The context of Riemann's paper on the distribution of prime numbers

In memoriam Andrew Ranicki

S.J.Patterson

Perspectives on the Riemann Hypothesis, Heilbronn Institute, Bristol 4th June, 2018

JOURNÉES "ARITHMÉTIQUE ET ANALYSE HARMONIQUE"

--- Le programme pourra éventuellement être changé---

Mercredi 7 novembre

10.45 : G. Henniart (Orsay). Le point sur la conjecture de Langlands locale.
14.00 : P. Kutzko (Iowa). Titre à préciser.
15.15 : C. Blondel (Paris). Représentations des groupes métaplectiques.
16.30 : M. Tadić (Zagreb). On classification of irreducible unitary representations of GL(n) over non archimedean fields and some conjectures.

Jeudi 8 novembre

9.30 : J. Schwermer (Bonn). Automorphic forms and the cohomology of arithmetic groups ; some specific examples.
10.45 : G. Harder (Bonn). Implications of the theory of Eisenstein cohomology classes to special values of L-functions.
14.00 : J.P. Labesse (Dijon). Cohomologie des groupes discrets et fonctorialité.
15.15 : S.J. Patterson (Göttingen). Problems in the representation theory of the metaplectic group.
16.30 : M.F. Vigneras (Paris). Représentations automorphes de GSp(4).
18.00 Drinks (45-55)(401)

Vendredi 9 novembre

9.30 : H.Matsumoto (Paris). Sur certains groupes d'opérateurs éventuellement unitaires.
10.45 : U. Stuhler (Wüppertal). Drinfeld modular varieties.
14.00 : C. Bushnell (London). Titre à préciser.
15.15 : A. Frohlich (London). Gauss sums for Weyl groups and for principal orders (tame case).
16.30 : P. Gérardin (Paris). Equation fonctionnelle des facteurs locaux de degré 2.

Samedi 10 novembre

9.30 : U. Jannsen (Regensburg). On imprimitive representations of local Weyl groups.
10.45 : H. Opolka (Münster). Thêta functions with characteristics and galois representations.

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Toutes les conférences ont lieu à l'Université Paris 7 (Métro Jussieu), dans la salle de conférence de l'Institut de Biologie Moléculaire, tour 42, au rez-de-chaussée.

Pour tout renseignement complémentaire les participants pourront s'adresser à : François Rodier, U.E.R. de Mathématiques, Université Paris 7, Tour 45-55, 5ème étage, 2, Place Jussieu, 75251 PARIS CEDEX 05 - Tél. 336.25.25 - poste 37-68.

Les participants seront accueillis mercredi 7 novembre à partir de 9.30 en salle 1, tour 45-55, étage 4.

More recent material on Riemann:

Der Briefwechsel Richard Dedekind Heinrich Weber Korrespondenz, Edd. Katrin Scheel, Thomas Sonar, Karin Reich

Riemann Nachlass in Berlin, Erwin Neuenschwander
Norbert Schappacher

Letters from Riemann to W. Weber, Jose Marin,
Wolfgang Gabcke

Notes of Ernst Meissel, found by Jan Peetre

One or two things to bear in mind about Riemann:

He spent most of his working life in Göttingen, but the two years he spent in Berlin were the most influential. He was really a student of Dirichlet and strongly influenced by the less convivial Jacobi.

In Göttingen the most creative teachers/colleagues were physisics, Wilhelm Weber and Johann Benedikt Listing. Gauss was too old to have any direct influence. Dedekind was a fellow student who befriended Riemann and helped him through his dark phases. Riemann seems never to have been really happy in Göttingen, but he seems to have been in Berlin and later in Italy.

Further – Riemann was a most voracious and retentive reader. From Dirichlet he was probably introduced to the French literature – most famously Cauchy. In Göttingen, because of the (by this point historical, after the accession of Victoria) Hanoverian court in the UK there was an excellent collection of British journals in the university library. They are still there and there is good evidence the Riemann read them.

We shall have reason to think about Fourier's masterpiece ”Théorie de chaleur“, Binet's memoir on the gamma function, and Stokes' great memoir on the Airy integral and Bessel functions.

