. We're taping and recording reminiscences of people about their experiences at the Institute -- for example, your memories of Oswald Veblen,² your mathematical career here, and before you came here -- various things like that. What we want it for is to put it in the archives, in the library. So I'm just mentioning that to you I didn't come to ask you impertinent questions, and if there's anything you don't want to say, just --

Selberg I think I probably wouldn't answer.

Devine I should hope not!

Selberg Or maybe I would answer. It depends on what direction they would go. Well, as you probably know, I first came here in September of 1947, as a temporary member, for a year. I met then, of course, some of the faculty. Not right away the person who really was behind my invitation. That was Professor Carl Ludwig Siegel.³ And he was still in Europe at the time, and arrived only a little bit later in the fall. But I met rather immediately Veblen, and Hermann Weyl.⁴ It took me a while until I met von Neumann.⁵ He was rather busy, and occupied with the computer project, which I think had been in progress for more than a year when I came. It really had started in '46. Do you think my voice is too low?

Devine No, but you do what I do -- at the end of a sentence your voice gets much softer.

Selberg Well, that, I believe, is the Norwegian custom. So it may be difficult for me to change that now.

And also I met Aydelotte.⁶ Aydelotte was still in the director's office when I arrived, and I met him. Oppenheimer⁷ only arrived later in the fall, I think in October or November.

¹ Atle Selberg (1917-2007), Member in the School of Mathematics, 1947-1951; Professor, 1951-1987, Emeritus Professor, 1987-2007.

² Oswald Veblen (1880-1960), Professor in the School of Mathematics, 1932-1950; Emeritus Professor, 1950-1960.

³ Carl Ludwig Siegel (1896-1981), Member in the School of Mathematics, 1935, 1940-1945, 1960; Professor, 1945-1951.

⁴ Hermann Weyl (1885-1955), Professor in the School of Mathematics, 1933-1951; Emeritus Professor, 1951-1955.

⁵ John von Neumann (1903-1957), Faculty in the School of Mathematics, 1933-1957.

⁶ Frank Aydelotte (1880-1956), IAS Director, 1939-1947.

⁷ J. Robert Oppenheimer (1904-1967), IAS Director, 1947-1966; Professor in the School of Natural Sciences, 1966-1967.

- Devine So Oppenheimer would have been director while you changed from being a member to being on the faculty.
- Selberg Oh, yes. Oppenheimer, of course, had already been appointed at that time, but he was not yet, so to say, in office. Aydelotte was functioning still until Oppenheimer arrived. I didn't, of course, get to know Aydelotte well. From what I heard about him, he was a rather nice man, well-liked, which has not always been the case with directors here.
- I have never heard anyone speak ill of Aydelotte. He struck me when I met him as rather old, although he was, as I now realize it, quite a bit younger than I am now. An old, and rather kindly-looking, gentleman, and there are two attributes that struck me -- one on each side of his head. He had enormous ears. And I don't know whether the fact that he was so well-liked had to do with the fact that he was so well-equipped to listen to the faculty .
- Devine I see! Well, if you read the old documents, one thing that has struck me is that when Flexner⁸ left and Aydelotte entered, every time the "faculty" was mentioned it was spelled with a capital "F." This was a change from Flexner, who always spelled it with a small "f."
- Selberg That may be. I have not ever had any encounter with Flexner. All I know about him is what I heard about him from the early faculty -- mostly from Veblen, I must say.
- Devine What did Veblen say?
- Selberg Well -- obviously he was not a great admirer of Flexner. It is clear that they had their differences about the nature of the Institute. Fortunately, I think, in the long run it was Veblen who won out, because as far as I can discern from what I have read of Flexner's writing, from what I understand from the early years, Flexner did not envisage the Institute really in the form that it came to have later. He thought of some kind of a faculty of superstars, who would essentially sit in their offices and think very deep thoughts. And they might have one or two associates. But clearly he did not think in terms of a large temporary membership. And this, I think, was Veblen's idea.
- Veblen may have been influenced by what he had seen in Europe. In Göttingen, for instance, there had been a center for mathematics. Göttingen didn't have anything like the setup of the Institute, but what they had was a rather large number of junior, of temporary, positions, so that they could actually provide some kind of livelihood for a good number of young mathematicians. This contributed, I think significantly, to its nature

⁸ Abraham Flexner (1866-1959), IAS Director, 1930-1939.

as a center. Of course many people came there on their own funds, or funds that they might have received from their home university as a stipend. But quite a number of the people, from what I understand, and I've heard this from other sources than Veblen, -- actually, some time ago, Saunders MacLane⁹ wrote somewhere, it may have been in the *Mathematical Intelligencer*, some reminiscences about Göttingen, from the twenties.

Devine I have seen that.

Selberg Where did it appear?

Devine I don't remember. I'll look it up.

Selberg It somewhat would confirm what I say. And this I think may have given Veblen the idea.

Devine Now, if you had been born maybe ten years before you were, it would have been pretty much a matter of course for you to go and visit Göttingen.

Selberg Then, it is likely that I would. Actually, I will tell you that, after I finished my studies in Oslo, the first place I really planned to visit was not Göttingen but Hamburg, where Erich Hecke¹⁰ was. But by the time it really came about -- I had even applied for a stipend, which I received, to help me to go to Hamburg -- but by the fall of '39 the war had started. I didn't particularly like Germany and the set-up there. Hecke, as far as I knew, was politically very sound. But I didn't want to go under the circumstances, so I never met Hecke. I never went to Hamburg at that time. I did visit Hamburg after the war. In '61 I visited Hamburg, I think. That's the only time. Of course, Hecke died rather shortly after the Second world war. His health was very bad towards the end of the war, and he spent some time in Denmark. They were hoping he would recuperate, but apparently he had a very severe setback. Whether it was malnutrition, I don't know. But at any rate, I would not have gone to Göttingen at that time, because Göttingen, from my- point of view, was not really what it had been. It is true that Siegel was still there. But he left rather shortly after the war had started. [Edmund] Landau could no longer teach at Göttingen, of course. I was not particularly interested in [Helmut] Hasse.

Well, to get back to the Institute, I think that it was Veblen who really brought about the form that the Institute now has, where the temporary members, in a sense, are the major part of the Institute. And I think that is

⁹ Saunders MacLane (1909-2005), Member in the School of Mathematics, 1954.

¹⁰ Erich Hecke (1887-1947), Member in the School of Mathematics, 1938.

right. I would regard it as its main reason for being, really, to receive and give some guidance to temporary members.

Devine It's certainly had enormous success with that.

Selberg I think it worked rather well. Veblen actually said, I remember, more than once, that he thought that the Institute could very well exist without a permanent faculty, if one only had very good committees for selecting the membership each year. Then he thought that a mix of senior and younger people, temporary members, might be able to fend completely for themselves, once they were here. I think it's not unlikely that that is true.

Devine It may be true -- but on the other hand, the idea of the permanent faculty is very exciting to the members. I remember one of my neighbors this fall had written his thesis about something Hassler Whitney¹¹ had worked on. I had the great pleasure of introducing him to Hassler Whitney. He was thrilled.

Selberg Well, I would say that to some extent the permanent faculty, at least some of them, have in a sense fulfilled what I might call some kind of religious function as a kind of idols, or centers of worship. We do have younger faculty members, some of them, and they are in their most productive years. It is true -- certainly in mathematics; what I say now will not apply to the School of Historical Studies, where people tend to make their main contributions at a much later age -- in mathematics and physics, the prime period in one's life is probably over by, say, 45 or so. That's a bit conservative. Some might put it a bit earlier. Of course one can continue to work, and do very good work afterwards, but certainly the most productive period would be before that, between 25 and 45. So that a large part of the faculty in mathematics will be beyond that age, and will not be in their most productive years. Then you have a lot of experience and knowledge, of course, and they can be of great assistance to the younger people. And one can continue to work on the ideas that one has had before, which one may not have had time to work out in details and complete. But I think most of one's really good original ideas come well before that time.

Devine I wanted to ask you about some of the things that you did, perhaps before you came to the Institute. And I want you to answer me not as if you're talking to someone who doesn't understand enormous amounts about mathematics, but imagine you were talking to some young mathematician who is dying to find out just how you thought about the mock-theta function of order seven, for example -- what gave you the idea.

¹¹ Hassler Whitney (1907-1989), Professor in the School of Mathematics, 1952-1957; Emeritus Professor, 1977-1989.

