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THE LORD OF THE NUMBERS, ATLE SELBERG. ON HIS LIFE AND MATHEMATICS

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The renowned Norwegian mathematician Atle Selberg died on 6 August, 2007, in his home in Princeton. He was one of the giants of the twentieth centuries mathematics. His contributions to mathematics are so deep and original that his name will always be an important part of the history of mathematics. His special field was number theory in a broad sense.

Atle Selberg was born on June 14, 1917, in Langesund, Norway. He grew up near Bergen and went to high school at Gjøvik. His father was a high school teacher with a doctoral degree in mathematics, and two of his older brothers, Henrik and Sigmund, became professors of mathematics in Norway. He was studying mathematics at the university level at the age of 12. When he was 15 he published a little note in *Norsk Matematisk Tidsskrift*.

He studied at the University of Oslo where he obtained the Cand. real. degree in 1939, and in the autumn of 1943 he defended his thesis, which was about the Riemann Hypothesis. At that time there was little numerical evidence supporting the Riemann Hypothesis. He got the idea of studying the zeros of the Riemann zeta-function as a kind of moment problem, and this led to his famous estimate of the number of zeros. From this it followed that a positive fraction of the zeros must lie on the critical line. This result led to great international recognition.

When Carl Ludwig Siegel, who had stayed in the United States, asked Harald Bohr what had happened in mathematics in Europe during the war, Bohr answered with one word: Selberg.

During the summer of 1946, Selberg realized that his work on the Riemann zeta function could be applied to estimate the number of primes in an interval. This was the beginning of the development leading to the famous Selberg sieve-method.

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In 1947 Selberg went to the Institute for Advanced Study in Princeton in United States where he continued the work on his sieve-method. In the spring of 1948 he proved the Selberg Fundamental Formula which later in 1948 led to an elementary proof of the Prime Number Theorem. This was a sensation since even the possibility of an elementary proof had been questioned by G. H. Hardy and other mathematicians.

For these results he was awarded the Fields Medal in 1950—at the time the highest award in mathematics.

He became a permanent member of the Institute for Advanced Study in 1949 and a professor in 1951, a position he held until he retired in 1987.

In the early 1950s, Selberg again produced a new and very deep result, namely what is now called the Selberg Trace Formula. Selberg was inspired by a paper by H. Maass on differential operators, and he realized that in this connection he could use some ideas from his Cand. real. Thesis. The Selberg Trace Formula has had many important implications in mathematics and has also been applied in theoretical physics, but Selberg was never interested in the wide range of applications. In his Trace Formula, Selberg combines many mathematical areas like automorphic forms, group representations, spectral theory and harmonic analysis in an intricate and profound manner. The Selberg Trace Formula is considered by many mathematicians to be one of the most important mathematical results in the 20th century. His later works on automorphic forms led to the rigidity results of lattices in higher rank Lie groups.

In his later years he continued to work on his favourite subjects: sieve-methods, zeta-functions and the Trace Formula. In 2003 Selberg was asked whether he thought the Riemann Hypothesis was correct. His response was: "If anything at all in our universe is correct, it has to be the Riemann Hypothesis, if for no other reasons, so for purely esthetical reasons." He always emphasized the importance of simplicity in mathematics and that "the simple ideas are the ones that will survive". His style was to work alone at his own pace without interference from others.

In addition to the Fields Medal in 1950, Selberg received the Wolf Prize in 1986 and then in 2002 an honorary Abel Prize prior to the regular awards. He was also a member of numerous academies.

Atle Selberg was highly respected in the international mathematical community. He possessed a natural and impressive authority that made every one listen to him with the greatest attention.

He loved his home country Norway and always spoke affectionately of Norwegian nature, language and literature. In 1987 he was named Commander with Star of the Norwegian St. Olav Order.

In November 2005 we—Nils A. Baas and Christian F. Skau—visited Selberg at the Institute for Advanced Study in Princeton and interviewed him at length about his life and mathematics. The interview took place on 11, 14 and 15 November in Selberg's office in Fuld Hall. One part is an audio tape, but most of it is a videotape. The interview took place in Norwegian, and it has all been transcribed in Norwegian. The video tape is more than six hours long. A short 20 minute version with English subtitles was shown at The Selberg Memorial at The Institute for Advanced Study on 11 January 2008. A longer version at about an hour was shown on Norwegian Television on 13 October 2007. The (edited) complete interview will appear in Norwegian in four parts in volume 56 (2008) of *Normat* (*Nordisk Matematisk Tidsskrift*); the first two parts have already appeared.

Syracuse 26/9 – 48

Kjære Sigmund,

Sender dig hermed en skisse av beviset, eller rettere et av bevisene eftersom den siste del kan varieres svært meget.

Definisjoner: (1) $\vartheta(x) = \sum_{p \le x} \log p$

(2) $\vartheta(x) = x + R(x)$, p, q og r betyr alltid primtall. Ellers bruker jeg at

(3) $\sum_{p \le x} \frac{\log p}{p} = \log x + O(1)$, samt $\vartheta(x) = O(x)$

1. Bevis av grunnleggende formler: La x positive og d pos. helt tall og sett

$$\lambda_d = \lambda_{d,x} = \mu(d) \log^2 \frac{x}{d},$$

og hvis n er hel, positiv

$$\theta_n = \theta_{n,x} = \sum_{d|n} \lambda_d$$

Da er

$$(4)\quad \theta_n = \begin{cases} \log^2 x, & \text{for } n = 1 \\ \log p \log \frac{x^2}{p}, & \text{for } n \text{ potens av primtall } p. \\ 2 \log p \log q, & \text{for } n = p^\alpha q^\beta, \\ 0, & \text{for alle andre } n. \end{cases}$$

De 3 første deler av (4) følger umiddelbart av definisjonen, den siste del følger ved induksjon, idet vi for enkelthets skyld antar n kvadrat-fri, har vi for $n = n'p$

$$\theta_{n,x} = \theta_{\frac{n}{p},x} - \theta_{\frac{n}{p},\frac{x}{p}}$$

Av dette følger siste del av (4) enkelt.

We have been asked to provide a translation of some of the most interesting parts of the interview. Here we have made a selection. Some editing and reorganizing have been necessary but without changing anything essential.

Of special interest is Selberg's personal account of the events around the elementary proof of the Prime Number Theorem. Various accounts of this have appeared in the literature.* In an extended version of the translation more details of Selberg's account may be found. In this extended version Selberg refers to two letters by Hermann Weyl to Nathan Jacobson who was the editor of the *Bulletin of the American Mathematical Society* at the time. All this can be found at `http://www.math.ntnu.no/Selberg-interview/`. (Confer also *Normat* **56**#2 (2008).) Here also is available the complete transcription of the interview in Norwegian, in addition to some photos and material in connection with his 90th birthday and his death.

The front page cover (which is reproduced here on the preceding page) is the first page of a handwritten letter Atle Selberg wrote to his brother Sigmund, dated 26 September 1948, in which he gives his first written version of the elementary proof of the Prime Number Theorem. It is written in Norwegian.

The interview

Question. When did you realize that you had a special talent for mathematics?

Well, I will tell you that the first time I remember—we lived then at Nesttun near Bergen—I must have been 7 or 8 years of age, and we were engaged in playing some sort of ball game, some boys in the neighbourhood and I. I think it was what we called "langball", which is a kind of softball game. During such a game one often has time to stand and wait and not do anything. Then I often made mental calculations. I looked at the differences between consecutive squares and saw that one got odd numbers. I managed to find a proof of that. I did not use letters or symbols at that time, but by thinking about the squares of the number and the number plus one I inserted the product of the number and the number plus one. Then I could easily find the differences on both sides. So I discovered that by adding consecutive odd numbers I got squares all the time, and I thought that was somewhat interesting. A little later I found by the same reasoning that, as we would express it, $A^2 - B^2$ equals $(A + B) \cdot (A - B)$. This can be shown of course by inserting AB between the two squares; then one can see the differences on both sides. The latter helped me quite a lot in doing mental calculations. One can simplify a lot of things in this way, especially because squares are easy to remember a long way upwards.

Question. How would you compare that with Gauss, who as a child was asked by his teacher to find the sum $1 + 2 + 3 + \cdots$ etc. up to 100?

Yes, yes, that I think was better done. I am not so sure that I would have thought of something like that. But nobody asked me to add the numbers from 1 to 100.

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*D. Goldfield in *Number theory*, New York Seminar 2003, edited by Chudnovsky, Chudnovsky and Nathanson, Springer Verlag Yellow Series.

B. Schechter, *My brain is open*, Simon & Schuster (1998).

M. de Sautoy, *The music of the primes*, HarperCollins Publishers (2003).

Question. Did you tell anyone, or did you discuss this discovery of yours with your father?

No, I did not do that. The discovery I had made was an interesting experience that I can remember even today. The regularity I had managed to establish made a deep impression on me—that it was true in general, and not only in concrete examples. It was some years later that I began to read a little. My father had in his relatively large collection of mathematical books also some textbooks from Denmark. The Danish books were of a higher quality than the Norwegian ones, and they were clearly written by better mathematicians. I took a look at the Danish textbooks, and I learned how to solve quadratic equations with one unknown, and linear equations with several unknowns—by elimination, not with determinants. Determinants I encountered much later, and I must confess that I did not like determinants that well, but later I found out that they could be quite useful. Then I started to read more advanced mathematics. I discovered Störmer's[†] lecture notes in mathematics. My father had a fairly old edition which was hand written. I often leafed through the book, and I found the formula

$$\frac{\pi}{4} = 1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \cdots$$

which I thought was very strange, because I knew already what π was in connection with the circle. So I made up my mind to find out how this could be, and I began to read the book carefully from the start. It was a wonder that I did not give up because the book started with introducing the real numbers by using Dedekind cuts. I read through it and I could not comprehend what this should be good for. I thought I had a pretty clear concept of real numbers, which I thought of as decimal numbers, perhaps infinite decimals. I must say that Euler undoubtedly had a clear concept of what a real number was, so there is no reason to think that it originated with Dedekind. I could not understand the purpose or usefulness of this introduction of real numbers in Störmer's lecture notes, but I did read through it. After I had finished that section of the book the material began to be interesting to me. Even today I think Störmer's lecture notes are very good, and it was very unfortunate, I think, that they were substituted with Tambs Lyche's[‡] textbooks. In a certain sense, of all the mathematical literature I have read, Störmer's book is perhaps the book that has meant most for my mathematical development!