Riemann's method of working seems to have had three distinct phases:

- Wide reading around the topic in question
- Thinking long and hard with only minimal use of paper, and that mainly for technical calculations
- Writing directly out of his head.

The final phase caused him considerable difficulty – even as a schoolboy he worked in this way and got into trouble for it. It is perhaps not surprising that he did make some mistakes – but mistakes at a very high level.

Having got all this off my chest I come to what are the main contentions of this talk:

1. Riemann was thinking in the context of summation formulæ - as in his Habil-Schrift – and this was only one part of a bigger project.

2. He was using very strongly an analogy with Bessel functions – but he pushed this rather too far.

3. He was convinced in 1859 that the Riemann Hypothesis but by 1863 he was no longer certain.

The first of these really needs little discussion. The second is the one on which I shall concentrate.

But first we consider the last of these. The point in his paper where he enunciates the Riemann Hypothesis is very well known but somewhat later, in discussing the distribution of the primes, he assumes without further ado that the zeros of ξ are real.

However when Schering and others published Volume 2 of Gauss' Collected Papers they included the letter from Gauss to Encke on the prime numbers and there is no reference to Riemann's investigation. It was published by the Akademie der Wissenschaften in Göttingen; Riemann was a member and had close contacts to the editors.

Now we turn to the main contention. Riemann gives two proofs of the analytic continuation. The first is based on a contour integral which he writes as

$$2 \sin \pi s \, \Pi(s-1) \zeta(s) = i \int_{\infty}^{\infty} \frac{x^{s-1}}{e^{x}-1} \, dx.$$

One can recognise in this a relationship to the theory of Bernoulli numbers (and so Bernoulli polynomials) and also with the Euler-Maclaurin summation formula. This is related to the kernel Riemann uses in his Habil-Schrift. The point here is that he can show how effective the methods of complex analysis and how advantageous a flexible contour is – as in his paper on the hypergeometric function.

In the other proof Riemann uses the elementary theta and an ingenious but simple integration by parts to show that if s=½+ti and we write

$$\Pi\left(\frac{s}{2}\right)(s-1)\pi^{-s/2}\zeta(s)=\xi(t)$$

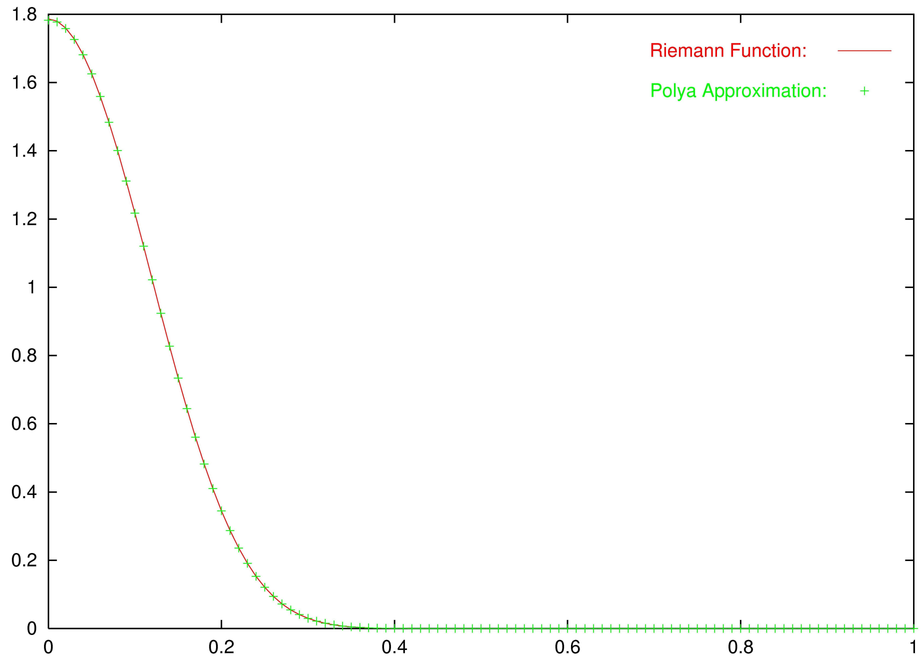
then

$$\xi(t)=4\int_1^\infty \frac{d\left(x^{3/2}\psi'(x)\right)}{dx}\cos\left(\frac{1}{2}t\log x\right)dx.$$