Selberg Well, that came about in the following way. I had seen in the collected works of Ramanujan an excerpt of a letter -- the full letter is not quoted -- that he sent to Hardy,¹² shortly before he died. Then I also received from G. N. Watson in England a number of papers that he had written about the mock-theta functions of order three and of order five. Actually, in those papers he also quotes more of this letter than was in the collected works. So I did see more fully what Ramanujan had written. But Watson didn't do anything about those of order seven. I had some idea that I could connect it with something that I had done earlier -- certain identities -- and use those to determine the asymptotic behavior at rational points of the unit circle. It turned out that it could be done. It was really not terribly difficult. So I wrote this paper. I don't think it has been read by many, because it was published in a somewhat obscure Norwegian journal. Anyway, it was a subject that was not particularly in the center of interest at that time. I did this in 1936, I think, and it was published around '37. I think I probably have a copy -- there we are. You see this? There are many things I don't have copies of, but this has never been much in demand. Let's see, it's '35 it says here.

Devine At least it's in German and not in Norwegian.

Selberg It's in German, yes. I wrote in German at the time. I switched over to English during the war.

Devine So is this your first paper?

Selberg No, this is not my first paper.

This [holding up another paper] is my first paper I did this in the summer of '35. It took a long time to get it [published] because it was sent to England, to G. N. Watson, for refereeing by Professor Stormer. I was just a beginning student in September of '35. I gave this to a mathematics professor in Oslo, Carl Stormer. He had some connection with G. N. Watson, and sent it to him for a verdict of whether it should be printed or not. Watson kept it a very, very long time before finally he returned it and recommended that it should be printed. So that was my first work. I was just eighteen then, when I wrote this.

Devine So you were getting a good head start on your productive years.

Selberg I started reading mathematics quite a number of years before I came to the university. But I hadn't really had any clear ideas of particularly what I wanted to do. It was really my first glimpse of the collected works of Ramanujan that started me working. That came about essentially in that one of my brothers, who was a student, had taken that out from the

¹² Godfrey H. Hardy (1877-1947), Member in the School of Mathematics, 1936.

university library, and had brought it home over vacation for a number of weeks. I had opportunities to look at it and it seemed rather more exciting than most of the other books that I had access to. My father actually had a quite large mathematical library, so I did have access to quite a lot of books, but not anything that appealed to me in the same way.

Devine In that little article that you wrote about your experience reading Ramanujan, you said that there were different kinds of mathematical genius.¹³ I can't quote your exact words, but the scholarly kind, who puts together things from a wide range of sources --

Selberg Yes, it's a good thing that not all people are alike. And the same in every other field also, of course. It would be terrible if all painters painted the same way.

Devine Yes. It seems to me that, here at the Institute, Hassler Whitney was the kind of person who liked to go in with his bare hands to some new field and throw up something. Whereas André Weil¹⁴ is more the opposite -- the person who reads Gauss and reads Poincare.

Selberg Yes. Some mathematicians read a lot, and learn a tremendous amount, and it doesn't seem to harm them. Others perhaps are not able to read that easily, to learn that easily. And you can always find also some that tend to, in a sense, start with very little and just make do with what, sometimes, may be called their mother wit --

Devine Their mother wit -- yes. Well, if you have enough of it.

Selberg Without really knowing all of the sophisticated tools or things that might be available. Of course, there is this advantage in starting on your own, that you are not quite likely to go in the same way as everybody else.

I mean, if you get anywhere, it's likely to be something that is quite different from what other people are doing. So it has that advantage. And it is true that some of the most original mathematicians are, in a sense, people that have worked in what one might think of as a rather primitive way. And actually knowing very little mathematics except what they invent for themselves.

Devine Where would you say that you would place yourself in that continuum?

Selberg I must say, I know I had easier to learn, at least when I was younger, than some of the people I know, -- I'm thinking of, for instance, a Norwegian mathematician Viggo Brun, who is now of course dead. Also another

¹³ *Science Age*, 1988. This article is based on notes taken during a speech by Selberg, and never checked with him before its publication. A more accurate version of the speech appears in Selberg's collected works.

¹⁴ André Weil (1912-1998), Member in the School of Mathematics, 1937; Professor, 1958-1976; Emeritus Professor, 1976-1998.

Norwegian mathematician Axel Thue. Both of them worked in number theory, although there was no relation between them. And I think, probably, I'm less original than they were, in the sense that I probably had learned mathematics and knew more. I am definitely far below, let us say, André Weil in general knowledge of mathematics, because mostly what I know are the things I have been interested in. I find it difficult to read mathematics; I don't read, really, outside of what I'm particularly interested in.

What I feel I might make use of, so to speak. And I'm sure that most of my colleagues here, and probably a lot of the temporary members, know a lot more mathematics than I do. Of course, on the other hand, I may know some things that they don't know.

Devine I'm sure you do! So, during the Second world war, you stayed in Norway?

Selberg I was in Norway. Well, I went, in the fall of '39, since I had this stipend, and I didn't want to go to Hamburg, I went to Sweden, to Uppsala. Just to change locations. I had really thought that I might seek some contact with Professor Beurling¹⁵ there, but it turned out that he was called in [for military service] -- he was working in the decoding. I didn't know that when I went there. I thought he would be there.

I had known him -- I met him in 1938, at the Scandinavian Congress in Helsinki. I was rather impressed by the lecture that he gave there. It concerned partly things that interested me. Well, as I say, he was not there. There were some younger people, with whom I had some contact, although I would say that I didn't learn any mathematics from them. And then there was an older professor, Nagel, originally a Norwegian, working on Diophantine equations. That is a subject that has never particularly interested me. It is number theory, but it's not my kind of number theory. So I spent the fall there, but then I decided that I wouldn't return there for the spring, because it seemed to me that I could work just as well at the University of Oslo as in Uppsala. In many ways, I preferred Oslo to Uppsala -- it had much nicer surroundings.

Devine So you were only about twenty-two years old then, but you were already a fellow?

Selberg I didn't have a Ph.D. at the time I got my Ph.D. in 1942. I must have been twenty-five. But you see, it was not customary, at that time, to start out by taking a Ph. D. I had finished my university studies...

[Pause in recording to change cassette]

¹⁵ Arne Beurling (1905-1986), Member in the School of Mathematics, 1952-1954; Professor, 1954-1973; Emeritus Professor, 1973-1986.

- Devine We were just at the part where you said you had gotten your Ph.D. later, when you were twenty-five years old .
- Selberg Yes. As I think I said, it was not customary to take a Ph.D. right away after finishing one's studies. Actually, I had quite a number of papers before I wrote my Ph.D. thesis. Of course, it was felt that should be a rather more substantial, longer paper. It was a different tradition, I would say, from what prevails for instance here. And I think different from what now prevails in Scandinavia. The pattern has changed a little in Scandinavian countries.
- Devine What would you say the pattern is now?
- Selberg People who take a Ph.D. now tend to take it earlier. It may be, very often, the first paper that they write. Of course, in Norway, it was a completely independent thing. There was no supervision. A person would write a Ph.D. thesis all on his own, and then give it in to the university. They would appoint a committee to study it and see whether this was to be recognized as a thesis or not. You did not have to be enrolled in the university to do that. It required no connection with the university previously. It was, therefore, natural in a sense that it tended not to be the first. It would come after one had gained a little experience, because, first of all, it supposedly should be a more substantial thing. It wouldn't be a short paper, say twenty pages or so. It should be at least fifty or so.
- Devine So, was your work on the zeroes of the Riemann zeta-function along that line?
- Selberg That was my thesis, yes It was the first long paper, really long paper, that I wrote. For that reason, I picked that. The things I had written before, all of them were too short to fulfill the requirements, at least as I knew them.
- Devine How did you happen to get interested in the zeroes of the Riemann zeta-function?
- Selberg I liked browsing in old journals, and I had run across a paper by Hardy and Littlewood where they prove the then-existing sharpest results. I didn't read the details, actually. I find it difficult to read all the details in a paper, but I tried to see what were the main ideas in it, and what kind of techniques were used. And then I also read with particular interest a kind of epilogue at the end, where they discussed why their method wouldn't give a sharper result. And I looked at that analysis and thought quite a bit about it, and then I realized that what they had there at the end was really nonsense. It showed somewhat a faulty understanding of what was the matter. And I realized what one had to add to the method that they used in order to sharpen the result. I did that in various stages -- at first only sharpening it rather slightly, and that was rather easily done. Then,

eventually, working up to a thing that gave the correct order of magnitude. That took me a little while of experimenting to find. Basically what was needed was a kind of auxiliary function, which would serve to neutralize the variations on the zeta-function, on the middle line of the critical strip. Because the real reason why the Hardy-Littlewood method wouldn't give a sharper result was that the fluctuations in order of magnitude are rather violent at times. The function gets occasionally rather large absolute value on the critical line, on the middle line of the critical strip. These large values are taken in a rather small, what you might say an exceptional set. It is still so that when you are looking at these mean values that they study, it is really in the end this exceptional set that tends to dominate when you form mean values. Essentially, they contribute most of them. So you have to put in a kind of damping factor that tends to reduce the fluctuations of the absolute value. I found the basic idea for forming a damping factor, and then I modified it by experimenting with various types, and finally saw what was needed to obtain the sharpest result that I could expect. I knew that would be the correct order of magnitude. That took me a little while, to find that. And, of course, there was a lot of detail. That's why the paper had to be rather long, because there were a lot of detailed estimations that went into it. I think, probably, it has been read by very few people. It's not the kind of thing that one easily reads. It's something that one can do. But it's very hard to read it if somebody else does it.