Question. In Störmer's lecture notes you became acquainted with continued fractions for the first time?

Continued fractions I thought were interesting. Among other things, I found out that they had some connection with what, for one reason or another, is called Pell's equation. In reality it has nothing to do with Pell. André Weil once said that if something in mathematics gets attached to the name of a person, then the person in question usually has very little to do with it.

[†] Carl Störmer (1874–1957), Norwegian mathematician, professor in mathematics at the University of Oslo, 1903–1946.

[‡] Ralph Tambs Lyche (1890–1991), Norwegian mathematician, professor in mathematics at Norges Tekniske Høyskole (NTH), Trondheim, 1937–1949, and at the University of Oslo, 1950–1961.

Question. How old where you when you began to read Störmer's book?

I believe this was the summer before I started in 7th grade, so this must have been around the time I turned 12 years of age.

Question. You had no benefit of the mathematics education you were exposed to in school, did you?

I read no geometry. I first encountered the trigonometric functions as power series, and also through Euler's formulas for sine and cosine in terms of e^{ix} and e^{-ix}.

Question. But did you later became interested in geometry?

Only if I could have some use for it, so to say. In what I have done later, I have sometimes needed to use some geometric considerations. I thought it was easier, however, to deal with symbols, and to use analysis and the like, even when I was more interested in applications to discrete problems. I was never particularly interested in general function theory. I liked the special functions—elliptic and automorphic functions, for example—and especially modular functions and modular forms and the like. The general analytic function I thought as interesting as the general real number. One is basically not so interested in that. The proletariat of real numbers is not so interesting in a way, even though it can be difficult to decide their nature, whether they are irrational or algebraic, or whatever it can be. For example, Euler's constant—nobody yet knows its nature.

Question. What about Riemann surfaces?

Of course, when I read about function theory I encountered Riemann surfaces. But I was more interested in algebraic Riemann surfaces than the general concept, and also in uniformization theory and automorphic functions.

Question. Did your interest in automorphic forms coincide with your discovery of Ramanujan's works?

Yes, yes, this started with Ramanujan, and that was my first contact with it. It started not with general automorphic functions or general groups, but with the modular group and the classical modular function which is associated to it, and also with subgroups of finite index—these were the objects I studied.

But let me backtrack a little: Originally it was mostly analysis that interested me, but my brother Sigmund got my interests shifted towards number theory. Not towards diophantine equations, which had interested my brother in high school and his early student days. That never appealed to my imagination, but Sigmund made me aware of a book in my father's mathematical library which contained a section on Tchebycheff's work on the distribution of prime numbers. I read that, and from that moment I was completely dedicated to this area of mathematics. It was also Sigmund who brought home Ramanujan's Collected Works from the university library in Oslo during one summer vacation. This was some time after Störmer's article about Ramanujan appeared in *Norsk Matematisk Tidsskrift*, a Norwegian mathematics journal, which I read. All this caught my interest and gave me the impetus to study and work with discontinuous groups, modular functions and modular forms, and more general automorphic functions and forms. These things have always been my main interest later in life.

My brother Sigmund, in fact, was the only one I discussed mathematics with while I was in Norway. There were others that were helpful in their way, my brother Henrik and Professor Störmer, in particular. Henrik typed my first article when I came to Oslo in 1935 to study. He wrote in the formulas—he thought that my handwriting was not good enough—and introduced me to Störmer, who then subsequently presented my article for *Videnskapsakademiet* in Oslo [the Norwegian Academy of Science and Letters] the same autumn.

Question. Let us return to the school: Did you follow the usual teaching there?

I read some foreign languages on my own. I had already started to learn English when I was in elementary school. I had found a copy in my father's library—not the mathematical part of it!—of *Alice in Wonderland*, so I got interested in the illustrations, and I wanted to translate the text word for word. It was very cumbersome, of course, but I got an older sister of mine to read and translate the text for me. It was my oldest sister, Anna, who did this, and it was very kind of her. I do not know if she was particularly interested in the book herself.

Question. Can you tell us about your first discovery in mathematics that resulted in a paper?

My first discovery was the thing I mentioned about differences of square numbers! I read a great deal in various mathematical books, but I did not make any discoveries that are worth talking about. There were some particular things, like finding a connection between the integral

$$\int_0^1 \frac{dx}{x^x}$$

and the series

$$\sum_{n=1}^{\infty} \frac{1}{n^n}.$$

It is a relatively simple proof if one knows about the gamma function and Euler's integral for the gamma function. Then it is an easy formula to show.

Question. How old were you then?

That was some years later. I was 15 years old, perhaps.

Question. The solution to that problem was published in *Norsk Matematisk Tidsskrift* in 1932, so you were 15 years old?

In 1932? What time of the year? I do not know precisely how long it took before it appeared. It was not me that sent it to the journal; it must have been my father. The formula in question can also be generalized.

Question. Can you tell us about the discovery you made when you read about Ramanujan and Hardy's work on the partition function?

That was what led to my first article: "Uber einige arithmetische Identitäten". I spent quite some time studying Ramanujan's so-called mock theta functions. There was an English mathematics professor, G. N. Watson, who had written about such functions of the 3rd and 5th order, as Ramanujan had called them, and shown relations between these. Ramanujan had also introduced something that he called mock theta functions of 7th order. By utilizing some of the things I had shown that led to my first article, I managed to prove that these functions had the property that Ramanujan had defined, namely that they could be approximated by irrational points, as one may express it, on the unit circle; that is, they could be approximated as well as a modular form. So I started to take a look at Hardy's and Ramanujan's article on "partitions", and I found the exact formula. But that turned out to be a disappointment! You see, I had finished my investigation of the partition function in the summer of 1937, and when I came to Oslo and looked in Zentralblatt, I found the review of my first article. On the same page was the review of Rademacher's article on the partition function. I had something that Rademacher did not have, and that was a much simpler expression for the coefficients that appear in the relevant series. This was undoubtedly something Ramanujan would have done if he had been at his full power when this work was under way. In fact, the inverse of the function that generates the partitions is in reality a theta function, and the root of unity that occurs in the transformation formula in front of the theta function can always be expressed as a kind of Gaussian sum. If one does that, it is clear that the series for the partition function is transformed by the inverse root of unity and conjugation. Doing this and inserting it into the definition of these coefficients, which are denoted by $A_q(n)$ for term number q in the series for the partition function $P(n)$, one gets a rather simple series that exhibits the order of magnitude for these coefficients. The convergence of the series is obvious. But this is something that in a way should have been done by Hardy and Ramanujan; but I think that it was Hardy that prevented them from reaching the final result because Ramanujan had already been on to the right formula earlier in the letters he wrote from India to Hardy, before he came to England. But at the time their joint article was written, there is no doubt that Ramanujan was not well. He probably suffered from vitamin deficiency; he only ate food that was sent to him from India. He had no fruit or vegetables or other fresh things, only what could be dried. He obviously suffered from quite serious nutrition deficiency.

Question. Can you tell us about the disappointment you had when you discovered that Rademacher had got at this result before you?

I made up my mind not to publish this result about the coefficients that I mentioned. I thought it was too little to write about. But I decided to do something else, and what I decided to do was the thing I talked about at the Scandinavian Mathematical Congress in Helsingfors in 1938 (we said "Helsingfors" at that time, not "Helsinki"). I gave a short talk lasting 20 minutes. It was the first talk I had ever given. I met quite a few mathematicians at that congress. For example, I met Lindelöf there, and Carleman was there. Carleman chaired the session when I gave my talk and he was very friendly towards me, I must say, and so was Harald Bohr. What otherwise made a strong impression on me at that congress was a talk that Arne Beurling gave. It contained quite a few things; for one thing he talked about his generalized prime numbers and the generalization of the Prime Number Theorem in that connection. As I told you, it made a deep impression on me. In 1939 I had obtained a stipend, a travel grant, which I intended to use for a travel to Hamburg in Germany to see Erich Hecke. A talk by Hecke at the ICM congress in Oslo in 1936 had made a great impression on me. I did not go to listen to his talk – I did not have enough sense to do that – but I read it later when the Proceedings from the congress was published. That was the article that made the greatest impression on me of all the articles contained in the Proceedings. So I wanted to travel to Hamburg to see Hecke. I had already finished my Master Degree (Cand. real.) at the University of Oslo during the spring term of 1939, and I had finished the first part of my mandatory military service by the summer of 1939. As I had already finished my university studies it made sense to travel to some place to get new impulses. However, the Second World War started at exactly the time I had finished my military service that summer, and I decided not to travel to Hamburg. So instead I travelled to Uppsala in Sweden. I had heard that they had a very good mathematical library located at the Department of Mathematics.

In Oslo at that time it was very cumbersome. They did not have many monographs at Blindern, where the Department of Mathematics was located. Mathematical journals and the like were scarce, and one had to go to the University Library which was located at Drammensveien. We were not allowed to go and look for things ourselves, but we had to look in the catalogue and then order. The University Library was located very inconveniently for us—it was cumbersome to get there from Blindern. Things were very much better in Sweden. So I travelled to Uppsala instead of Hamburg, and thought that Beurling would be there. But it turned out that he was drafted into military service to work at the cryptography unit, or "the cipher unit", as they called it, and where he made some impressive piece of work during the Second World War. He was very talented in this direction. I met Beurling only once while I was there. It was on a Sunday when I was sitting and working alone at the library of the Mathematics Institute, that Beurling came. I recognized him from the Scandinavian Congress in Helsingfors the year before. I talked with him, but apart from this encounter he was of no use for me, simply because he was not there. Nagell was there. He gave some lectures that I attended. But most of the time I sat at the library, which was very good. They had a lot of journals there, so I had much better access to the literature than I would have had in Oslo—I mean *easy* access to the literature. In Oslo it was, as I said, more complicated to get hold of these things.

Question. When did you start to get interested in the Riemann hypothesis—was that at this time?

