Excerpt from

THE LORD OF THE NUMBERS

ATLE SELBERG ON HIS LIFE AND MATHEMATICS

By

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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.

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, etcetera. 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.

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

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

where $d(n)$ denotes the number of divisors of n. One usually chooses $z \leq \sqrt{x}$. 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 can not 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 $4 + O(1/\log x)$. 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 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 can not 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

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

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

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

So is it formula (4) that is the key to the elementary proof of the prime number theorem?

Yes, that is so.

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.

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.

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

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

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.

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

You could not do this inside the US, 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 who 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 to the vice-consul the Hua case 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 Rumanian, 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 US 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.

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 lim sup and lim inf of $\psi(x)/x$ – 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 $\psi(x)/x$ 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 US, 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 it 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 with and which did not use any of his things. But he insisted upon being involved more directly in this.

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 have 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

*Selberg sent a hand-written 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.

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..

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 Riemanns hypothesis for this particular L-function.

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 US, 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.

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.

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 whom of us he thought was Hardy and who was Littlewood!

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.

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.* Hermann Weyl became some kind of intermediary. When Herman Weyl took his leave from the Institute here in Princeton, he gave me some relevant documents†. Weyl had heard from both Erdős and me, and he had formed his opinion on how things hanged together. By and large he sided with me, but he thought that I could be generous and let Erdős publish his work. I had really no objection to him doing that. But the case is that Erdős at that time had in a way already published. He had given some talks in Boston, at MIT, I believe, and later he had travelled to Europe and given some talks in the Netherlands, where van der Corput was. Subsequently, van der Corput wrote up an outline of Erdős' presentation. That was actually the first publication. It appeared already in the autumn of 1948, and my paper appeared in the spring of 1949. I sent my manuscript to the Annals of Mathematics. My original idea was that we both should publish in the Annals, but it ended up with that Erdős would publish his paper in the Bulletin of the American Mathematical Society, whose editor at the time was Nathan Jacobson at Yale University. Jacobson sent Erdős' manuscript to Hermann Weyl for him to referee. Weyl wrote a report to Jacobson where he started off by saying that he thought that Erdős was not altogether honest. He claimed credit for too much, according to Weyl. It is likely that this had something to do with what I later heard from Siegel. He had talked with Erdős and said that it must be possible to reach some sort of agreement on this. But Erdős had said: "What I want is immortality". This is just a completely idiotic thing to say. There does not exist in actual fact any immortality, not even in mathematics. What I mean to say is that if the names are preserved they are written, after a while, starting not with a capital letter, but with a small letter, like in "abelian". Well, there will not be that many that will know about these names as time passes.

Why would Erdős not send his paper to the Annals?

Well, he did not agree to the idea that I had put forward. I meant that we should present what each of us had done, but he wanted more involvement than that. Hermann Weyl wrote to Nathan Jacobson later that it was wrong of Erdős to present so much of what had actually been done by me. It ended up with Jacobson not wanting to have this included in Erdős' publication. So he rejected the paper. Erdős then contacted someone he knew at Colombia University, who was a member of the National Academy of Science in Washington, and who had the right to present his manuscript there. So it was published in their Proceedings.

*The version published in BAMS does not contain the following text up to the footnote on page 11.

†See Appendix 1.

This is actually not a good place to publish mathematics, since it is a place that contains all kinds of articles within science. It is not often that mathematicians read this journal. On the other hand, it was published rather quickly. I do not know which paper appeared first, his or mine. But the very first publication on this was the one that van der Corput was behind, and that came, as I told you, in the autumn of 1948. I have a copy of it in my drawer. I have lying here some kind of "file" containing much of the material that concerns all this, but I believe that the papers are all mixed up. Yes, here it is. I think that van der Corput has modified the proof somewhat – it was not exactly this way that Erdős presented it to him. He has given it his own form. Anyway this was the very first publication. There are perhaps not that many that know about this publication?

It was just circulated like this?

Yes, van der Corput sent me a copy of it. He mentions my name first, he does not take it in alphabetical order. Incidentally, I have never met van der Corput..

Selberg-Erdős, Selberg-Erdős. He, that is, van der Corput, mentions you first everywhere.

He understood; and besides, he has modified the form of presentation that Erdős gave. I must admit, I have never read the details – reading other people's work I find strenuous.*

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.

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 $O(x/\log^\alpha x)$, that is

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

*The version published in BAMS resumes here.

what can you say about $\pi(x)$? Beurling proved that if $\alpha > 3/2$, then $\pi(x)$ is asymptotic to $x/\log x$, 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 $o(x/\log^\beta x)$ for all β; that is

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

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

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 can not conceive that it should not be possible to do so.

On the following pages follow the letter and report from Hermann Weyl that Selberg refers to.

Confidential.