Devine I see! Unless someone were trying to do it better, or differently.

Selberg Even then, I think -- I told you, I didn't really go through all the details of the Hardy-Littlewood paper. I tried only to see what were the essential ideas, and what were the main points of the technique. I didn't go through all of their calculations, in fact, to check them. I assumed that was correct. It's very, very boring, and very hard, to check someone else's calculations.

Devine Yes -- well, that's what referees are for .

Selberg Usually they don't do it.

Devine No, really? I thought they did .

Selberg I suspect that they don't really go that thoroughly into the details. I can't imagine that they do that. I must say, I try always to see if I can find some other way of checking the conclusion, without having to go through the same thing as the fellow who writes them.

Devine Now, all this is during the second world war.

Selberg Yes. And then of course later -- I was at that time a research fellow. It was a temporary position with very little duties. It was a five-year position, with essentially just the obligation to give one lecture a week, which is of course not very onerous. And even that, I could have gotten out of it I had

wanted to. It was a position that was given to young people trying to make a career in science who seemed somewhat promising. It was the only kind of temporary position that the University of Oslo had. It paid rather little, but it was without duties, essentially, so it left you a lot of free time. There were, of course, in the old times -- I mean before the fifties -- the number of positions almost anywhere in Europe was rather small. It was very chancy if someone would get a permanent position. I didn't really count on getting something permanent until I was around forty or so. Things did change later, and it became almost too easy to get positions at universities for a while.

Devine Were you living at home?

Selberg No, I was living in Oslo. I didn't have any family in Oslo, at the time. I had one brother who lived in Oslo, an engineer, and another who was in the university. I had a third brother who was still a student -- he studied philology. I was living by myself, renting a room, essentially, and eating out, and of course going home in vacations. Actually, I was not that far. I could even go home on weekends if I wanted to. My family was not that far.

Devine I was wondering how it was that you got to know C.L. Siegel -- the person you said is responsible for bringing you here.

Selberg I'll tell you. I actually met him early in the spring of 1940. He came to Norway, probably in January or in February, to give some lectures. He came from Copenhagen, and he had given some lectures there. And then actually, as I heard before he had left, he was not going back to Germany. He was leaving for the United States. And he was lucky to get away, because his boat left only very shortly before the German invasion. But I did meet him. I did listen to his lectures that he gave at Oslo. And I also met him at a party that Professor Skolem gave for Siegel.

Devine So you had met him. Was he interested in your work?

Selberg Well, probably not at that time. There was no reason why he should be. But after the war, he clearly was. He had seen some of my work, perhaps, I think, already during the war. Because the periodicals that the theorem about the zeroes of the zeta-function appeared in, in the proceedings of the Norwegian Academy -- in the early part of the war, these might have gotten to America. Later it is a bit more dubious whether they would reach the United States. But copies did reach the reviewing journals, so these things were reviewed. There were two sets of reviewing journals. There were the two old German ones, the Zentralblatt, and the Jahrbuch. And then the new Mathematical Reviews. in the United States. Somehow it got reviewed also in the Math Reviews. Some copy must have gotten to someone. And I think it was clear that this interested him, so he wrote to

me, actually, and sent me the forms to apply for membership [at the Institute for Advanced Study]. Otherwise, I must confess, I didn't know of the existence of the Institute at the time.

Because before the war, it had not really been that well known. I mean, I knew of Princeton as a center, but actually in the years before the war, the few mathematicians that were at the Institute were really located at the university, so for practical purposes it was thought of as one thing.

Devine And I think in the thirties it was more of a center of topology, and mathematical physics, than it was of number theory.

Selberg Well, yes. Number theory, I think Siegel was the first number theorist in Princeton, as far as I know. But topology was, of course, the strength that started with Veblen.

And it continued to dominate for quite a number of years. But it was never one of my interests here.

Devine So, according to the chronology that I have here, after you came to the Institute, you did a couple of your most well-known pieces of work -- which is unusual. I mean, did your proof of the prime number theorem, and Dirichlet's theorem about primes --

Selberg No, the Dirichlet theorem, I had really finished already when I was in Oslo. I didn't write it up until I was in this country. But the prime number theorem, that was completed in the summer of 1948, about the time that I was ending my first year as a member. I was offered a second year, but I was also at the same time offered a position at an American university -- at Syracuse University, in New York state. I was rather curious as to how American universities operated. Also, since I didn't have any position at the time. Actually, there was a vacancy in Norway, but it would not have been available for me already in the fall of '48. So I did go to Syracuse then for a year, and it was an interesting experience. It's my only experience as a bona-fide member of an American mathematics department, and contact with a regular brand of students. Because otherwise, I have been visiting American universities, but then I usually always lectured on something that particularly interested me, and which attracted only a rather select group of students, perhaps only a few graduate students and some faculty members. Well, it was interesting in more ways than one. I was very shocked at the lack of preparation of a number of them.

Devine I noticed that you got married in 1947. Was that before you came to this country?

Selberg Yes, that was before. I married in the summer, actually very shortly before I left. I married on the 13th of August, and I left for the United States

probably around the 27th of August, or something like that, two weeks after.

Devine I see. Did Mrs. Selberg¹⁶ come with you?

Selberg Not right away. We married in Stockholm. She was working there at the time, at the Institute of Technology, in the physics department. She was doing some work there that she wanted to complete before she came, and that took her -- she was supposed to come in October. But then there was an airlines strike, also.

So it came to be, I believe, November before she came. It took a long time, at any rate. I was alone here for quite a while.

Devine I see. Well, I hope you got a lot of work done.

Selberg Well, I don't know. It took a while, of course. It was all very new to me. You see, I had never been in a place where there were very many mathematicians, and I was not really quite prepared for this kind of -- in Oslo, there were other mathematicians in the university. But nobody else had really the interests that I had, so there was no reason why I should talk to anyone about my work. I didn't, and I was used to being completely alone in my mathematics, and I was also not really that conditioned to talk or discuss mathematics. In that sense, I must have seemed, and I may still may be, a bit unusual, because the normal pattern now, and it was when I came here in '47 -- there was a very lively group. I don't know, it may be that many of them also came from rather isolated situations, but there were always people discussing mathematics. I never got into that pattern. I can talk about it if somebody comes to ask me some questions; I seldom bring mathematics up in a conversation myself.

Because I simply grew up and was conditioned to a different pattern. I tended always to work in isolation, and not depend on other people.

Devine It interests me though that I have read that you worked an awful lot, when you were on the faculty, with the temporary members.

Selberg Well, they came to me. I have always been -- I consider that one of the prime obligations of a faculty member -- that if anybody wants advice, or wants to talk to you about something, then of course you should be available. That I think I have been to the extent I have been able. I don't think I ever initiated the contact, to tell you the truth.

Devine It's hard, I imagine. With the members this year, there were some that went right out and talked with the faculty members. And then there were

¹⁶ Hedvig (Hedi) Selberg (1919-1995), Electronic Computer Project staff, 1950-1957.

some that were much more shy. Of course, they may have been doing wonderful mathematics on their own.

Selberg Yes. Perfectly possible. At one time, of course, practically all mathematicians worked in isolation. If you go back in time, their only contact would be by letters, essentially, which probably took a long time.

Devine So, then do you think it's valuable -- you obviously don't think it's really essential to mathematicians to have a lot of contact --

Selberg Well, for most mathematicians, it probably is rather important. It may well be that if I had developed in a different kind of mathematical environment, with much more contact, that I would have perhaps have learned a lot more when I was young, and I might have known a lot more mathematics. In that case, I might have done a lot better things. One can't know that. On the other hand, it may be that if I had been in one of the hot spots of mathematics in my youth, that I would have been sidetracked, and not have gotten time to think on my own.

Devine Deane Montgomery¹⁷ was talking about a pitfall of the Institute, or it could be. He said that you come here, and there are so many brilliant people, many of them working on the same things that you're working on, that if you're a person who's easily discouraged --

Selberg Well, you see, that is one thing. It is always better to work on something else than what the other people are working on. I think if everybody else started working on something, then one should consider leaving it.

Devine That sounds like a good idea! I want to ask you, forgive me, this is a dumb question -- when you won the Fields Medal in 1950, what particular aspect of your work thus far was it for? Or did they tell you?

Selberg Harald Bohr¹⁸, who was the chairman of the committee, gave a talk where he described the work.