No, that came later. That was after the military campaign in Norway in 1940. I also had another disappointment, I must say. When I came to Uppsala, I saw an article where I learned something that I did not know before. I had no knowledge about hyperbolic geometry, and, in particular, I had never before heard about the measure

$$\frac{dx\,dy}{y^2},$$

which is the invariant measure with respect to the hyperbolic geometry in the upper half plane. I learned that by looking at one of the German journals that arrived while I was there. Then it dawned on me that I could do something that I had already done in not such a good way for the modular group in my Master Thesis, and I sat down and wrote an article about what today is called the Rankin-Selberg convolution. If you have two modular forms, you can form a Dirichlet series, where the coefficients are the products of the corresponding coefficients of the two modular forms, and which then has a certain functional equation. I gave the proof for the functional equation and deduced some consequences which I did not give complete proofs of, but only sketched the proofs. I waited until I had returned to Oslo before I submitted this manuscript. That was in the spring of 1940. I had come back to Oslo at the end of December 1939, and I made up my mind not to go back to Uppsala since the weather there was rather nasty in winter. It had been quite pleasant earlier that year, but awfully windy. In the winter when snow falls, it very quickly turns into slush, so it was difficult to keep one's feet dry. I did not like the wind either. Oslo was much better—it had a much better winter climate. Oslo was colder, but much more pleasant. In Oslo there was more sunshine in the winter, and it was not so windy. So I made up my mind not to return to Uppsala, but stay in Oslo in the spring.

It was in March that I saw in Zentralblatt a review of a paper by Rankin. He had not really defined a convolution of two functions. He only operated with one function and the squares of the coefficients associated to it, so that was more special than what I had. He had drawn some consequences of all this. What he had done had only applications to modular forms of the same weight or automorphy factor, while the thing I had defined could be used for two forms of different weights. My idea was somewhat more general than his, but he was unquestionably first. Even if I had submitted my article to Störmer by sending it from Uppsala, the priority would nevertheless belong to Rankin. He had, as I could see from his manuscript, finished his work during spring 1939, while I finished mine in the autumn.

Question. So that was another disappointment?

That was a disappointment, I must say. At this time Siegel came passing through Oslo on his way to the US.[§] He gave a talk that I went to, and it made an impression on me. I had not gone to his talk at the IMU congress in Oslo in 1936. I did not have enough knowledge to make the right choices about which talks to go to. I did hear the talks of others, among them Mordell and Polya, who gave the talks I liked the best of those I heard. I have to say that my brother Henrik sometimes got me to help fill up the auditorium, when it seemed that there would be few listeners. So I listened to certain things that I had absolutely no interest in listening to. On the other hand, Henrik had been so helpful to me in other ways, so I shall not complain.

Then the war came to Norway at the beginning of April 1940, and that caused an interruption of my mathematical research. I did not think about mathematics while I fought with the Norwegian forces against the German invaders in Gudbrandsdalen,[¶] and also not while I was a prisoner of war at the prison camp at Trandum. When I finally was released, I travelled to the west coast of Norway, and later with my family to Hardanger. I wanted to start with something entirely new. I came across a paper by Polya, "Über ganze ganzwertige Funktionen", where Polya had strengthened a result by Hardy a little.

I looked at that paper, and saw that I could sharpen it considerably. So I wrote a paper that was about entire analytic functions which take integer values when the argument is a positive integer. Polya had also another work about entire functions taking integer values when the argument is an integer, that is, either positive or negative. I could make the same improvement on his result also in this case. I also wrote a third paper on entire functions that took integer values, but where in addition the derivatives up to a certain order took integer values.

[§]Siegel left Norway by ship for the United States just days before the German invasion of Norway on April 9, 1940.

[¶]We were also in the upper part of Østerdalen, and ended up near Åndalsnes. I was a soldier in Major Hegstad's artillery batallion, and I held him in very high esteem.

My focus then shifted towards the Riemann zeta function $\zeta(s)$. For s real-valued and greater than 1, Euler had shown the product formula

$$\zeta(s) = \sum_{n=1}^{\infty} n^{-s} = \prod_{p \epsilon \mathcal{P}} (1 - p^{-s})^{-1},$$

where $\mathcal{P}$ denotes the prime numbers. Riemann showed that $\zeta(s), s = \sigma + it$, could be extended to a meromorphic function on $\mathbf{C}$, with a simple pole at $s = 1$, and with the so-called trivial zeros at $-2, -4, -6, \cdots$. The non-trivial zeros lie in the critical strip $0 < \mathrm{Re} s < 1$, and Riemann's conjecture—also called the Riemann hypothesis—is that all the non-trivial zeros lie on the critical line $\mathrm{Re} s = \frac{1}{2}$. I began to think about an idea I had of trying to show the existence of zeros of the Riemann zeta function $\zeta(s)$ on the critical line, by considering certain moments. These were not moments of the zeta function, but by considering integrals of the real-valued function that one can get if one uses the symmetric form of the functional equation

$$\pi^{-s/2}\Gamma\Big(\frac{s}{2}\Big)\zeta(s) = \pi^{-(1-s)/2}\Gamma\Big(\frac{1-s}{2}\Big)\zeta(1-s),$$

where Γ denotes the gamma function, then one gets a function that is real-valued on the critical line. By considering that function and its various moments, and then looking at the sign changes of these moments, one can say something about the zeros on the critical line. I could do something with this, but it did not give as sharp results as the ones obtained by Hardy and Littlewood. I took a closer look at their paper, and I understood the reason why they could not get better results than they got. I discovered, so to say, what was the basic flaw in their approach, and what they had misunderstood. They made some comments at the end of their article where they showed that

$$N_0(T) > \text{constant} \cdot T.$$

Here $N_0(T)$ denotes the number of zeros on the critical line between 0 and T. Their comments had to do with the variations of the argument, but it became clear to me that this could not be right. I looked at it, and then I saw what one should do: one should try to reduce the oscillation, because the real function that one gets on the critical line is a strongly oscillating function. It has amplitudes that vary highly—some places the fluctuations are small and some places they are very large. When they, that is Hardy and Littlewood, considered the integrals of squares, which they took over short intervals and then computed the average over a long interval, it will be these regions, where the interval is too short and where the amplitudes are very large, that will dominate. By doing this, one will not get information about the average behaviour of the function, but simply what happens when the amplitude is very large. So I came up with the idea to try to mollify and normalize, such that the contributions would be somewhat larger where the amplitudes were small and less where they were large. The first thing I tried to do was to take a section of the Euler product and then take the square root of the absolute value. That gave a result which I wrote up as an article, which I sent to *Archiv for Mathematik og Naturvidenskab.* Then I started to experiment by taking instead an approximation to the series that one gets by looking at a section of the Dirichlet series for $(\zeta(s))^{-1/2}$, reducing the coefficients so they become zero for $n \geq z$, and then use the square of the absolute value of this as the mollifying factor on the line $s = \frac{1}{2} + it$. It turned out that I got better and better results, until I discovered that the best way to reduce the coefficients was to multiply them with the factor $(1 - \frac{\log n}{\log z})$ for $n < z$. Then we get

$$\sum_{n \leq z} \frac{\mu(n)}{n^s} \cdot \frac{\log z/n}{\log z},$$

where μ denotes the Möbius function. I then found the correct order of magnitude for $N_0(T)$ relatively quickly.

It required some work to estimate the sums that occurred in the integrals, but with some patience I managed to obtain the result that $N_0(T)$ was greater than a positive constant multiplied with $T \log T$, which is of the right order of magnitude. I did not try to compute the constant, but if I had been interested in doing that I would have modified the proof to get a better constant. By modifying in the right way, I think one can get a constant that will lie somewhere between 1/10 and 1/20. I have never gone through the computations.

Question. The article you sent to *Archiv for Mathematik og Naturvidenskab*, did it have the right term

$$\text{constant} \cdot T \log T?$$

In the course of the proofreading I added this as a footnote. I sent a short note announcing this result to the Royal Norwegian Society of Sciences and Letters in Trondheim, which was published in their Proceedings in 1942.

Question. This became your doctoral dissertation?

That was what I chose for my doctoral dissertation. I had already published quite a few papers at that time, but I had the idea that a doctoral thesis should be something weighty, not too short, but something that consisted of many pages, and my thesis was 70 pages long. So I wrote the result up and handed it in as my doctoral thesis.

Question. This took place during the war when Norway was occupied by the Germans. Was the result contained in your thesis communicated to Harald Bohr in Denmark? It was a sensational result, was it not?

It was Störmer that presented it to the Norwegian Academy of Science and Letters in Oslo, of course. As opponent Harald Bohr was the obvious choice because there was nobody in Norway that had any real competence in this field. The second opponent was Skolem, who had struggled with this material, of course. It was not really his field, it is safe to say. Harald Bohr could not come to Norway at that time, since Norway was occupied, and Bohr had already fled from Denmark, which also was occupied by the Germans.

Question. Did he stay in Sweden?

He was in Sweden. His brother, Niels Bohr, was already in the U.S. at that time.

Question. How did the defense go?

Störmer read Harald Bohr's report. Skolem had improved on my English writing, and he was right. In fact, I had just recently changed to writing in English. I had decided not to continue writing in German, even though German was the language I mastered best. But I had already started to read more articles in English, especially Hardy and Littlewood.

Question. Later Norman Levinson got a better result with respect to the zeros on the critical line. Did he essentially use your methods and techniques?

He did use the mollifying factor that I had introduced, but he used it on another function. His proof gives a rather good constant, but the problem is that his method works only for the zeta function and for the so-called L-functions that have functional equations that are very simple. If one looks at quadratic number fields, or L-functions that one gets from modular forms that have Euler products, then one can prove results by my method which one cannot obtain by Levinson's method. The reason is that by Levinson's method one gets the result in terms of a difference between two things, and the question becomes whether the thing you subtract is sufficiently small so that something remains. You need a very good estimate for that, and this you can only get when the functional equation is very simple.

It does not work for the quadratic number fields, for instance. For higher number fields one cannot prove anything because the functional equation is too complicated for one to be able to do something about the relevant integrals that one needs to compute.

Question. You defended your doctoral dissertation in the autumn of 1943?

Yes, that is correct, the defense was in the autumn of 1943.[‖]

Question. You, as well as other university students in Oslo, were arrested by the Germans in the autumn of 1943, just after your doctoral defense took place. But then you were released from prison. Did your working conditions become more difficult then?

Yes, especially after the university was closed. I was released after I had been arrested, and the security police told me that I should not go back to Oslo but to my hometown, Gjøvik, where my parents lived. So I spent the rest of the war years there and worked there, except on a few occasions when I went away during vacation time, but then I did not go to Oslo. A couple of times I did travel down to Oslo to consult the literature at the university library, which was kept open, but then I needed special police permission to travel.

Question. During that time you continued to work on the Riemann hypothesis—or did you change subject?