Here $\psi(x)=(\theta(x)-1)/2$ and θ is the usual theta function. We next express the integral as

$$\int_0^\infty \Phi(u)\cos(tu)\,du.$$

The graph of Φ looks like



This is very, very close to the Poisson integral for a *J*-Bessel function (with an additional factor) in which Φ is replaced by a function of the form $(1-u^2)^{\lambda-½}$., supported [0,1] whereas Riemann's function does not have compact support.

There are two lines of thought that one can follow starting from this formula.

Firstly – there is a strong analogy with Bessel functions. In his *Théorie de chaleur* Fourier proves that the zeros of $J_0(x)$ are all real. Both Fourier's motivation and his proof are extremely interesting and we shall come back to it.

Secondly – the notes of Ernst Meissel make it clear that one can compute both $\xi(t)$ in an interval around 0 and the early Taylor coefficients effectively and without too much trouble by relatively straightforward numerical techniques. Meissel's rough notes take up only 9 pages although he would have done “sums” on a slate or sheet of paper. He uses the “Polya approximation”, the term in Φ coming from the first term in the defining sum for θ.

Let us write:

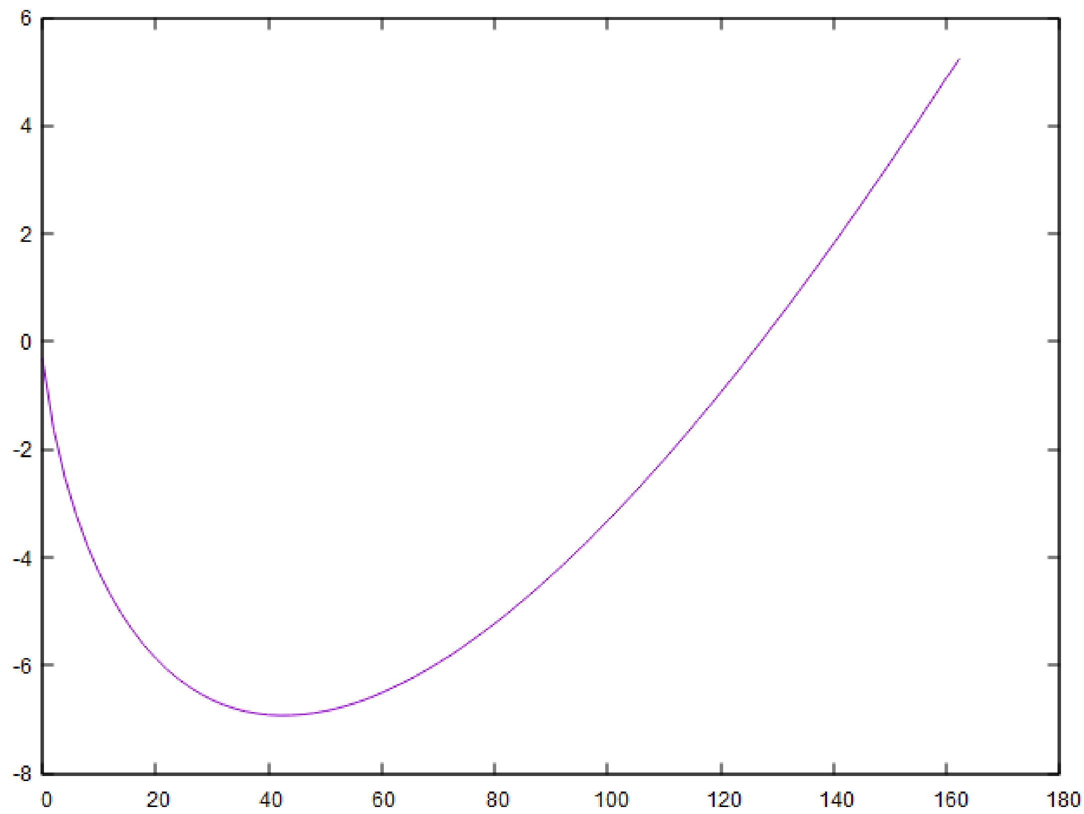
$$\xi(t) = \sum (-1)^n \frac{a_n}{(2n)!} t^{2n} .$$