[Pause in recording to change cassette]

Selberg Harald Bohr's address will describe -- it's in the Congress of 1950.

Devine I see, Proceedings of the International Congress of Mathematicians.

Selberg And that would describe what he thought were the reasons. I think that there is a great deal of arbitrariness in all prizes, and I think who gets the prize depends probably to quite an extent on who is in the committee.

¹⁷ Deane Montgomery (1909-1992), Member in the School of Mathematics, 1934-1935, 1941-1942, 1945-1946, 1948-1951; Professor, 1951-1980; Emeritus Professor, 1980-1992.

¹⁸ Harald Bohr (1887-1951), Member in the School of Mathematics, 1948.

I think so, yes. Probably in some way the composition of the committee may have favored me.

Devine Who was on the committee for 1950?

Selberg Well, Harald Bohr was on it. It may say there [in the Proceedings] who was else on the committee, somewhere.

Devine Ah, the committee to select the winners -- Harald Bohr, Ahlfors,¹⁹ --

Selberg Hodge, Morse²⁰--

Devine And Frechet. [Borsuk²¹, Kolmogoroff, and Kosambi²² were also on the committee.]

Selberg Well, I had met Bohr and Ahlfors before. Namely, in 1938 at the Scandinavian conference. Both were rather friendly to me. Oh, and I had also met them again in 1946, at the first Scandinavian congress after the Second world war. That was in Copenhagen. So I have every reason to think that Harald Bohr and Ahlfors might be rather friendly disposed towards me. After all, I was a fellow Scandinavian!

Devine Well, I dare say they wouldn't have given it to you for that reason. They would have found it hard to convince the rest of the committee on that basis.

Selberg No, but I think that such things probably do play a bit of a role also in selection. It also depends on what type of mathematics these people are interested in, on the committee. Of course, for the Fields Medal they have a fairly large committee, so that you have a variety of interests. But for some of them, if something is decided by a smaller committee it is even more chancy. I know the Wolf Prizes in Israel are decided by a very small committee. I consider that it's almost irresponsible -- it's just a committee of three, and it changes. I think that is really too small a committee to have enough variety in the composition of it. But regardless of how it is done, I think there is always something very arbitrary about the prizes, whatever kind of prizes there are.

Devine Well, at least having won two prizes,²³ those two big ones, is some sign that --

¹⁹ Lars Valerian Ahlfors (1907-1996), Member in the School of Mathematics, 1962-1963, 1966-1967.

²⁰ Marston Morse (1892-1977), Professor in the School of Mathematics, 1935-1962; Emeritus Professor, 1962-1977.

²¹ Karol Borsuk (1905-1982), Member in the School of Mathematics, 1946-1947.

²² D.D. Kosambi (1907-1966), Visitor in the School of Mathematics, 1949.

²³ Selberg won the Wolf prize in 1986.

Selberg I mean, it could just as well have -- I did not particularly expect, in 1950, actually. I should have known, because I remembered the Congress in Oslo in 1936, where I did not participate, I did go to some of the lectures, and I heard of the Fields Medal then. But by the time the Congress in 1950 came around, I must confess I had completely forgotten about it. I had forgotten its existence until I was, some time before the Congress, notified that I would get the medal. It came out of the blue. I had not speculated on who would get it, so in that sense I was rather lucky. I can imagine that later, when it was more established, because everybody knew that there would be some medals given out, then they might wonder about it. I think probably it tends to make more people unhappy than it makes happy.

Devine Yes, I bet that's true.

Selberg So it is a bit of a dubious thing. One shouldn't put too much emphasis on this sort of thing. I think it is somewhat wrong, the attention that this is given and to try to isolate those that get these prizes in a separate class, and claim that their opinion on whatever question has more weight -- I don't think that this is correct. There have from time to time been some attempts to solicit opinions on various kinds of controversial questions from the winners of the Fields Medal, in the same way that often people with Nobel Prizes are asked for opinions of questions that have nothing to do with their particular expertise. I think it is ridiculous to think that their opinion has more weight than the average other person in his field .

Devine Then I'll ask you some more questions about mathematics. I wanted to ask if you could say something about the sieve method that you developed.

Selberg In a way, I came across it partly by a lucky accident. I had not really been able to grasp what had been done before in that field. Of course, the originator of the sieve methods in modern times was Viggo Brun. I tried to read, when I was young, some of his presentations, and I couldn't always make any sense of it. I tried also to read some of what other people, who had processed this thing into a form that should be more understandable -- I think that was mainly done by Hans Rademacher.²⁴ In Landau's three-volume *Vorlesungen über Zahlentheorie*, there was an exposition in connection with certain theorems that he wanted to do there. It was too complicated for me. I'm not very good at trying to follow details in someone else's -and I couldn't get the main idea behind it. But then, in connection with my work on the zeta-function, actually in seeking to find suitable factors to neutralize the behavior of the zeta-function on the critical line, I did run into a certain extremal problem, which I somewhat

²⁴ Hans Rademacher (1892-1969), Member in the School of Mathematics, 1953, 1960-1961.

later saw I could apply to obtain some of the results that had been obtained by the sieve. I mean, a sharper form. Then I saw also that I could generalize this approach and really attack all of the problems that dealt with the upper bound. Viggo Brun had developed the sieve method that gave upper bounds and lower bounds. For the problems we are really interested in, the lower bounds became always zero, which meant that they had no significance. I had developed a rather simple method that enabled me to get upper bounds, and they were sharper than those that could be gotten by Brun's method, or the refinements that had been made by Rademacher. It took me a while to try to develop my method so I also could get lower bounds. It seemed a more complicated thing, and in essence it really is. But in the process of doing this, I finally understood what a sieve is, and what is the principle behind a sieve, and I then tried to develop a general sieve theory, which would be a theory that would consider all possible sieves and try to characterize the optimal ones, that would obtain the optimal results. I was eventually successful in that. The problem is that the solution, in general, is not a practical one. It gives you theoretically a way of computing something any given accuracy, but I think even today, for the problems one really are interested in, it would be beyond the capacity of computers. So the method has not really been applied in a numerical sense. What I also could do was to establish that certain things could not be done. I could get a number of results by showing there were certain limitations, things that could not be obtained by any sieve method. This is, I think primarily, the most new feature, that I found a way of characterizing the most general sieve method, and was able to prove some results about all sieve methods for various problems. In some cases, I could obtain best possible results. In other cases, one does not yet know precisely where the limits are because, as I say, that would require numerical computations that are probably beyond the capacity of present-day machines.

Devine You said that Siegel helped you come to the Institute. Did you work with him at all when you came?

Selberg Well, not really, no. I never worked with other people really. I had one -- while I was here that year, Professor Chowla²⁵ came. I got drawn into a question that he raised, so we did write a joint paper about that. But this was initiated, really, in the way that he came to me with a question, and I happened to already know the answer to the question. It was a formula I had developed some time before, in connection with something called the Epstein zeta-function. That was really out of Chowla's interest in the class number of quadratic forms, of imaginary quadratic fields. How shall I

²⁵ Sarvadaman Chowla (1907-1995), Member in the School of Mathematics, 1948-1949; Visitor, 1950, 1957-1958, 1970-1973, 1980-1981.

describe it? It was known that there are a number of imaginary quadratic fields with class number one. Actually, it had been proved that apart from the known ones, there could exist at most one more. And it furthermore had been proved already before that if there existed the tenth -- because only nine were known -discriminant for which the quadratic field had class number one, then the resulting quadratic L-function would have a zero that was not on the critical line, so it would violate the Riemann hypothesis for that function. What was not known was whether, for the largest of the known discriminants of class number one, there was a zero that was off the line, because nobody had made any computations Chowla wanted to compute the value of the quadratic L-function for this character that belonged to the discriminant of, I think, -163.

That's the quadratic field whose discriminant is -163. It's the largest one with class number one. It has been proved now that there are no more. But it was not known at that time. It was only known then that there was at most one more. He wanted to know if there were zeroes off the line. And he thought one way of establishing this, an indication of this, would be if one could compute the L-function. And he wanted first to compute it at the middle point of the critical strip.

Devine That must have been pretty exciting

Selberg Yes. It happened that I had, in connection with something else many years before, developed a formula that was extremely rapidly convergent and could do this. So he set about computing it at the middle line, at the point one-half, and if it turned out negative that would show that there was a zero that was off the line. He came back with the startling information that it turned out negative. I asked to look more at it. Now, a very strange thing had happened! There exist really two theories of quadratic forms -- one going back to Legendre, and one going back to Gauss. In one theory the middle coefficient is considered to be even. You have $ax^2 + 2bxy + cy^2$.

Devine Ah, yes. Because it's simpler to complete the square?