I did extensive work on the zeta function, but I also worked on certain other problems. I wrote two long papers of about the same size as my doctoral thesis. One was about the zeta function, and it dealt with possible zeros off the critical line. The other treated the corresponding problems for Dirichlet's L-functions, but not precisely the same problem, because that I thought was too trivial, but one could make analogies. There was an English mathematician, Paley, who had started to consider something that he called "k-analogues". If one considers all L-functions that belong to the module k, then there is a certain analogy with what one has for a single function if one looks at its behaviour when the imaginary part varies on the critical line. So I wrote up some of these analogies, and I improved some of Paley's results and I made use of these improvements. They were sufficiently sharp so I

[‖]The defense took place October 22, 1943. The University of Oslo was closed by the Germans on November 30, 1943.

could make analogies to other results, to those that I had obtained in my doctoral thesis. If

$$h = \frac{\varphi(k)}{\log k},$$

where $\varphi(k)$ is a function of k that tends to ∞ when k tends to ∞, and $|T| < k^a$, where a is a certain positive constant, then "almost all" L-functions belonging to the module k have a zero on the line $s = \frac{1}{2} + it$ in the interval $T < t < T + h$. This in turn leads to quite a number of results which I obtained later about the value distributions of the L-functions, and also about the value distribution of the zeta function both on and in the neighbourhood of the critical line.

Question. Did you have a new and original way to look at the zeta function, and that by this you succeeded in proving your remarkable results?

Is it true that nobody else had thought about introducing a mollifying factor in this way. Something like that had been used in connection with the study of zeros outside the critical line—this had been done by Bohr and Landau—but it really was not that interesting. It gave weaker results. There was a Swede, Fritz Carlson, who proved the really first "density" results about zeros of the zeta function outside the critical line. He used a section, that is, a finite partial sum, of the Dirichlet series for $(\zeta(s))^{-1}$, where one had the Möbius function $\mu(n)$ as coefficients up to a certain cut-off bound, and then multiplied this with the zeta function. This forces it to lie fairly close to 1 on average when the real part of the variable is greater than 1/2. So Carlson was able to prove some important results about this, and they were the first so-called "density" results.

There was nobody who had tried to do anything on the critical line itself, that is when the real part equals 1/2. Firstly, it is considerably more difficult, and secondly, I guess that nobody thought that it would be particularly useful. I have to say that I understood fairly soon after I started to look into this that a mollifying factor could be very effective. It turned out that it could be even more effective than I initially thought, because I really had not thought that I would be able to obtain the sharp result that I actually got when I obtained the right order of magnitude for the zeros on the critical line. I had not believed that the method would lead so far. It turned out that I did not need to experiment very long before I found the right mollifying factor, and it did not take me a very long time to complete this work. However, it became quite complicated to carry out the computations of all the estimates that were needed. It can be made somewhat simpler if one uses Fourier analysis, something Titchmarsh did later, after the war, but his proof was also complicated. He sent it to me, and I made him aware of some simplifications that he could do. As a consequence, the proof became considerably shorter when he published it. However, I think that in order to find a good numerical estimate, the Fourier integral is not the best method. Then one should rather make a modification of the method I used.

Question. Is it correct to say that your method, perhaps also other people's methods, are of an averaging and statistical character, so to say, such that none of these can lead to a proof of the Riemann hypothesis?

By considering a statistical method one can get quite far, but it will never lead to a proof of the Riemann hypothesis. I mentioned to you that I could use this method to investigate the value distribution of the zeta function on the critical line, and also in the neighbourhood of the line. I obtained quite a few results in this area just before and just after I came to Princeton in 1947, but I did not publish these results at that time. The reason was that I was then more occupied with what I could do with elementary methods in number theory, which was something that grew out of my version of the sieve-method.

Question. We talked with you earlier about the talk you gave at the 10th Scandinavian Mathematical Congress in Copenhagen in 1946, where you seemed to cast doubt on Riemann's conjecture, but you emphatically repudiated that?

What I wanted to emphasize in my talk in Copenhagen was that there did not exist at that time what one could call numerical evidence for the truth of the Riemann hypothesis. The computations that had been done did not go very far, so if there existed zeros outside the critical line, one would not expect them to show up so early. In fact, at that time the computation of the Riemann-Siegel formula for the function on the critical line had only been done for the imaginary part a little more than 1000. There are very few terms that come into play by this, and the function behaves extremely regularly. One also had the matter of the so-called Gram's law, proposed by the Danish mathematician Gram, and as far as the computations went at that time there were only two exceptions to Gram's law. Gram's law would have implied that Riemann's hypothesis was true, but it would also have implied too much regularity for the distribution of the zeros. I knew from the results that I had obtained earlier that Gram's law became more and more wrong. Instead of a few exceptions it would rather be an exception when it was true. As I said, it was to be expected that the first zeros, if there were some that were not on the critical line, would occur a long time after the first exceptions to Gram's law. So the numerical material at the time did not point to what would be the case and other results were only of statistical nature. But I must say that if one believes that there is something in this world that is as it should be, then I think that must be the truth of the Riemann hypothesis. It gives the best possible distribution of the prime numbers, and also what one would expect from a statistical point of view, namely that the deviation from

$$\mathrm{li}(x) = \int_2^x \frac{dt}{\log t}$$

is not greater than the square root of x. It would entail an elegance that is striking. Besides, I must say that I trust Riemann's intuition very much.

Question. Do you expect that there is some kind of regularity in the distribution of zeros on the critical line?

There is undoubtedly some kind of orderliness, but how far that goes is hard to guess. For example, one may ask the question whether the imaginary parts of the zeros are in any way connected with other mathematical constants that we are familiar with. Nobody knows anything about this, of course, but it is not impossible that there is such a connection. In fact, I do not rule out the possibility that there could be a whole lot of regularity that would be quite unexpected, and which remains to be discovered. That could very well be the case. I mean, there is no reason to think that we have come very far towards what can be done some time in the future. It is certainly possible that there may be novel ways to look at this, and which would lead to totally unexpected connections to other parts of mathematics.

Question. Were you the first that made use of spectral theoretic methods?

I really do not know. For that matter, many people—and this goes way back—have surmised that perhaps the zeros are connected in some way with a spectral problem, but nobody had been able to point to something specific. But I think it is of little use to speculate on how soon someone comes up with a positive idea on how to attack this problem and obtain new results. It will happen one day, I believe, but how long we have to wait—or you have to wait!—is hard to guess, of course.

Question. But if you should guess, would you then think that a core of ideas centered around a spectral problem on some type of space, which yet is unknown, will eventually lead to a solution of the Riemann hypothesis?

That is certainly a thought that several people have had. In fact, there have been some people that have been able to construct such a space, if they assume that the Riemann hypothesis is correct, and where they can define an operator that is relevant. Well and good, but it gives us basically nothing, of course. It does not help much if one has to postulate the results beforehand—there is not much worth in that.

Question. Have you yourself worked seriously with the Riemann hypothesis for the last 30 years?

Well, I have thought about it from time to time. Once I had an idea that I thought perhaps could lead to a proof. I followed it part of the way, but I thought it unlikely that it would lead all the way to the goal. It would only have given the proof of the Riemann hypothesis for the zeta function $\zeta(s)$ and for some of the Dirichlet L-functions, but not for all. I have never tried to complete the proof. The idea depended on the fact that I had found a method to approximate $\varphi(s)\zeta(s)L(s)$ by polynomials, where $L(s)$ is an L-function with quadratic character χ such that $\chi(-1) = -1$, and $\varphi(s)$ is an entire analytic function that makes the product real on the line $s = \frac{1}{2} + it$. The fact that the polynomials had the symmetry built into them gave some hope that something could be achieved following this path. The question was what one could say about the zeros of these polynomials. After a while I became more and more convinced that it would not work as I had thought initially. It just seemed unlikely to me. However, I have now and then seen that people have attacked a problem in a way that seemed “hare-brained”, to use an English term, but then it turned out that they could make it work. They have proven something that would not be easy to prove in another way. On the other hand, I have seen people have ideas that seemed absolutely brilliant, but the only problem is that if one follows these to the end one is not able to get anything out of it after all. So it works both ways: sometimes a good idea does not work, and what seems like a bad, even idiotic idea, may actually work.

Question. Have you communicated some of these ideas that you have had to others?

Yes, I have mentioned what I have told you. I told people that I did not particularly believe that my approach would have led to something if I had followed it further. It did appeal to me in the beginning, and I tried to follow it for a long time, but I became more and more convinced that it was unlikely that I could achieve anything in this way. However, I have not verified that it could not be done.

Question. Do you have many ideas, or other thoughts, that you would like to leave for posterity?

No, I cannot say that about the Riemann hypothesis. I have some results of statistical nature. There have been some people that for the last few years have talked to me, and have wanted me to publish the details of what I have lectured about several times, namely about linear combinations of certain Dirichlet series. These linear combinations have the property that, like the zeta function, they have functional equations that lead to a real-valued function on the critical line such that typically a positive portion of the zeros lie on the critical line. I must say that it is interesting that this can be done, but one cannot use it for anything substantial. It is not really so many new ideas that enter into this, only old ideas that are combined in a new way. So I thought it was interesting to work it out and lecture about it, but I do not know yet if I ought to publish it. It would become considerably longer than I actually feel like writing up. As I told you earlier, I am by nature somewhat lazy, and that is my excuse for not having published so much. Many of these things have been published little by little by others. So even if I should never publish this myself, then eventually there will be others that will do that, I would think.

Question. You mentioned that other attempts at proving the Riemann hypothesis—like Alain Connes'—essentially, as you see it, only give reformulations?

Yes, that is a new way to arrive at the explicit formulas—a new access, so to say—but it basically does not give more than what one already had. Connes undoubtedly believed to begin with that what he was doing should lead towards a proof, but it turned out that it does not lead further than other attempts. When I last talked with him he had realized this. This often happens with types of work that are rather formal. There was, for example, a Japanese mathematician, Matsumoto, who gave several lectures that made quite a few people believe that he had the proof.

Question. To put the Riemann hypothesis in some perspective; if we, as non-experts, asked you the following question, and you should give a short answer: What does the Riemann hypothesis tell us about the prime numbers?

It tells us that they are very nicely distributed, about as evenly and as good as altogether possible. One cannot expect a completely even distribution, of course. But it tells us that at least in mathematics, certainly in number theory, we live in Leibniz' "best possible of all worlds", just as the good Candide in Voltaire's *Candide* is told by his teacher Pangloss that he lives in the best of all possible worlds. Well, in number theory at least, one has the best relation possible among primes, even though we cannot prove it yet. It would give me great satisfaction to see a proof, because it would demonstrate that there are some things that are right in this world. There are so many other things that do not work as they should, but at least for the prime numbers, and of course also for the zeros of the zeta function, they are distributed as well as they could be.