The a_n are then given by

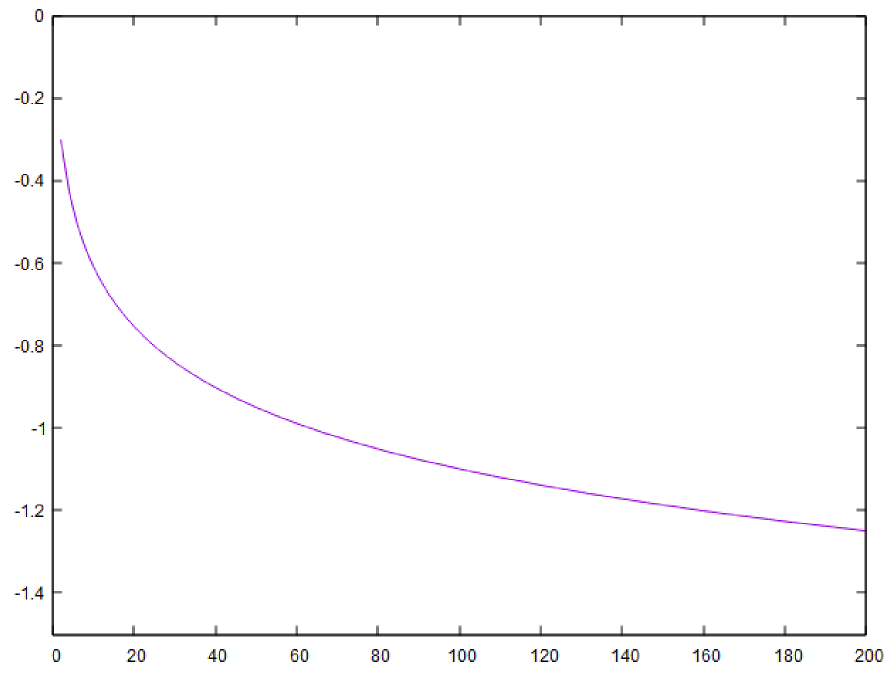
$$a_n = \int_0^\infty \Phi(u) u^{2n} \, du .$$

Riemann writes, shortly before he enunciates the Riemann Hypothesis that this is a very rapidly converging series. He neither gives a justification for this nor does he make it particularly clear what he means. The asymptotics of an were only resolved in 1964 (with a correction in 1966) by Emil Grosswald. It is not easy.

It turns out that for small n that a_n is very small – Wolfgang Gabcke gave me a list of values for $2n \leq 1000$ - but the values start increasing very rapidly. Here are the values ($\log_{10}$ thereof) plotted against $2n$.



The corresponding plot for J_0 is:



Although there is a lot of wriggle room in the interpretations it would seem as if Riemann was working from analogies and a few computations. There is no evidence that he had developed any theory of the asymptotics of a_n.

We now turn to Riemann's general strategy. He had been thinking about summation formulæ in his Habil-Schrift. The analysis of such formulæ is generally by means of kernel functions which are special cases of the general formula. One which can be used to prove versions of Fourier's theorem was the formula:

$$\pi \cot(\pi x) = \frac{1}{x} + \sum_{n \geqslant 1} \frac{1}{x-n} + \frac{1}{x+n}.$$

This is logarithmic derivative of Euler's formula:

$$\sin(\pi x) = \pi x \prod (1 - x^2/n^2).$$

The zeta function is not far away.