Selberg And in the other theory, the middle coefficient, instead of '2b' you simply denote it by 'b'. So there is a bit of confusion there, according to the theory, about what is the discriminant. It turned out he had applied my formula as if it was developed according to the other theory. I had developed this considering the middle coefficient without the coefficient '2'. He had applied it as if I had a factor of 2. This led to, of course, a somewhat wrong result -- he had gotten this negative value. When I had verified that this was the case, he repeated the computation in the correct interpretation of my formula. It turned out that it was very, very small at one-half, but it was still positive. But it was a very small number.

Devine When you thought it was negative.

Selberg So then we had become a bit interested in this, so we did see some other applications. Actually, what came out of it in the end had very little to do with the class number. The interesting thing that came out of it was rather a result about the periods of elliptic functions, in the case where there is complex multiplication. This is a rather classical thing. It's rather amazing, in a sense, that the result had escaped attention in the nineteenth century when people were extremely occupied with such things. But the result we got was essentially that when there is complex multiplication the periods of the elliptic functions in the Jacobi notation are always expressible as an algebraic number times a certain expression involving gamma-functions of rational numbers. Some special cases have been known classically. In the case of the elliptic functions connected with the integral with the square root of $(1 - x^3)$, or $(1 - x^4)$, where these periods were connected with gamma functions with relatively small denominators. But no result beyond that was known. It turned out that there was a quite general theorem behind this, and this was essentially what came out of this --

Devine Your effort at collaboration.

Selberg Yes, this was something, of course, quite different from what started it. It's rather typical in many ways that in mathematics very often what you end up with has very little to do with what you start out with. You may start out trying to do something, and as you get into it and learn something either your attention may switch completely, -- because you understand something more of the problem, perhaps what you had initially as a goal is quite impossible -- or you may come across something as you are going along, quite by accident, that completely thro'HS your attention in a completely different direction. One can never, I think, predict where one is going when one starts out.

Devine I see. One of the reasons I was asking you if you had worked with C. L. Siegel, of course, is that I wanted to hear from you some of your impressions of Siegel.

Selberg I had quite a bit of contact with him but, he did his mathematics, and I don't think that he needed anyone to work with him! I mean, he did extremely- well by himself. He was in some ways, perhaps, the most impressive mathematician I have met. I would say, in a way, devastatingly so. The things that Siegel tended to do were usually things that seemed impossible. Also after they were done, they seemed still almost impossible. It was not the kind of thing that could be made simple. There are other things in mathematics that may seem impossible to begin with, but after they have been done, they seem very simple. In some sense, I think those are probably the most important things, those that can be made simple. Siegel's work was not really of that kind. It was, in a way, somewhat devastating to sit and listen to him when he lectured on his

work, because you got away with the feeling that you might as well give up, because clearly you couldn't do anything like that. I think that he may have had, in some cases, a negative effect on some of the younger people. They felt very discouraged, perhaps, after listening to him.

Devine Now, why didn't you feel discouraged? Because obviously you didn't.

Selberg Well, I knew of course that I could do other things I couldn't do the kind of things that he did. I think the things I have done, really all of them, are things that -- although sometimes there were technical details, and sometimes even a lot of calculation, in some of my early work -- the basic ideas were rather simple always, and could be explained in rather simple terms. I think in some ways, I probably have a rather simplistic mind, so that these are the only kind of ideas I can work with. I don't think that other people have had grave difficulties understanding my work.

[End of first interview]

[Second interview, recorded on June 15,1989]

- Devine This is Betsy Devine, and I'm going to be interviewing Atle Selberg for the second time. This is side one, tape one, June 15, 1989. So we were deep in the 1940's, heading on toward the Fields Medal when we stopped. You pointed out that if I read Harald Bohr's speech, I could find out why you won it.
- Selberg Well, I think that probably tells it reasonably well. And then, of course, there was the fact that as you saw, there were two Scandinavians on the committee.
- Devine But four or five non-Scandinavians!
- Selberg So I had two votes there, of course, and that undoubtedly is part of the explanation.
- Devine Well, I think it's especially quite an honor to win it after fourteen years of no Fields Medal. They had quite a field to pick from! But I also wanted to hear more about your first years at the Institute, and your impression of the place when you and Hedi first came here.
- Selberg I must say, it was a very great change for me, environmentally. Because I was used to being rather in isolation. I was not used to talking about my work. Essentially, my mathematical contact was reading periodicals and browsing in libraries. I didn't have any personal contacts, really, because the mathematicians in Norway, the older ones that I had contact with, all had other interests. I was not particularly interested in what they were doing; they were not particularly interested, I would think, in what I was doing.
- Devine Veblen felt it was very, very important for mathematicians to have contacts with other mathematicians.
- Selberg I think it probably is important. I think it does help a number of people. It may be that if I had started off more in that kind of thing, I would possibly have developed in a different way. I might have been more dependent on communication, and perhaps also more communicative.
- And, who knows -- I might have done better. It's quite possible. It's rather idle to speculate on these things. I think, though, that there are probably different kinds of personalities. It's also quite possible that my personality may be more suited to work in isolation. But I think for most people that the contacts are very important. And I must say it was also quite something for me to see several names that I knew from books, but had not really thought of as real persons -- like Hermann Weyl. Carl Ludwig Siegel I had seen before, as I mentioned. I had seen him in 1940, in Norway. But the other people here I had not seen. I could have, if I had

tried to in the summer of 1936, when there was this international congress in Oslo. And I did go to some of the lectures, but I didn't really go and see any of these people. Although I know, for instance, Siegel and Weyl both gave lectures, I didn't go to their lectures. I did not go to the lecture given by Hecke, which turned out to be the one that interested me most when I finally saw the report of the congress. I went only to listen to some of the things that I thought -- the title intrigued me in one way or another, not necessarily the right choices. I think what impressed me most at the time was a lecture given by Polya, which I went to and which I found very entertaining. Also, actually, it contained very interesting results. I also listened to Mordell give a talk on some problems in number theory. Otherwise, I mostly went to the number theory section, to some of the things there. And most of that was not really that interesting, I thought.

Devine I see. So, when you came here -- could you tell me a little bit about Hermann Weyl?

Selberg Hermann Weyl? It was a bit slow, to get contact with him, I would say. It is possible that his wife already then -- in less than a year, she died of cancer. I talked with him, but mostly in the Institute. I would say, that first year, of the professors it was Veblen and Siegel that I mostly talked with. Von Neumann, very little -- and some of the others, not at all.

Devine Veblen is one of the people that I'm the most interested in. So, you had mentioned, or maybe it was your wife who said it, the other day at lunch, that you thought that, especially because you were Norwegian --

Selberg Yes, I think that may have been the reason, partly, in Veblen's case. Veblen came from a Norwegian family. Actually, Veblen's father was born in Norway, but came to the United States as a little boy and grew up in Minnesota. The younger brother of Veblen's father, the more famous Veblen -- Thorstein Veblen -- was born in the United States, after the parents had come. Oswald Veblen did not know any Norwegian. His father, and I assume also Thorstein, had deliberately broken away from it, because they grew up in a place where the Lutheran Church tried to keep the young people from learning English, and so from getting into the larger society and so forth. So they broke away and went to study at Carleton College. Consequently, Veblen's father did not speak Norwegian at home, ever, to any part of his family, and he did not try to teach his children anything about their Norwegian background and relatives, and so on. There was a complete break. According to Veblen, only later in life did his father get interested again in his Norwegian background. But it was too late, of course, for his children, for Oswald Veblen in particular, to really benefit from this. But he did learn about where his family came from in Norway, and where he had relatives. And later he actually did visit these places. I don't know that he was able to communicate much with them,

though, because at that time, this must have been some time relatively early in this century, the instruction in English was not that universal in the schools as it is now. You had to take some what you would call higher education, really, before you started learning foreign languages. Whereas now English is taught already in elementary schools, in the later years. And of course today, they get English via television and all kinds of other things besides. English is now very universal in Norway, but it was not at that time.

I assume that when he visited this farm in Valdres that the family came from, there was probably no one there that spoke English.

Devine So, this visit took place before you met him.

Selberg Oh, yes. But there may have been someone around that could act as an interpreter. Because at that stage, he had made contact with Norwegian mathematicians, who of course did know English. So he may have had someone with him that could act as an interpreter. Otherwise, I don't see what he would do then, because he had not learned any Norwegian. But he had an interest in Norway, and he had learned about his Norwegian background. This probably made him feel some affinity. At least, he was extremely friendly, to both myself and to Hedi.

Devine Tell me more about their house. They were still living at 58 Battle Road?