Question. Does there exist some geometric analogue to the prime numbers as far as fundamentality is concerned?

If you take a compact Riemann surface with the hyperbolic metric, and consider the closed geodesic curves, then you can say that their lengths correspond to the logarithms of the primes. In the compact case one has that the Riemann hypothesis is essentially correct, except from a few cases where there are some zeros lying between real parts 1/2 and 1, which I do not believe can occur for those functions that we usually consider in number theory. However, I know there are some people that believe that perhaps some of the L-functions belonging to quadratic extensions have zeros lying between real parts 1/2 and 1.

Question. Is there anything else you want to say about the Riemann hypothesis before we leave this subject?

I think it is a good possibility that it will take a long time before it is decided. From time to time people have been optimistic. Hilbert, when he presented his problems in 1900, thought that the Riemann hypothesis was one of the problems that one would see the solution of before too long a time had elapsed. Today it is a little more than one hundred years since he gave his famous lecture on these problems. So one must say that his opinion was wrong. Many of the problems that he considered to be more difficult turned out to be considerably simpler to solve. There were, for example, some problems about the transcendence of certain numbers that were decided earlier. There has been great progress in the area of transcendence results since Lindemann's original work from 1882. Incidentally, I have to tell you a story about Lindemann. When I was in Uppsala, Sweden, in the autumn of 1939, I found in the library two papers by Lindemann which he had published when he was quite old—certainly more than seventy years of age—where he claimed to have proven Fermat's conjecture. In the second paper he said a whole lot of nasty things about those that had pointed out mistakes in his first paper. This can happen to the very best. Lindemann was really a great mathematician. His transcendence results were extremely far-reaching and of very general nature—he had built on some earlier results by Hermite. It happens that people, also great mathematicians, become a little senile in their old age. You have to bear with me, if . . .

Question (Laughter)**.** We have not noticed any sign of that yet!

Were there any discoveries that you made during the war years that led to the discovery of the Trace Formula?

The Trace Formula came a bit later, and it really had little to do with my work on the zeta function. It came about after I had seen a paper by Hans Maass, where he considered the solution of a certain partial differential equation that was invariant under the modular group, for example. He had left quite a few problems unresolved. I saw that one could use some ideas that I had pondered on before the war in the wake of my Master Thesis. At that time I had looked at integral operators, which I felt much more familiar and comfortable with than differential operators. I preferred to look at Fredholm type equations instead of differential equations, and I thought that if one considered the class of all invariants, that is, all the integral operators that were invariant under the modular group, for example, then that would be a more natural thing to do than just look at the hyperbolic Laplace operator. So I started to look into this. Of course, these were integral operators extending over the upper half-plane, if one uses that representation, but one could use the invariance under the group to consider integral operators defined only over the fundamental domain. After I had done this, it felt natural to investigate if one could compute the trace of an integral operator that acted within the fundamental domain. It turned out that by combining the terms in a suitable way, one could give this an attractive form.

It was fairly easy to do this if I considered instead of the modular group a group that had a compact fundamental domain in the hyperbolic plane. In 1952 I gave some lectures on this in the midwest at four universities: Ann Arbor, Purdue, Chicago and Urbana. It gave me some trouble to carry through the proof for the modular group which, of course, does not have a compact fundamental domain, and it gave me even more trouble to do it for groups in the hyperbolic plane whose fundamental domain have finite area, but have what one calls "cusps". I succeeded in completing these proofs. The most difficult part was to get a grip on the continuous spectrum, involving so-called Eisenstein series. It turned out that these could be extended analytically even for a general group, not merely a modular group, but a general group that had fundamental domain with finite area, but which was not compact. I completed this during the summer of 1953, and I communicated this result to Siegel. I thought that he would be somewhat interested, and he was. He asked me if I would come to Göttingen in 1954 and lecture on this, and I did that. I gave a series of lectures which ended by treating the non-compact case in general, not only for modular groups, but generally. There was an assistant present at these lectures that took notes. I was going to write up the last part of the lectures, but I was not satisfied with the notes of the first part that I received, so I never sent the first five chapters back to Göttingen. Therefore, the Göttingen notes start with Chapter 6. Any intelligent person would be able to complete the Göttingen notes from what was published, partly in the Proceedings from the meeting in Bombay, and also what later appeared in an Indian journal.

Question. Was Poisson's summation formula a motivation—maybe of a philosophical nature—for you when you worked on the Trace Formula?

It was really not a motivation, but it can be viewed as an analogy if one considers Euclidean spaces and commutative groups and their actions. Then Poisson's formula, if one looks at it from this point of view, becomes a special case of the Trace Formula.

Question. Could you write down the Trace Formula for us?

Well, it looks quite complicated in general, but in the simplest case where the group Γ has a compact fundamental domain, say $\mathcal{D}$, in the hyperbolic plane, then it becomes considerably simpler. So let $\lambda_n = \frac{1}{4} + r_n^2,\ \ n = 1, 2, \cdots$, be the eigenvalues of the associated Laplace operator, and let $h(r)$ be an even function which is analytic for $|\mathrm{Im}(r)| < \frac{1}{2} + \epsilon$ for some $\epsilon > 0$, and with growth condition $h(r) = O(\frac{1}{(1+r^2)^{1+\epsilon}})$. Let

$$g(u) = \frac{1}{2\pi}\int_{-\infty}^{\infty} h(r)e^{iur}\,dr$$

be the Fourier transform of $h(r)$. Then the Trace Formula can be written as

$$\sum_{n=1}^{\infty} h(r_n) = \frac{A(\mathcal{D})}{4\pi}\int_{-\infty}^{\infty} rh(r)\frac{e^{\pi r} - e^{-\pi r}}{e^{\pi r} + e^{-\pi r}}\,dr + \sum_{k=1}^{\infty}\sum_{\{P\}_\Gamma} \frac{\log N(P)}{N(P)^{k/2} - N(P)^{-k/2}} g(k\log N(P)).$$

Here $A(\mathcal{D})$ is the area of $\mathcal{D}$ measured with respect to the invariant hyperbolic measure, $\{P\}_\Gamma$ denotes a primitive class of hyperbolic transformations, $N(P)$ is the norm of P. The P's correspond to geodesics on the associated Riemann surface, with $\log N(P)$ the length of P.[†]

Question. When you look at the enormous importance and widespread applications that the Trace Formula has had, do you consider that to be your greatest discovery?

Well, yes, it probably is. It presumably is the one that has most applications, I would think, even though I do not necessarily understand some of these applications. What I mean is, I have heard that the Trace Formula has been put to use in physics, but I do not know precisely in what way. And, to be honest, I am really not that interested in knowing how it is applied there.

Question. Are you surprised at the importance and role the Trace Formula has attained?

Well, as I said, I am surprised that it has got applications in physics. Mathematically speaking, I have always thought that the Trace Formula was a significant result. It contains a whole lot of information, and the problem itself encompasses and raises many questions to be explored in future research. This is especially true in higher dimensions where one encounters continuous spectra of more than one dimension, so to say. That complicates things. So there is a whole lot more to be done there, but that must be done by others since I have no intention of writing anything more about the Trace Formula. I gave some lectures in the 1980s, and also a few times later, about what I decided to call the "hybrid trace formula". The simplest example of that is, one may say, if you take the hyperbolic plane and an algebraic Riemann surface mapped into the hyperbolic plane (all this is intimately tied up with the uniformization theories) then—how shall I express it?—you can deduce quite a few classical results as special cases of the Trace Formula, for example the Riemann-Roch formula. You have to use some special kernels. Well, analogous things can be done in higher dimensions, too, but there it is somewhat more difficult to find the right kernel functions. It can be done, for instance, for all so-called "bounded symmetric domains", and for certain classes of functions. There you find similar things, especially if you have a product of several of these, where some of these functions give by themselves what corresponds to a Riemann-Roch formula. But the other component of the kernel is of a more general nature and that is what I referred to as a "hybrid trace formula". From some of these one can obtain quite interesting results. One simple case is the so-called Hilbert modular group that is associated to a real algebraic number field. If it is of degree n, then you have a product of n hyperbolic planes. You can choose a kernel there, which is what one could call the singular kernel, and which leads to the Riemann-Roch formula for, let us say, $n-1$ of these variables. For the last variable you can take a general kernel and then you get formulas that lead to the Dirichlet series that you can construct from the Hilbert modular group. For one thing you get the interesting case that when n is even you get a series of something that corresponds to a zeta function and a series of L-functions—that is one way to express it—that are

[†]The above question and answer are taken from another context, but we feel it was natural to include it here.

associated to the group, and the zeta function has a pole at $s = 1$. In the classical situation, if you only have one hyperbolic plane, or if you have an odd number of hyperbolic planes, then the same problem would lead to something that has a zero at $s = 1$. For n an even number you therefore have something that is more analogous with the zeta function and Dirichlet L-series. As I told you, I gave some talks on this in the 1980s, but I have never published this. I do not know for sure if somebody else has published some of this by now. I have not really followed what has happened in this area over the last decade or so. I know that a whole lot of stuff has appeared in the literature, but I do not read as much as I did before.

Question. Do you think that anything essentially new was added in the later extensions and generalizations of the Trace Formula?

Well, the viewpoint was changed somewhat, of course, after one started to look at group representations. At the time this started I was not thinking so much about it, in fact, I have never really read much about these things. It was only much later that I began to take a closer look at it. In the hyperbolic plane it is not necessary to consider group representations, everything can be achieved by looking at automorphic functions and automorphic forms—they are all scalars, so to say, just one component. In the higher dimensional symmetric spaces the situation is more complex. If you have a discrete group whose fundamental domain has finite volume, then you can look at automorphic functions, of course. But in most of the cases there is nothing that corresponds to the scalar automorphic forms. Even for bounded complex symmetric domains, where the scalar forms always exist, they do not tell the whole story. In the higher dimensional case one also has to consider vectors of functions that are transformed under the group action, as a matrix. It is only in the hyperbolic plane that we get everything by looking at automorphic functions and scalar forms. It leads to more generality by considering all group representations, but I have to admit that I have really not studied this field in any detail. I have never read much mathematical literature. I operated mostly by looking at certain papers to see what various people were up to, and to see what I could understand, and then to build on this in my way. I have read very few books in mathematics, but those that I have read have meant a lot to me. But I have mostly looked at papers, more than at textbooks. However, one textbook that has meant very much for me was Erich Hecke's "Algebraische Zahlentheorie". From that book I learned a lot. That book is a gem.