P. Erdös, On a new method in elementary number theory which leads to an elementary proof of the prime number theorem.

Erdös's paper is based on a fundamental asymptotic formula F due to A. Selberg, and it leads up to an elementary proof of the prime number theorem P also first obtained by Selberg. Neither the formula F nor the proof of the prime number theorem based on it have as yet been published by Selberg. In the time between the establishment of F and the attainment of P a development took place during which Erdös and Selberg worked in what may be called close competition or collaboration. There is no doubt that, before Selberg reached P, Erdös proved the lemma L described in §2 of this paper. How much help Selberg derived from the ideas involved in this proof and Erdös' communications is not easy to make out, while Erdös depended admittedly very much on Selberg's contributions. I have corresponded with both, have seen copies of the notes which they exchanged and ~~listened~~ listed to their accounts; and, although I first was inclined to ascribe to Erdös a considerable share in the development, I have finally come to the conclusion that Selberg from beginning to finish followed his own head and was influenced by Erdös only to the extent that he was forced to work at a pace altogether unnatural to him. The two men are of such very different temperament as to make the ensuing misunderstandings almost inevitable. Selberg would have liked the whole matter to reach a certain stage of finality before making it public, and he was not inviting help or collaboration when, half reluctantly, he mentioned his asymptotic formula to his friend Turan. I have seen Selberg's MS of his elementary proof of P which he recently submitted to the Annals of Mathematics for publication. From this it is evident that he considered the stage described in Erdös' paper as preliminary only; his proof has now taken on a much more direct and, to my taste, more satisfactory form.

Under these circumstances it would heve been reasonable for Erdös to accept Selberg's offer to have his lemma L and its proof published before Selberg. E. thought that this did not do full justice to his contribution. In the present paper §3 deals with "Selberg's deduction of the prime number theorem from (2)" and §4 with a "Sketch of Selberg's simplification of the proof of (2)". Erdös is scrupulously fair in giving Selberg his due credit. But has he a right to publish things which are admittedly Selberg's, but which the latter considers as intermediary and therefore not fit for publication? I am sure that everything in Erdös' paper will have "historical" interest only once Selberg's paper is out.

I honestly think, one would do a disservice to Erdös by printing his paper in its present form. After he rejected Selberg-s offer, I see two possibilities only. (1) He publishes his lemma L and its proof and mentions but briefly that it has played an essential role in the development of an elementary proof of the prime number theorem (for which he refers to Selberg's forthcoming paper). Or (2)Erdös writes the story as the participant and eye-witness of an important event would tell it, post festum, as it were, to an audience that already knows the net result (as given in Selberg's paper) and is interested in learning how it all came about. But then Erdös' paper should follow that of Selberg in time and should clearly point out that it describes stages of development that are now definitely superseded.

Before coming to such radical conclusions I tried to remedy the situation by alterations of the MS on a smaller scale. The title is certainly misleading and unfair to Selberg. "Report on the development of a new elementary method in number theory" seems to me a more appropriate title, as it would indicate that the paper reports on something which the author can only partially claim as his own product. For the entire paragraph after formula (3) to the end of page 1 I should substitute something like this: "Such simplifications of the proof of (2) and of the prime number theorem as resulted from discussions during the next days between Selberg and the author are included in this chronological report. Another paper will deal with the application of the method to prime numbers in an arithmetic progression."

I offer these suggestions in case the editors, out of consideration for Erdös and his undeniable merits in this matter, should decide to print his paper essentially in the form in which he has submitted it. In that case some editing seems desirable also on the following pages; the outward appearance of the MS is indicative of the carelessness of its composition. §6 seems to have no heading.

Hermann Weyl

February 15, 1949

Dear Jake:

It is very unfortunate that Erdös insists on having his paper published in the present form. I had hoped that when he carried out his plan he would see for himself that it doesn't work and that what he wrote down sounds somewhat ridiculous. I had questioned whether he has the right to publish things which are admittedly Selberg's, but which the latter considers as intermediary and therefore not fit for publication. He argues that if that principle were upheld in all circumstances, one of two collaborators in a mathematical enterprise could prevent publication if he so wants. I would answer: So what of it? If you enter into collaboration with another, you ought to have enough respect for him to run that risk. But in the present case it is not Selberg's stubbornness that would prevent publication of those intermediate results, but his good sense for what is mature and what is not. I really think that Erdös's behavior is quite unreasonable, and if I were the responsible editor I think I would not be afraid of rejecting his paper in this form.

But there is another aspect of the matter. It is probably not as easy as Erdös imagines to have his paper published in time in this country if the Bulletin rejects it. And we should avoid even the appearance of trying to suppress the opinion of one side in the present issue. So it may be better to let Erdös have his way. No great harm can be done by that. Selberg may feel offended and may protest (and that would be his right), but I am quite sure that the two papers — Selberg's and Erdös's together — will speak in unmistakable language, and that the one who has really done harm to himself will be Erdös.

But even if you accept the paper you should insist on some changes, especially on a change of the title. I made some suggestions to that effect in my previous letter. The title in its present form is really unfair to Selberg. Moreover, the paper has been drawn up so carelessly that it needs a certain amount of editing.

I return Erdös's letter of February 6 herewith.

Sincerely,

Professor Nathan Jacobson
Amer.Math.Soc.Bulletin
Yale University
New Haven, Conn.

Hermann Weyl

HW:GB