Selberg No, they had moved out, to this other place [Herrontown Road], where now the arboretum is that they donated to the county. So we have only visited them there. But he told us about the house a lot, particularly when I came back as a permanent fixture here and we began thinking about getting a house. He very strongly advocated that we should buy that house [from the Institute]. Veblen told me of the wall that had been built by himself and Birkhoff,²⁶ the stone wall. It's a very impressive stone wall. Actually, it was even more impressive in those days, because the gate -- if you go past there, they have widened the gate, essentially by taking away the gateposts, which were also of stone. It looked more decorative. Now the wall just sort of ends on both sides of a rather wide space. Presumably it was built originally at a time when they didn't have that much car traffic, and most of the cars were probably small ones, narrower than they were for a time. Now they are a bit narrower again, I think. So the gate was widened by just taking the gateposts off and finishing the walls off abruptly on each side. It looked nicer the way it was before, but I can understand it was impractical. There was also a gate attached to these posts that had to

²⁶ Selberg may be referring to Garrett Birkhoff (1911-1996), Member in the School of Mathematics, 1940; or to his father, the mathematician George David Birkhoff, who taught at Princeton before 1912.

be opened to drive through. Now there is nothing, which I think is probably more convenient for people today.

Devine Did you ever go out cutting wood with Veblen?

Selberg Yes. It used to be on Wednesdays, in the afternoon. A group of people would go down. Essentially they would be mathematicians, and occasionally physicists mixed in. I know that Pauli²⁷ was along on some occasions -- he was not reputed to be very skillful! Actually, some considered him outright dangerous.

Devine Well, I hope he had a saw rather than an ax.

Selberg But another physicist was Res Jost,²⁸ perhaps you know him.

Devine I don't know him, actually .

Selberg From the E.T.H. in Zurich, also Swiss. He is retired. He is probably about my age. There is a book that lists --

Devine Yes, *A Community of Scholars*.

Selberg You will find him there with some relevant information. But it will not tell who was --

Devine It won't tell if he cut wood?

Selberg No. I remember, though. He was a rather big and burly fellow. He was quite good at it. Some people had, of course, some experience in handling tools before although most of them did not. Of other temporary members who took part in that, I should mention Raoul Bott.²⁹ He's at Harvard now.

Devine Yes, he was telling me about it as well -- that's what made me think of it.

Selberg Bott took part. He's a fairly big strong fellow, so he did rather well.

Devine He's looking very well. He was here just about three weeks before you came back.

Selberg Well, he looked a bit different in those days. He didn't have a gray beard that, as you know, he now has.

Devine Yes, but it's a handsome beard. I think it's a Hilbert beard.

²⁷ Wolfgang Pauli (1900-1958), Member in the Schools of Mathematics and Natural Sciences, 1935-1936, 1940-1946, 1949-1950, 1954, 1956.

²⁸ Res William Jost (1918-1990), Member in the Schools of Mathematics and Natural Sciences, 1949-1955, 1957, 1962-1963, 1968.

²⁹ Raoul Bott (1923-2005), Member in the School of Mathematics, 1949-1951, 1955-1957, 1971-1972.

- Selberg It's a bit more than Hilbert had, actually. And of course, Bott's face is rather different from Hilbert's, I think. I don't think that Hilbert was particularly handsome. I would say Bott definitely has it over Hilbert when it comes to the exterior.
- Devine And perhaps is a better woodcutter as well, who knows?
- Selberg Yes, I don't know how Hilbert would have done. Siegel, of course, was very active in this. Veblen at that time already was the oldest. He did not really take part so much -- he would come along to take part in the conversation, and not be very active with either the ax or the saw.
- Devine What sort of things did he say? Did he talk about the trees, or about mathematics?
- Selberg Often the talk was about mathematics. It was not restricted to talk about the best way of trimming a tree or things like that. I mean, that is not a subject that you can occupy yourself with for such a very long time. Because the rules are not really that many, and the things that have to be learned have to be learned, really, by doing them rather than by talking about them.
- Devine What sort of things -- you said that Veblen was friendly to you -- what did he do?
- Selberg Veblen's wife was English, and she was very particular about her tea. I believe she was rather well-off, independently. When they married, I assume that people thought that Veblen made a good catch, as one says. I think he probably had nothing, essentially. He was still a fairly junior person at Princeton, and she came from a rather well-off British family. Actually, there are several physicists in her family, even two with the Nobel Prize in physics.
- Devine Owen Richardson, I think, was her brother?
- Selberg Yes, and there was another one.³⁰ And among other things, she had lots of shares in some tea company, and got some very special tea that was given only to the stockholders, and was very particular about her tea, which was very good, by the way. So, we had tea there a number of times. They did not, at that time, engage much in other forms of entertainment. I don't think they gave dinner parties. They probably did so in earlier years. They may possibly also have given cocktail parties, which they certainly did not do in later years.
- Devine I think they left the cocktail parties to von Neumann and Alexander.

³⁰ Elizabeth Veblen's sister Charlotte was married to Nobel Laureate C. J. Davisson.

- Selberg Well, actually, Hermann Weyl gave cocktail parties, later. And probably also before, by the way. After his wife was dead, and he remarried, then he gave some cocktail parties up at the house. I assume that he had a somewhat similar social life in earlier years with his first wife.
- Devine Who lives now in Weyl's old house?
- Selberg I thought it was Constable.³¹ Let's see, what is the address that he is listed under? 284 Mercer Street -- it is a Mercer Street house, but I don't know the numbers well enough to know. The first one who had it after the Weyls was, I believe, Frank Yang,³² the physicist. And Marshall Rosenbluth³³ had it at one time.
- Devine Did you design your own house, the house you live in?
- Selberg We had it built. We had an architect. We had some input, but I would say that the layout was probably more the architect's. My wife probably had a little input, but I did not. I made a sketch for the fireplace in the living room for the architect. He modified it a bit, although he kept some essential elements. I think architects are artists, and will try to satisfy themselves.
- Probably not bad, unless they completely disregard the needs of the people who are going to live there. That can occasionally happen. I don't think it happened in our case. I think we have been rather satisfied with it, and I think in retrospect that modifications were made in the design that I made, that probably do fit better with the rest of the house.
- Devine Did Veblen ever talk with you about the early days of the Institute? About his ideas of what the Institute should be like?
- Selberg We talked from time to time about this. Sometimes about things that maybe one shouldn't put into the historical record, but ones that had to do with the history of the Institute. I think that some of them were rather characteristic of Veblen. Veblen was politically very shrewd. I remember he told me, in the early days, when the Institute started, he wanted to have one man more from the university with him. Actually, the two people in the math department at that time that would have been the best choices would be Lefschetz³⁴ or Alexander.³⁵ It seems clear to me that Veblen probably felt that Lefschetz was the more difficult personality, so that he really wanted Alexander. He did not say so. What he did was that he presented

³¹ Giles Constable (1929-), Professor in the School of Historical Studies, 1985-2003; Emeritus Professor, 2003- .

³² Chen Ning Yang (1922-), Member in the Schools of Mathematics and Natural Sciences, 1949-1954; Professor, 1955-1966.

³³ Marshall Rosenbluth (1927-2003), Professor in the School of Natural Sciences, 1967-1982.

³⁴ Solomon Lefschetz, 1884-1972.

³⁵ James W. Alexander (1888-1971), Faculty in the School of Mathematics, 1933-1947.

it to the trustees that these men were rather evenly matched, and that he would leave it to the trustees to make the choice.

His reasoning, as he explained to me, was the following: the board of trustees, at that time, was rather rather heavily Jewish. Lefschetz was Jewish. Alexander was not. And Veblen felt sure they would pick Alexander!

That is how he explained it to me. Whether it was the best choice is hard to know. It's true that Lefschetz was a difficult personality -- that, everyone agrees on. On the other hand, he was a more active person. It was very good for the university to have retained him, of course, because he helped the university very much. He was a very strong chairman. So in that sense, it was probably better for the university that he remained there rather than Alexander. Alexander, on the other hand, -- after a while, he sort of lost interest in mathematics. You see, Alexander was independently wealthy. His income from a professorship at the Institute was insignificant compared to -- whatever. That can be rather dangerous.