Question. Are there other books than the one by Hecke that have been important for you?

As far as algebraic number theory is concerned, there were many other books that I tried to read, but none of them had my way of looking at things. I found Hecke's book very understandable, for instance the way he introduced and treated the ideal concept. Hermann Weyl had written a book on algebraic number theory, a fairly short one, and Landau had also written a book on the same topic. I never felt comfortable with their books. I preferred Hecke's way of looking at things. It worked very well for him, and he was the first that could make any significant progress with respect to the zeta functions and L-functions associated to algebraic fields of higher degrees, and he was able to prove the functional equation.

Question. Aside from algebraic number theory, were there other books, for instance Titchmarsh's book on Riemann's zeta function, that have been important for you?

Titchmarsh's book contained quite a few nice things. But it also had things that were not so good. As for the latter, there is no good reason to include something about almost periodic functions and regard the zeta function as an almost periodic function. That point of view is not very useful. It may be of some interest per se, but one has never been able to deduce anything that is of any value about the zeta function, or some of the other number theoretic series that one studies, using this approach. So that chapter in the book could be deleted. But Titchmarsh's book contained a lot of good things. I must admit I did not read all his proofs. By and large I must say, concerning many of the proofs that one finds in these books treating analytic number theory, things are done in an unnecessarily complicated way. In fact, when one considers the explicit formulas, it really is one particular formula that one is primarily interested in, and where the series does not converge absolutely. In all these cases it is much simpler to use the integrated formulas. One can always obtain convergence by integrating a couple of times, then the series becomes absolutely convergent. The fact of the matter is that one can obtain equally good results by working with these absolutely convergent series, and then taking derivatives. It is totally unnecessary to consider something that does not converge absolutely.

Question. We have now talked about several of your discoveries—we have talked about the Riemann hypothesis and the Trace Formula. However, sieve-methods are also something that you have an affinity for, and where you have made fundamental contributions.

Well, that is so. That came as a by-product of my work on the zeta function.

Question. After your doctoral defense?

Yes, after I finished my doctoral defense. I realized that I could utilize some of the things I had used in connection with working on the Riemann zeta function. I could use it to find upper bounds, and it worked in a more general context than I had earlier. It was only then that I really understood what the sieve-method was all about. I had looked at Viggo Brun's papers, but I never really understood them. He extensively applied some kind of geometric presentation. He depended upon seeing things in a geometric way with figures and diagrams, et cetera. I looked at all this, but it just did not make much sense to me, so I never got anything out of it. There existed other presentations that avoided this. For instance, Rademacher's presentation was more accessible for other mathematicians. Rademacher had, so to say, translated Brun's work into another language. So it became more and more Rademacher's presentation of Brun's sieve-method that was used, and which made the theory accessible to a larger group of mathematicians. I also took a look at Landau's three-volume lecture series on number theory, which did contain a section on Brun's work, but he followed, to a large extent, Rademacher's presentation. I did not think that was so good, either. In order to find upper bounds, I discovered that I could use squares of $\sum_{d|n,d<z} \lambda_d$, and that worked very well. In fact, for all the problems that could be attacked by Brun's sieve-method, I could find better upper estimates using my method. Not only that, but the estimates were much easier

to find. Also, the constants involved became simpler and more natural, because I ended up with something that was an integer multiple of what presumably was the correct value. So then the only question that remained was to find something which gave lower bounds. One can achieve that by putting in front of these squares a factor that takes a negative value as soon as n has more than one prime factor, provided it lies under a certain bound.

Question. The so-called Selberg's sieve-method, that was your first main result in this area?

I published a note in the summer of 1946, and I continued to work on it further. When I came to Princeton in 1947 I made a discovery that put me on a path to what I call parity, and which is quite important for what one can do—and can not do—with sieve-methods. I tried to show the existence of prime numbers in intervals—relatively small intervals—by considering a quotient of two quadratic forms. I considered

$$\frac{\displaystyle\sum_{x<n<x(1+\epsilon)} d(n)\Big(\sum_{d|n}\lambda_d\Big)^2}{\displaystyle\sum_{x<n<x(1+\epsilon)} \Big(\sum_{d|n}\lambda_d\Big)^2}, \qquad d \le z,\ \ \lambda_1 = 1, \tag{1}$$

where $d(n)$ denotes the number of divisors of n. One usually chooses $z \le \sqrt{x}$. I looked at the quotient in (1). It is obvious that if you can make this quotient (one may restrict to square-free numbers that will yield the same), if you can make the quotient less than 4, you will essentially have shown that there exist prime numbers in the interval between x and $x(1+\epsilon)$, where ϵ is a small positive constant. One has the quotient of two quadratic forms in the λ's, and we want to minimize this, of course. You cannot really diagonalize the whole quadratic form by introducing new variables. I found that if I only tried to make the dominating part of the numerator as small as it could be if the λ's are free, except $\lambda_1 = 1$, then I could make the quotient as close to 4 as I wanted, namely as $4 + O(\frac{1}{\log x})$. It seemed to me that there was no reason to believe that I had found the right minimum by only taking the minimum of the dominating part of the numerator, and then inserting the λ values I had thus found. In fact, the remaining part of what I had found above is of the same order of magnitude, and it seemed clear to me that since I was not at the right minimum, then I should be able to make it a little less than 4 by adjusting it a little. But it turned out that that was not the case. I also tried with other expressions, and after a while it became clear to me that the numbers that have an even number of prime factors and those that have an odd number of prime factors will contribute about the same, so that the quotient can indeed not be made less than 4. The fact that it can be made as close to 4 as one may wish shows in reality that numbers with exactly two prime factors will contribute vastly more than all the others that have an even number of prime factors. In other words, those numbers with an even number of prime factors higher than two will give a contribution of a smaller order of magnitude. This phenomenon showed up in quite a number of other situations as well, so I realized that apparently whatever I did with these methods I would get the same asymptotic contribution from numbers with an even and an odd number, respectively, of prime factors. It then dawned upon me that it should be possible to construct an expression where I would get approximately the same contribution from the primes and products of two primes, and that was what led me to this formula that forms the basis for the elementary proof of the prime number theorem. I refer to this problem as parity: that in these various formulas the contributions from the numbers with an odd number of prime factors and those with an even member of prime factors are asymptotically the same. The sieve-method cannot distinguish between these two contributions. I mentioned this already in Trondheim in 1949, where I gave a talk, and I elaborated in more detail on the fact that one has this limitation in my talk at the IMU Congress in Cambridge, Massachusetts, at Harvard in 1950. This limitation also gives you an idea of what one can do, and the fact is that one can find an infinite number of formulas where the only contribution comes from primes and products of two primes. These two parts have the same weight and there are some different functions of the two that appear in these formulas. The formula that I presented in my published paper on the elementary proof of the Prime Number Theorem is actually the simplest. That formula emerges if one first considers the formula

$$\sum_{n<x}\sum_{d|n}\mu(d)\log^2\frac{x}{d} = x\sum_{d<x}\frac{\mu(d)}{d}\log^2\frac{x}{d} + O\left(\sum_{d<x}\log^2\frac{x}{d}\right) \tag{2}$$

(μ is the Möbius-function).

Here one easily sees that the inner sum on the left-hand side always is zero if n has more than two different prime factors. By looking at the values for $n = 1$ and $n = p^a$, where $a > 0$ and $n = p^a q^b$, with $p \neq q$ and $a > 0$, $b > 0$, the left-hand side of (2) becomes

$$\begin{cases} \log^2 x + \sum\limits_{p^a<x}\left(\log^2 p + 2\log p\log\frac{x}{p}\right) + \sum\limits_{\substack{p^a q^b<x\\ p\neq q}} 2\log p\log q \\ = \sum\limits_{p<x}\log^2 p + \sum\limits_{pq<x}\log p\log q + O(x). \end{cases} \tag{3}$$

At the right-hand side of (2) one can estimate the two sums, and one gets that the right-hand side is

$$2x\log x + O(x).$$

Taken together this yields

$$\sum_{p<x}\log^2 p + \sum_{pq<x}\log p\log q = 2x\log x + O(x). \tag{4}$$

Question. So is it formula (4) that is the key to the elementary proof of the Prime Number Theorem?

Yes, that is so.

Question. It is very important for us to establish this: It was in fact the sieve-method, and in a certain sense the simplest application of the sieve-method, that led to the asymptotic formula (4)?

Well, yes. It is some kind of sieve, it is a local sieve. You see, when one uses these methods in general one always finds that the λ's that one ends up with, depending upon what type of coefficients that appear, always are of the form

$$\lambda_d = \mu(d)\frac{\log^k \dfrac{z}{d}}{\log^k z}, \tag{5}$$

where z is a bound for how large d can be, and the exponent k depends upon the problem one considers.

Question. Is it some sort of Lagrange multiplicator method that you use to minimize these expressions?

Well, I have a method of introducing new variables that diagonalize the expressions, so that it is very easy to find the minimum of such a formula after you have done that, at least for the dominating part of the formula. Actually, it is a little more complicated. The optimal λ_d's , when we are able to determine them exactly, appear as $\mu(d)$ multiplied with a quotient of two sums. If one estimates these sums one gets an asymptotic formula of the form (5). Usually, one must be satisfied with obtaining an approximation.

Question. So this was a great major discovery, in fact?

Well, it takes some time before you are able to draw further inferences from it. It took me some time to get it in the way I wanted it, and it went through several phases. One of these I had not planned for. To put it this way: there came an "interloper in the way". Before we get to that, I have to tell you that I used something similar in connection with something I had already finished, and which was ready for publication. It was an elementary proof of Dirichlet's theorem about the existence of prime numbers in arithmetic progressions. I did not use (4) then, but something that may be deduced from the analogue of (4) for an arithmetic progression $kn + l$, where $(k, l) = 1$, namely

$$\sum_{\substack{p<x \\ p\equiv l(\mathrm{mod}\ k)}} \frac{\log^2 p}{p} + \sum_{\substack{pq<x \\ pq\equiv l(\mathrm{mod}\ k)}} \frac{\log p \log q}{pq} = \frac{1}{\varphi(k)}\log^2 x + O(\log x), \tag{6}$$

where $\varphi(k)$ is Euler's number theoretic function.