Fourier series are not the only "eigenfunction expansions" which Fourier considers in his *Théorie de chaleur*. He also studies Fourier-Bessel expansions, i.e. the expansions of functions on $[0,1]$, vanishing at 1 and zero derivative at 0 in series of the functions $J_0(j_n x)$ where j_n runs through the positive zeros of J_0.

These first appear in works of Daniel Bernoulli (1738) on swinging chains and of Leonard Euler (1764) on circular drums. In these works, which we cannot go into now, the zeros of J_0 appear as the frequencies of vibration.

Fourier encountered them as well in studying the heat equation and later Riemann was to as well in his paper on Nobili rings (electric conduction).

Fourier proves the remarkable theorem that

all the zeros of J_0 are real.

This is presumably the godmother of the Riemann Hypothesis.

He proves it by constructing an excellent rational approximations to J_0 and then using “Fourier's criterion”, a prototype of Sturm's criterion which Charles Sturm proved a few years later.

The proof which we would now give (self-adjointness) was first given by Riemann in his lectures (WS 1854/55,WS 1860/61, SS 1862), and by Eugen v. Lommel (1868).

Why was this important? The zeros were, as reported above, were important in the applications. If one tries to tabulate a function like J_0 by using the series one very rapidly gets to the point where on is "subtracting infinity from infinity"; the calculations become unstable. In a paper *De oscillationibus minimis funis libere suspendi* published in 1781, i.e. close to the end of his life gave a most ingenious method for computing the zeros. Recall the formula

$$\sin(\pi x) = \pi x \prod (1 - x^2/n^2).$$

Euler's original "proof" was formal – and bogus – but he later gave a valid proof using the addition formula. One can also use the differential equation, for example, for $\cot(\pi x)$. Euler, on the principle that what's good for the goose is good for the gander, applied his formal argument again to J_0.

Euler then posited:

$$J_0(x) = \prod_{n \geqslant 1} \left(1 - x^2 / j_n^2\right).$$

Let $$s_k = \sum_{n \geqslant 1} j_n^{-2k};$$

One then has

$$\frac{J_0{}'(x)}{J_0(x)} = -2 \sum_{k \geqslant 1} s_k x^{2k-1}.$$

The coefficients of the Bessel function are rational numbers and so it is easy to write down a recursion to compute the s_k.

If we order the j_n by size then we have that

$$j_1^2 = \lim_{k\to\infty} s_{k-1}/s_k\,.$$

This comes with error estimates. Once one has computed this term to a given degree of accuracy one can remove it from the sums s_k and repeat the process with the remaining zeros. This method was invented in the case of polynomials by Daniel Bernoulli. Euler obtained very good results using it.

The method can be pepped up in various ways, most especially by a method of Gräffe (introduced in 1833 for polynomials). For those interested it is described in Perron's *Algebra*, Bd.2.

Why was he successful?
Because of the reality of the zeros. Euler assumed this and it explains the significance of Fourier's theorem.

Now we can return to Riemann, or, at least, the zeta function. It would be, in principle, possible to compute numerically, for some values of *k*, the series

$$\sum_{\rho} (\rho - 1/2)^{-2k}.$$

The computations are more intricate; there is no evidence that either Riemann or his immediate successors attempted it. Had they done so, it would, in principle, have been possible to compare these values with the corresponding sum over the real zeros. One could imagine a justification of Riemann's “so many of” statement along these lines. However, one has the formula

$$\sum_{\Im(\rho)>0} \frac{1}{\rho(1-\rho)} = \frac{1}{2}\log(4\pi) - 1 - \gamma/2.$$

If one computes this the convergence is painfully slow and if anything it would be a strain on the belief of the most faithful.

Euler's method gets less and less accurate the higher the zero. This problem was first really solved by Stokes in a paper variously dated 1847(MVB), 1850(GGS) and 1856(GNW).

Stokes was primarily concerned with the Airy function but considered other Bessel functions – in this paper he proves that the Airy function is essentially a Bessel function of order ⅓, a result usually ascribed to Nicholson (1909) and Wirtinger (1897).