[Pause in recording to change cassette]

Devine So his income at the Institute was insignificant--

Selberg I think, in comparison to what he had inherited from his father. I think that can be dangerous, because he did see the freedom to sort of retire completely from work. The incentive of having to make a living often keeps people working in periods when they otherwise might not. And I think everybody may have periods when his interest at least slackens, for a long period or so. Also, it contributed in Alexander's case that he had some people at the Institute with whom he had a close personal relationship. But Veblen retired, and Siegel left the Institute. Hermann Weyl also retired, and I think when all this happened, that really brought Alexander to the point that he felt -- so he gave up his professorship, and stayed on nominally as a permanent member, but didn't really come around. Except occasionally, he came to visit Veblen, who is still living. But Veblen was greatly handicapped -- he was essentially blind. He could only read by holding very big lenses, because his central vision was rather ruined by, as I understand it, some disease whereby scar tissue was formed on the retina. He had some vision. He could recognize shapes. He might recognize a person, say, walking down the corridor, but not by recognizing the face, but rather by recognizing how his whole body moved. And he couldn't read, really, except by using very extraordinary measures. As I say, Alexander came to visit him occasionally then, and was, to some extent also helpful in some ways, in helping him to obtain certain types of equipment -- big magnifiers, and things that would enable him to extend what little vision he had a bit more. But basically, Veblen was reduced at

that time to having someone to read to him, when he wanted to know what was in print. He couldn't really do anything himself any more that way I spoke with Alexander in those times. It seemed rather pitiful, in a way. What he told me was that he essentially was experimenting with various types of high fidelity, or stereo, equipment, and listening to music. This was in the early days, right at the beginning of stereo, certainly. It seemed to me rather a sad -way of spending one's time! It's fine to do this for some recreation, but to have that as your main occupation -- I don't think he was very happy with what he was doing after his retirement from the Institute, but somehow he didn't have any incentive to snap back into work. I think if he had not had this great private income, he would probably have stayed on, at least gone through the routine duties of administration, reading applications, and so on. And eventually he probably would again have become interested in something.

Devine That may well be true.

Selberg I do believe Alexander is a rather sad case, in a way. He was only in his fifties, I believe, when he withdrew from the Institute.

Devine Well, according to Professor Tucker, in his later years he began to be very nervous about crowds.

Selberg I think that may be aggravated when you start withdrawing from things. If you all the time have to go out and see people, and so forth, then these things may not develop so much.

Devine I guess that by the time you came -- was he not doing very much? He was a topologist --

Selberg Yes. Well, when I came, he was still here. During my first year, I had no contact with him. I saw him later, around 1950, in the early years after I had come back as a permanent member. There was a time when we had de Rham³⁶ here. There was a seminar on things that -- that was the last time I saw Alexander really being interested in something in mathematics. He came to this; he had occasionally even some comments. It was a seminar on de Rham-Hodge theory, something that I don't really know that much about. I knew a little bit at that time, but what I knew I have forgotten, because I didn't further engage in that sort of thing myself. I did go, at least so that I had little inkling of what it was about at that time. And I do remember that Alexander was there, but that was the last flicker of interest in mathematics, as far as I know, on his part. It was a somewhat sad case, I think.

Devine Well, certainly Hermann Weyl, as he was growing older --

³⁶ Georges de Rham (1903-1990), Member in the School of Mathematics, 1950, 1957-1958.

- Selberg Oh, yes.
- Devine -- got a lot of satisfaction in working with the younger people, and giving seminars.
- Selberg Oh, yes. And Siegel kept working well into his eighties, after he had returned to Göttingen. I think that the normal thing is to keep on working unless you have some disease that impairs your ability to. That can of course happen. Not to speak of Alzheimer's.
- I don't know any mathematicians who have Alzheimer's. I know of some who have Parkinson's disease. Szego, at Stanford, had Parkinson's for quite a number of years, and would probably have been quite active, if that had not happened.
- Devine After you came to the Institute, you did some work here on automorphic forms, and I think Harish-Chandra³⁷ also did some.
- Selberg Yes.
- Devine Did you work together?
- Selberg No, we did not work together. Actually, we came to these things from quite different sides. Harish-Chandra thought about the physics, and became interested in group theory and representation theory originally by working with the Lorenz group in physics. With me it was quite different. I had been interested in -- not automorphic forms and functions in general, but modular forms. At the quite early stage, that interest was really awakened partly by Ramanujan and partly by Hecke. I would say, mainly by his lecture which I did not go and listen to in '36.
- But I started to do some work, and did work mainly on such subjects until I switched over to the Riemann zeta-function in '41 or so. And most of the work I did at that time was not published. I only published a few small things, but I did have a lot more I did not publish.
- Then what happened was, I think in 1949, a paper appeared by a German mathematician Hans Maass, which raised some rather interesting problems. You see, earlier the automorphic functions and forms -- one had essentially thought of functions that were often called holomorphic, analytical. And Maass started studying functions that were not of that nature, but were instead solutions of a certain partial differential equation of second order -- solutions of a certain eigenvalue problem, which had a certain type of behavior with respect to the discrete group which

³⁷ Harish-Chandra (1923-1983), Research Assistant in the School of Mathematics, 1947-1949; Member, 1955-1956, 1961-1962; Professor, 1963-1983.

corresponds to the modular forms. Maass also worked essentially just on the modular group and its subgroups, not on general groups.

At any rate, I saw this paper, I saw some of the open problems that were left there. I thought they could be attacked in a different way by using some ideas that I had had before and experimented with, but only in connection with the analytic, holomorphic modular forms. That's what I started with, and it turned out that I could use them to answer some of these open problems, but then, as often happens, my focus of attention shifted because I saw that I could also do some things that I thought were very much more interesting. This was really then the path that led me to what is called the trace formula, which I established in various stages, first looking just at the hyperbolic plane, and the modular group, and also at other groups as long as they had compact fundamental domains, which the modular group does not have.

Then it was a big problem to handle the general case of a non-compact fundamental domain of a finite volume. I did succeed in that in the summer of '53. The other things, I think I had more or less in line by '51 for the compact fundamental domain, and for the modular group, and the arithmetic subgroups, the congruent subgroups of the modular group.

I also started to look at higher dimensional spaces, and see if I could do that. In particular, first in the case of compact fundamental domains, and also the problems that would arise if the fundamental domain was not compact. That was, of course, much more complicated in higher dimensional spaces because, simply, the type of non-compactness that could occur was -- not only that it was in general more complicated, but there also was a variety of possibilities to choose between. There is, in the hyperbolic plane, only one type of non-compactness that can occur, so that there was essentially only one type of thing to consider.

So this is what led me into -- when I started looking at the higher dimensional cases I had to try to find out more about the groups, which I didn't know anything about. Before, I had never studied any Lie-group theory. I still haven't, really. I mean, also in group theory I mostly worked with my own methods, but I did of course have to deal with certain questions which, for some reason, nobody had considered before, concerning the possibilities of groups in higher dimensions.

This led also to these first results, about the rigidity of groups in the higher-dimensional cases, which are by now rather completely resolved. The first results about rigidity I think date back to '57, when I proved the first results for irreducible groups on product spaces of hyperbolic spaces of dimension two and three. Then later I obtained somewhat more general results.

- Devine I guess André Weil, after you had done group work, he was one of the people who was interested in that as well.
- Selberg Yes. He took up some of the questions, yes.
- Devine Was this before or after he came to the Institute?
- Selberg It was after he came to the Institute. He came to the Institute in '58.
- Devine Just a little after Borel?³⁸
- Selberg I think Borel came in '56 or '57. One can look these things up. But with André, I know it was '58. There are good reasons why I remember it.
- Devine You were on the faculty already in 1958, so you must have been --
- Selberg Well, I was a permanent member from '49, and then a professor since, I think, 1951 -- yes, the summer of '51. The permanent member status in our school now has fallen into disuse. It was felt that it didn't serve any legitimate purpose. I can see, in a way, that it could serve a purpose -- if you have someone whom you want to support, and think that this would be a good place for him, but for some reason or other you feel that he is completely unsuited for taking part in faculty meetings or school meetings.
- Devine I see you smiling -- are you thinking of any particular permanent member?
- Selberg That would be, to my mind, the only good reason for having this possibility. I would not argue with what the other schools do, but I think in our case we didn't really need this, and I doubt that they'll ever revert to using that position.
- Devine It's been kind of a grab-bag position. Originally, of course, there was no intention by Flexner or Veblen to have that kind of a job, but Einstein³⁹ wanted it, so --
- Selberg Yes, but I think Walther Mayer⁴⁰ got really a designation that has never been used for anyone else. He was the only associate professor the Institute has ever had. He was called "associate professor." Yes, and the reason was: Einstein wanted him very much. Clearly, the mathematicians did not feel that he really should be a full-fledged faculty member. This was at an early stage, so they took a designation that was used in American universities. Only later did it occur to them to create a new thing

³⁸ Armand Borel (1923-2003), Member in the School of Mathematics, 1952-1954; Professor, 1957-1993; Emeritus Professor, 1993-2003.

³⁹ Albert Einstein (1879-1955), Professor in the Schools of Mathematics and Natural Sciences, 1933-1946; Emeritus Professor, 1946-1955.

⁴⁰ Walther Mayer (1887-1948), Member in the School of Mathematics, 1933-1948.

called 'permanent member'- I don't know who was the first permanent member. It may have been Gödel.⁴¹ It could have been in one of the other schools, because in the School of Historical Studies there were, when I came here, several permanent members.

Devine I see. I think that's Neugebauer's⁴² title.