It is then not so difficult to get a contradiction if you assume that there are not infinitely many prime numbers in progressions. I went through this proof with Turán who was here in Princeton then, the summer of 1948. He had asked some questions in connection with this, and I had come to mention the formula (4)—in the proof itself only the formula (6) entered. I believe what caused me to mention the formula (4) was that he, Turán, had asked how sharp one could make certain estimates. He had been here for the spring term and he was about to travel back to Hungary, and he would probably have left before I returned from Canada. I was about to travel up to Montreal to get a permanent visa, because I wanted to take a job at Syracuse, New York, for a year. I had been offered another year at the Institute here in Princeton, but I thought it would be interesting to see what it would be like to be at a different American university.

Question. Did you have to travel to Canada to get a visa?

You could not do this inside the U.S., you had to travel to another country, and Canada was the closest place. So I went up to Montreal, and I returned to Princeton nine days later. In Montreal I did not talk with the consul himself, but with the vice-consul. I had been advised to go to Montreal by Hua, a Chinese mathematician who had been here in Princeton and whom I got to know. He had travelled to Canada to change his visa in order to take a position at Urbana, Illinois. So I mentioned the Hua case to the vice-consul to encourage him to do the same for me. He told me after having looked at the files related to Hua that it seemed obvious to him that Hua should not have received a visa. After some days had elapsed, he did make the visas ready for us. However, before we got them he had second thoughts and withdrew them. So it took a few days more, and we had to have some documents translated. It was particularly complicated with some documents that my wife Hedi had in Romanian, but we found a translator who was able to give an official attestation that the documents had been correctly translated. We finally got our visas, and we travelled back by train, entering the U.S. at St. Albans in Vermont. When I came to the Institute next day, this was on Thursday, July 15, it turned out that Turán, to my surprise, was still there—he left and went back to Hungary the next day, I believe. He had gone through my proof for arithmetic progressions, which I had told him about, and he had also mentioned in passing the formula (4). In the meantime, while I was in Montreal, Erdős had also arrived in Princeton, and he had been one of the listeners to Turán's presentation. Erdős told me on that same Thursday that he was interested in this formula (4), which he called an inequality. I always called it an equality—it was an asymptotic formula. Well, one can say it is an inequality since it is greater than an expression if you multiply x with a negative constant, and smaller if you multiply with a positive constant. But I have always called such a formula an asymptotic equation, not an inequality. But he called it an inequality. He wanted to try to see if he could use it to show that there existed prime numbers between x and $x(1+\epsilon)$, where ϵ is arbitrarily small, if x was sufficiently large. Well, I told him that I had nothing against that. I was not working on something like that at that time. In fact, I had left this problem after I discovered what I called parity, and had realized that what I had tried to do with the quotient in (1) would not succeed; that is, I could not make it less than 4.

Question. So you did not have the Prime Number Theorem in your thoughts then?

Oh yes, I had the Prime Number Theorem in my thoughts, that was my goal based upon formula (4) that I had obtained. I told him that I did not mind that he try to do what he said he wanted to do, but I made some remarks that would discourage him. I told him that he should not be too confident that it would be possible to deduce so much from my formula. But then, a couple of days later—I believe it was on Friday evening or it may have been on Saturday morning—Erdős told me that he had found a proof for the existence of primes between x and $x(1+\epsilon)$, and he gave me some of the details of how his proof went. I had much earlier obtained a few other results. For example, if one takes the function $\psi(x)$, which is the sum of the logarithms of the prime numbers less than x, that is

$$\psi(x) = \sum_{p<x} \log p, \tag{7}$$

and considers $\limsup$ and $\liminf$ of $\frac{\psi(x)}{x}$, call this A and a, respectively, then I had deduced that $A + a = 2$. This fits very nicely, of course, with the supposition that both of them should be 1, and that would give a proof of the Prime Number Theorem. One easily observes that it is highly unlikely that they are different from each other, since this would imply a very peculiar distribution of the numbers that have an even number of prime factors and those that have an odd number of prime factors, as well as a very peculiar distribution of the primes themselves. Well, I discovered that I could incorporate his result, which actually said more than the existence of prime numbers between x and $x(a + \epsilon)$—that result in itself would not have been sufficient for me—in what I had been working on, and this led me to a proof that A and a are equal. Then they have to be equal to 1, of course, and that is equivalent to the Prime Number Theorem, namely that $\frac{\psi(x)}{x}$ tends to 1 as x goes to infinity. So I told Erdős the next day that I could use his result to complete the proof, an elementary proof, of the Prime Number Theorem. We talked somewhat more about this, and it turned out that one could avoid using his result, but use some of the ideas he had used, to get a more direct and shorter proof. I really did not have in mind starting a collaboration with him. He asked me if we should go through this proof, and I thought he meant that we should go through the proof with a few other people here at the Institute that were interested in number theory. Among these were Chowla from India and Ernst Straus, who was Einstein's assistant and who was somewhat interested in number theory. Turán had already left—I believe he left on Friday, while this was taking place on the following Monday. I said okay, and I came over to the Institute in the evening to go through the proof. It turned out that Erdős had announced this at the university so instead of the small informal gathering that I thought this was supposed to be, the auditorium was packed with people. I went through the first parts that I had done earlier. Then Erdős went through what he had done. Finally, I completed the proof of the Prime Number Theorem by combining his result with mine.

After a few days I travelled up to Syracuse to look for an apartment. Besides, I had promised them that I would teach at the summer school and take care of engineering students in what they called "advanced calculus". In Syracuse they would pay me somewhat more. They also promised to provide a job for Hedi, something she would appreciate. So we went there. It took some time before I found an apartment, so we lived with a colleague of mine in the meantime. I started to hear from different sources that they only mentioned Erdős name in connection with the elementary proof of the Prime Number Theorem, so I wrote a letter to Erdős and told him how I would proceed. He had in the meantime given several talks about this in the U.S., but I must admit that I did not give a talk on this since the one time in Princeton. I had to take care of teaching, and then there was the matter of finding an apartment, which took a lot of time. After some time had elapsed, I began to type my proof of the arithmetic progression result, and I tried to simplify the proof of the Prime Number Theorem simultaneously. I found fairly soon a proof that I liked which did not use upper and lower limits, and which was more direct. The proof was constructive, and it was this proof I wrote up at the same time as I typed the one for the arithmetic progression.[†] I wrote to Erdős that we could publish each separately, and that I would let his paper appear first, if he would publish the result that I had used originally to prove the Prime Number Theorem. Then I would publish a paper where I first sketched the proof that used his result, and afterward I would give the proof that I was more satisfied

[†]Selberg sent a handwritten, eight-page letter in Norwegian, dated September 26, 1948, from Syracuse to his brother Sigmund in Norway, outlining the elementary proof of the Prime Number Theorem.

with and which did not use any of his things. But he insisted upon being involved more directly in this.

Question. What else did Erdős say in his reply to your letter?

He answered that he reckoned we should do as Hardy and Littlewood. But we had never made any agreement. In fact, we had really not had any collaboration. It was entirely by chance that he became involved in this—it was not my intention that he should have access to these things. It is clear that Hardy and Littlewood had an agreement that when they worked together on something they should both get equal credit for the results they obtained. It is all well and good that they had an agreement, and they worked together. Erdős and I had really not collaborated on anything. The only thing was the discussion we had after I had found the first proof of the Prime Number Theorem by using his result. That was perhaps the only thing that could be called some kind of collaboration, but we did not have any agreement that we would "share alike everything". I must say that I never had any thought of collaborating with anybody. I have one joint paper, and that was with Chowla, but I must say that it was Chowla that first came to me with a question. He was interested in computing the L-function that belongs to the largest discriminant of an imaginary quadratic number field, the largest discriminant that has class number 1. That discriminant equals 163, and he would like to find a way to evaluate the L-function that belongs to the quadratic character for this module at the point 1/2, and to see if this gave a positive or negative value. If it came out negative it would imply that there was a zero which was not on the line 1/2, but somewhere between 1/2 and 1. It so happened that I had, incidentally, a formula which should make it fairly simple to make a numerical computation, and which could be used for any zeta function associated to a positive quadratic form in two variables. I gave that formula to Chowla, and he came back a short time later and said he had found that it gave a negative value at the point 1/2. This implied that there must be a zero that was not on the line 1/2. I pondered a little over this and looked into the details. As a matter of fact, there existed two theories of quadratic forms, that is, binary forms, long time back. One of these has a mid-coefficient with a 2 in front, that is, of the form $Ax^2 + 2Bxy + Cy^2$, while the other is of the form $Ax^2+Bxy+Cy^2$. What one calls the discriminant gets a different expression, depending upon which of these forms one considers. In the one case it equals $AC - B^2$, and in the other it equals $4AC - B^2$. My formula had been developed with respect to the smaller discriminant, while Chowla had put into the formula a discriminant that was too big. It turned out that when he made the change to the smaller, the formula yielded a very small, but positive value. So there was no zero after all. By looking closer at this we came across a whole lot of other things. In particular, by not considering the point 1/2, but rather the point 1, and looking at the residue there after one has removed the associated Epstein zeta function we got some interesting results. The Epstein zeta function is in reality the zeta function of the quadratic field when the class number is 1. Then it has an Euler product that has a zeta function and an L-function with a quadratic character. If you remove the zeta function you are left with the value of the L-function at the point 1. We had an expression for this from the formula I had, and it turned out that it actually gave access to a rather interesting result about the periods of elliptic functions that have complex multiplication, in the classical form, that is. One considers the periods. If you use the old Jacobi form, which was also used by Abel, then you get that the periods can always be expressed as an algebraic number multiplied with a product of gamma functions. This was only known in two special cases before, namely, if you take elliptic integrals of the form

$$\int \frac{dx}{\sqrt{1-x^4}}$$

which correspond to arcs of the lemniscate. The other known case, where complex multiplication also occurs, comes from considering the integral

$$\int \frac{dx}{\sqrt{1-x^3}}.$$

Both of these cases were classically known. The integrals from 0 to 1, for example, could be expressed by gamma functions evaluated at certain rational values. But our result was more general. If the class number was 1, we got a rather simple expression. But I generalized the result somewhat so that it also encompassed the case when the class number was larger than 1. Then the formula became more complicated, but it still had the form of an algebraic number multiplied with a product of gamma functions evaluated at rational points. This was a rather interesting result. I wanted Chowla to put his name first, but he refused vehemently, so the paper was published under the names Selberg and Chowla, in that order. It is completely illogical, of course, instead of having it in alphabetical order. I got him to write it up; except that I wrote up the part that treated the case when the class number was greater than 1, since he was not that familiar with some of the things that was needed to treat this case. He computed some examples where the class number was 1, where he also determined the algebraic factor explicitly. One knows that it is in general an algebraic factor. One can express it in terms of a radical expression, but it can be quite complicated.