Stokes first uses an argument close to Cauchy's theorem, but done by hand, to convert Airy's improper integral into a convergent integral and derives from this both the series expansion and the differential equation.

He then compares the differential equation with ones whose solutions he knows and which he suspects gives the asymptotics and uses a geometric comparison, the sort of method more often used for non-linear equations, to give the asymptotics for both the functions and for the zeros.   He also makes use of the method of stationary phase, usually ascribed to Kelvin (1887) – but Lamb does point out, in his *Hydrodynamics*, the relevance of Stokes' paper.

Stokes was influenced by Hamilton's *On fluctuating functions* (1844), an analysis of Dirichlet's proof of the representability of functions of bounded variation by their Fourier series.   In it, and another paper of Stokes, there are early versions of the Riemann-Lebesgue Lemmas.

Stokes's paper is very much in the same spirit as Riemann's work.   The paper was available in Göttingen and we know the Riemann did read other papers of Stokes.   Moreover he seems to quote from Stokes' paper in his paper on Nobili rings.

One could imagine then the Riemann would have aimed at a similar result for the zeta function.   There is a brief note which Siegel converted into the Riemann-Siegel formula.   In his proof he uses Riemann's first representation of the zeta function.   It is a very sophisticated argument and it seems unlikely that Riemann had got this far.   Unfortunately there is simply no evidence.

Riemann's approach to $N(T)$ is unknown. Strictly speaking the formula he gave is incorrect. It is often interpreted as a misprint for v. Mangoldt's theorem but there is no evidence at all that Riemann had made any attempts to estimate arg $\zeta(s)$ where one would need it.

Also the verification of the Riemann Hypothesis relies on the effective version of v. Mangoldt's theorem which dates from 1911 or 1912 and more thoroughly later – by Backlund. Before that there were no verifications.

In this talk I have concentrated on the significance of Riemann's second representation of the zeta function and the analogy with Bessel functions.   I should remind you – and it is referred to in Titchmarsh – the Pólya did use this representation in his paper *Über die algebraisch-funktionentheoretischen Untersuchungen von J.L.W.V. Jensen*, (1927), 34 pp. to deduce Hardy's theorem on the infinitude of zeros on the critical line – see pp . 28-31.   This does seem to be a bit more sophisticated than one would expect from someone working it is possible that something along these lines, perhaps incomplete or heuristic, was what Riemann had in mind.

This work of Pólya's is ist one of several interesting and subtle papers which he wrote in the 1920s, and which are still relevant today. The phrase “Hilbert-Pólya Conjecture”, which is not even a conjecture but a pious hope, undervalues the real achievements of Pólya. The “contribution” of Hilbert is anecdotal – the most authentic-looking version is given by Weil in his Collected Works where is is a gung-ho statement made in a lecture. Hilbert was rather given to bursts of wild enthusiasm. As we have seen the idea of exploiting self-adjointedness probably goes back to Riemann himself.

If some sort of self-adjointness argument were to be posited it would only be meaningful if the differential operator were to be stipulated. A “random” i.e. “typical” operator would be the result of some existence theorem and would probably rely on the Riemann Hypothesis. Experiments can be done. One can look for a 1-dimensional Schrödinger operator. This has been done by Christa Mirgel – Diplomarbeit, Göttingen, 1985. The results do not suggest that there is some very special potential, but that by a judicious choice of paramenters one can get close to a group of zeros.

And – one should not forget that this is not the only game as Stepanov showed in connection with the Riemann Hypothesis for curves. Noteworthy is that Keith Ball has recently taken up the path trodden by Fourier and Pólya.

One final point – the Riemann Hypothesis, however one might understand it – has become one of the “sexiest” (a better nomenclature would be C.G.Jung's “numinous”) questions if one judges by its exposure in the media and the number of non-proofs which appear day for day. It certainly has been very influential as a guide and has engendered a great deal of valuable mathematics over the last hundred years or so, perhaps from Hadamard and de la Vallée Poussin on, as work-arounds. But the historian has the same question as that which Max Weber confronted with his concept of “charisma” in leaders.

Why? Is it really justified?