Selberg Yes, but there are several others. Most of them are probably by now dead, but if you would look in the early lists of membership of the Institute, you would see that in the School of Historical Studies there would be quite a number of permanent members. Now, some of them had positions, really, elsewhere, so they weren't really here full-time. As a matter of fact, I don't know that any of them were full-time, so maybe Gödel was the first one for whom this was used who really was here full-time.

Devine But he did eventually become a professor.

Selberg Yes.

Devine And was quite a nuisance in the faculty meetings.

Selberg You see, what I know about this is what Veblen has told me -- I mean, what I know about Gödel. When he came, he was considered to be somewhat unstable mentally, and that was probably one reason. Actually, according to what Veblen told me, the association between Einstein and Gödel arose in the following way. Veblen felt that he had to look out for Gödel, and spent quite a lot of time talking with him. And then, he thought that he might perhaps get Einstein to take over part of this responsibility. And that seemed to go so extremely well that Veblen removed himself, essentially, from the picture. Einstein and Gödel remained very close. They tended to come to the Institute together, and leave the Institute together, very often. Of course, Gödel's interest in the theory of relativity undoubtedly goes back to this association with Einstein. I don't think he had any interest in physics before that. I know he had some philosophical interests, but I think the specific interest in the theory of relativity, in which he did write some papers and create some results of significance, that goes back to that association.

It was clear that, I believe, Hermann Weyl and Siegel certainly, after Veblen had retired, they did not want to have Gödel as a faculty member. So it was only later, after Siegel was out of the picture and Hermann Weyl was retired, this was, I think, initiated by von Neumann. It was then Gödel became a full faculty member. Of course, he was rather peculiar in many

⁴¹ Kurt Gödel (1906-1978), Member in the School of Mathematics, 1933-1935, 1938, 1940-1953; Professor, 1953-1976; Emeritus Professor, 1976-1978.

⁴² Otto Neugebauer (1899-1990), Member in the School of Mathematics, 1945-1946, 1950-1966; Member in the School of Historical Studies, 1950-1990; Member in the School of Natural Sciences, 1966-1990.

ways. He was, I would say, from some points of view, rather difficult to handle at school meetings. He tended to reason, in a sense, probably impeccably logically, but his premises were usually rather different than those of other people.

Devine I see. Could you give me an example?

Selberg Well, even his use of the language was often very strange. I remember once I talked with him at a school meeting, he described someone as "excellent." It turned out, when I questioned him, that for him "excellent" did not mean at all as much as, say, "very good." That's, of course, very contrary to the interpretation that most people give. Because "very good" is fine, but "excellent" would for most people seem to mean about the same as "extremely good."

Devine Yes, it would seem to be excelling over, say, "very good," that's right.

Selberg So even in assessing the various shadings of language, you often wondered what he meant by his --

[Pause in recording to change cassette]

Devine So with Gödel, you often wondered what he meant by some of his statements.

Selberg Yes, it was a problem. He had another idiosyncrasy, and this was that it was very hard for him to end a conversation. I was the executive officer of the school for a number of years, and -- I have to go into some of the conflicts that we had. This was after the first Milnor⁴³ affair, if I may call it that.⁴⁴ This led to rather high disagreements and high tempers within the faculty. After that, I deemed that it was better if the school meetings of mathematics would take place without the presence of Oppenheimer. Earlier, Oppenheimer had been sitting in on the school meetings. They were actually held in his office. I thought this was something -- it was better if the school did not take a vote on it, that it was just the personal responsibility of the one who was arranging the meetings. So I sent Oppenheimer a note to this effect, that I would call, from now on, the meetings in my office, and that I would appreciate it if he would not try to attend, but that I would keep him informed of the agenda in advance and come to him with the minutes after the meetings and go over the items, and keep him informed in that way. But I thought, under the circumstances, it was better to avoid the friction that I considered inevitable if he was there. He agreed to try this. So we began to meet in

⁴³ John Willard Milnor (1931-), Member in the School of Mathematics, 1966; Professor, 1970-1990; Visitor, 1999, 2002.

⁴⁴ A good account of this conflict is given by Armand Borel in his article in *A Century of Mathematics in America: Part III*.

my office. Gödel did come to the first meetings, but then he raised objections. Essentially, the background is the following: for Gödel it was essentially so that all authority comes from God.

So that meant -- Yes. Surely, I have always thought that if Gödel had remained in Austria he would probably, although he might personally have reservations, he would probably have accepted the Nazi authorities as also representing God.

Devine Good thing he left.

Selberg Yes, he did leave. I think part of the reason that he left was that he didn't feel he had any position. Actually, there is something about this in the obituary that Kreisel⁴⁵ wrote for Gödel in -- the Royal Society in England publishes obituaries of members. Kreisel had a rather close association with Gödel over many years, and also Kreisel is from Austria. George Kreisel. He wrote about this. You see, apparently Gödel had had some difficulties in Austria, because some people thought he was Jewish, which he was not. According to Kreisel, he considered this to be typical Austrian *Schlamperei*, as he called it, and actually was supposed to have said that the Germans will never make such mistakes.

Devine Gödel was supposed to have said that?

Selberg Yes, that it was typical Austrian *Schlamperei*, meaning that Austrians are not as efficient and exact as the Germans! Which might well be true, for all I know! As far as I know, it was so that he ended up without a proper position. It was, I think, for this reason, that he was passed over. This was the reason that he -- you may look up this obituary.

Devine I will look it up. It's in the proceedings of the Royal Society?

Selberg Yes, I think they publish some separate volumes with obituaries. So Gödel withdrew from the meetings and said that he could not take part in our school meetings because they were then without the director. Well, I thought we could try to manage a way around that, by the following scheme: I would habitually consult Gödel about the applicants that were in the areas that he was interested in -- foundations, and logic, and so on. I would then find his opinion about them, and I would cast his vote for him, and I promised that I would also add my own. It didn't work that way, because it was very hard to get an opinion out of him. Also, he tended to take those applications that he was interested in and sit on them for a long time, so that I really had to try to badger him a bit to get them back so that other people could also see these applications .

⁴⁵ Georg Kreisel (1923-2015), Member in the School of Mathematics, 1955-1957, 1963-1964; Visitor, 1960.

As I say, I wanted then to have a list of his preferences, but he apparently had great difficulties making up his mind. I often had phone conversations with him, very often that were initiated by him, he was complaining about something, and the problem was that he didn't know how to end them. I felt that if he initiated them, really it was he who should end them also. He would go over the same thing, again and again, and I would answer the best I could. He would repeat -- and finally I had always to find some excuse. It was the only way I could get out of it.

Devine Did you know his wife at all?

Selberg I didn't really know her. I knew her, of course, because I had seen her, but I had not really talked with her.

Devine Deane [Montgomery] said he used to sit next to her at Oppenheimer's parties.

Selberg Ah ha. That may well be. I don't think I ever sat next to her at any party. I don't know, but this may indicate that Deane was not particularly in Oppenheimer's favor!

Actually, I did see her from time to time. The Institute used to have a spring dance, as it was called in the Oppenheims' time, and Mrs. Gödel was very fond of dancing. He did not dance. They usually were there, undoubtedly at her request. I am sure he had no interest in coming. I believe that she had been some kind of entertainer before they married -- she may possibly have been a dancer.

Devine I think she was an actress.

Selberg Well, maybe an actress. Some type of entertainer. I think probably Kreisel would have that information also. It's probably there. I don't remember what it was.

Devine Well, I want to thank you very much.

[Recording stops and then resumes]

Selberg He was American.

Devine Mordell⁴⁶ was American? I thought he was European.

Selberg When he was sixteen years old, he traveled on his own to Cambridge to take part in the tripos, these examinations -- and got a scholarship! It's rather an astounding story. Actually, I heard that from him. That a boy that age would have the audacity to do this on his own -- he must have had great confidence in his own abilities as well. Fortunately for him, it was justified! It would be otherwise very dangerous, and I think for most people

⁴⁶ Louis J. Mordell (1888-1972).

it would have been a rather disastrous thing to attempt something like that. But it shows a kind of -- I couldn't have imagined that I would have done something similar at that age, although I had when I was sixteen started with mathematics.

Devine I wonder -- was there somebody special at Cambridge he wanted to work with?

Selberg I don't think so. I don't know why he would go to England, first of all. But there may not have been much in the way of fellowships available in the United States at that time. Because this must have been comparatively early during the century, certainly before the First world war.

Devine That would make it more understandable, then, because I'm told there wasn't that much real mathematics here then.

Selberg Well, Veblen had already started something at Princeton, at that stage. And surely George Birkhoff was active somewhere, I don't know where he was at that time.

Devine George Birkhoff was at Princeton for a while --

Selberg He was also at Princeton. Princeton was really the best place already then.

[End of interview]