Question. This is the only paper you have published with anyone else?

Yes, but the first impulse came from Chowla. If he had not come and told me what he tried to do, and if I had not remembered the formula that I had found on another occasion, then nothing would have come out of it. It could also very well have happened that if we had got a negative value at the start, then we would have been satisfied with that and not gone any further, just registered that we had disproved the Riemann hypothesis for this particular L-function.

Question. Let's get back to Erdős. Is it correct to say that it was an unintended accident that he saw your fundamental formula?

Well, yes. You have to understand that Turán had become a good friend of mine while he was in the U.S., and I knew that he would soon go back to Hungary. I thought that he would have left when I returned from Montreal, but it turned out that he was still here. Erdős had arrived in the meantime, and he got to know about this via Turán. As I told you, I had gone through my proof of the arithmetic progression with Turán and he had posed a question which caused me to mention formula (4) that I had obtained.

Question. So you did not tell Turán *not* to mention this formula to others?

No, I did not do that. Firstly, Erdős was not there when I talked with Turán, and besides I thought Turán would have gone back to Hungary before I returned from Montreal. I had no inkling that Erdős would arrive in Princeton for a visit of several weeks duration. But these two, Turán and Erdős, knew each other from Hungary, mostly from before the Second World War. Erdős was not in Hungary during the war, but Turán was there.

Question. Did Turán express some sort of regret for what happened later?

No, but you must understand that Erdős was his friend, and he would be unwilling to offend him. I kept good relations with Turán afterwards, but we avoided talking about these matters later. As I said, Erdős answered my letter and referred to Hardy and Littlewood, something I thought was irrelevant in this case—we were not anything like Hardy and Littlewood. I do not know which of us he thought was Hardy and which was Littlewood!

Question. You said earlier that when Erdős talked with you, you tried to "discourage" him. Can you specify that a little more?

I told him at that time that one could give a counterexample, namely that an analogous formula to (4) would imply something else. Let us look at the continuous analogue. Let's say that one has a formula like

$$\int_1^x \log t \, df(t) + \int_1^x f\left(\frac{x}{t}\right) df(t) = 2x \log x + O(x). \tag{8}$$

If one has such a formula it is not necessarily so that $f(x)$ is asymptotical to x. I can construct a counterexample. However, the function $f(x)$ I used is not everywhere monotonely increasing. On the other hand, the function in (7), $\psi(x) = \sum_{p<x} \log p$, is a monotone function, and it is monotone functions that are relevant for number theoretic applications. I kind of tried to scare him away from the Prime Number Theorem itself. It was, one may say, a little dishonest that I did not tell him that my counterexample was based on a non-monotonic function.

Question. We understand the psychology very well. You know you are close to a proof of the Prime Number Theorem, and you do not want any meddling?

I did not want any interference in this matter. Anyway, I suggested to Erdős that each of us could publish separately what we had done. He could have the priority to publish his result, so that would appear before my result—which actually did not need his—but I would give a full sketch of how I first had used his result to obtain my first proof of the Prime Number Theorem. That was also what I did.[†]

Question. Hardy believed that it was not possible to give an elementary proof of the Prime Number Theorem?

Yes, but that is not so strange, actually. But there were some people that made a great fuss about this. Erdős created a whole lot of propaganda for himself. I was in Syracuse, and I did not lecture on this anywhere. In fact, I have never really given a talk about the elementary proof of the Prime Number Theorem. I have given talks a couple of times about an elementary proof of the essential part of the results Beurling obtained for so-called generalized prime numbers. The elementary proof gives a somewhat weaker result than Beurling's. I do not know if it is possible, but I would think it should be, to give an elementary proof that would give a sharper form, like the one Beurling had.

[†]For the full detailed account of this, including two letters of Hermann Weyl, see `http://www.math.ntnu.no/Selberg-interview/`.

Question. Can you explain to us what a generalized prime number is?

Beurling lectured on this at the Scandinavian Congress in Helsingfors in 1938, and I later looked at his published paper. So you have a sequence of real numbers $1 < p_1 < p_2 < p_3 < \cdots$, and you form all possible products $\{n_k\}_k$ of these and order them according to size, $1 \leq n_1 \leq n_2 \leq n_3 \leq \cdots$. You denote the number of p's less or equal to x by $\pi(x)$, and the number of n_k's less or equal to x by $N(x)$. The question is, if you assume that $N(x)$ is asymptotic to a constant multiplied with x plus a remainder term of the form $O(\frac{x}{\log^\alpha x})$, that is

$$N(x) = Ax + O\Big(\frac{x}{\log^\alpha}\Big), \tag{9}$$

what can you say about $\pi(x)$? Beurling proved that if $\alpha > 3/2$, then $\pi(x)$ is asymptotic to $\frac{x}{\log x}$, and so it corresponds to the Prime Number Theorem. He proved more than that: If (9) holds for all α, then he could prove sharper estimates. In fact, then you get that $\pi(x)$ is equal to the logarithmic integral of x plus $o(\frac{x}{\log^\beta x})$ for all β; that is

$$\pi(x) = \mathrm{li}(x) + o\Big(\frac{x}{\log^\beta x}\Big). \tag{10}$$

The logarithmic integral can be defined in different ways, but let us say that $\mathrm{li}(x) = \int_2^x \frac{dt}{\log t}$.

Question. Does Beurling's proof use the Prime Number Theorem?

No, the proof does not use the Prime Number Theorem. Beurling's proof is an analytic proof. I was able to find an elementary proof, but I had to assume that the remainder term is $o(x/\log^2 x)$—only then could I obtain a proof.

I have often wondered if it is possible to improve my proof so it is valid for $\alpha > 3/2$, but I have not had the patience to work it out. But I cannot conceive that it should not be possible to do so.

Question. You have received the Fields Medal in 1950. You also have received the Wolf Prize in 1986. Three years ago the Abel Prize in mathematics was established. What are your thoughts on these types of prizes in general—do you think they have a positive effect?

It does not advance science. No one does scientific work because there exist prizes—I cannot imagine that. A prize will make one or more persons happy, but it also gives rise to disappointment among many people, I would imagine.

Question. But do you believe it serves mathematics in the sense that it creates publicity and thus raises the awareness of the public?

Whether it serves mathematics to get publicity is an open question.

Question. Coming back to the Abel Prize: what are your thoughts on the awards so far?

I proposed Serre and Grothendieck as candidates for the first award in 2003, as some people I would have preferred. I thought that Serre would get it, and that also happened. Since then I have not made any proposals. Concerning the Abel Prize, I have a somewhat ambivalent attitude. Let us consider the Nobel Prize: I think it has caused some unintended harm by creating a strong distinction in prestige between those that get the prize and others, who certainly deserve it, but do not get it. There are, of course, some people that so clearly outshine others that the award is uncontroversial; in physics, for example, you have Einstein, Bohr, Heisenberg, Dirac, and a few others. However, since the prize is awarded yearly, it is inevitable that the distinction will not be so clear. The same problem is bound to happen with the Abel Prize. There are mathematicians that outshine others and so clearly deserve the prize—I mentioned Serre and Grothendieck above as two such people—but in the long run one cannot expect that the recipients of the Abel Prize will be of the same calibre.

Question. One of the reasons you did not get the Abel Prize is perhaps that you are Norwegian?

I am a little too old, I think. One should perhaps have an age limit.

Question. You did receive a so-called honorary Abel Prize in 2002, at the Abel Bicentennial Meeting in Oslo. You were not able to be present to receive this prize. However, you sent a thank you letter where you referred to Abel's two-page short note in Crelle's journal in 1829, which appears as number XXVII in his Oevres Complètes, where he proves the most general from of the addition theorem for abelian differentials. You wrote: "It still stands for me as pure magic. Neither with Gauss nor Riemann, nor with anybody else, have I found anything that really measures up to this." Can you make some further comments on this?

I want to make it clear that I never have read in detail Abel's so-called Paris Memoir, which for a long time disappeared before it was recovered and published long after Abel's death. But it is that little note, upon which the results of the Paris Memoir rest, which is so extremely elementary. There really is no comparison in the mathematical literature, I think. Such a fundamental and far-reaching theorem proved by so simple and elementary methods—it is pure magic. I cannot imagine anything that quite compares to this.

Question. A famous problem that was solved a few years back was Fermat's Last Theorem. Many will hail this achievement as a victory for modern mathematics, that one needed a huge machinery of modern tools to accomplish this. We have a question for you in this connection. Do you think that there will appear, as time goes by, a simple proof, or do you think that this is the future, namely that one will need big machineries to solve apparently elementary problems à la Fermat?

It is certainly possible that one will find a simpler proof some time in the future. I am not able to say from what direction this will come. There are two issues here: one may be able to find a great simplification of the present proof, which relies on the connection to the cubic curve that must exist if there is a solution to Fermat's equation; but it could also happen that one may be able to find a proof that avoids that connection. I do not think one will be able to rediscover Fermat's original proof.

Question. If it existed?

One cannot doubt Fermat, can one? He was a very intelligent man, Fermat. No doubt about that.

Question. But you do not really believe he had the proof?

Either he had it, and he could not find sufficient space to write it down, or he discovered later that it was not entirely correct as he had thought. But it is not likely that he had a proof because one knew too little about algebraic numbers at that time. If every algebraic ring had a nice Euclidean algorithm, then it would have been possible for him to construct a proof, but Euclidean algorithms seldom exist, in fact.

Question. We want to end our interview with you by asking the following question: What in your opinion is it that characterize mathematicians of high and exceptional quality?

Imagination, resourcefulness and a feeling for relations and patterns are important ingredients. It is also very important to have a whole lot of perseverance, combined with patience. Needless to say one needs a lot of energy as well. Finally, I think that quite simply some luck is part of it. Yes, some people are lucky many times, and others are lucky only one time, while some perhaps are not lucky any time. What I mean is that I have seen good ideas, even brilliant ideas, that some people have had, but which in the end did not lead anywhere. And I have also seen examples of people with ideas that did not seem good or exciting, but which strangely enough led to interesting results. I have known people that seemed to have lots of ideas and that knew a lot of mathematics, but that never obtained really exciting results. I have also met people, whom I did not consider to be particularly intelligent when I talked to them, but who came up with things, often in a clumsy and inelegant way, which turned out to lead to results of great importance. No, I dare not define what is the essence of a mathematical talent. It is too multifaceted and of such great variety.

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