Alan Turing and the Riemann Zeta Function

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1. Introduction

Turing encountered the Riemann zeta function as a student, and developed a life-long fascination with it. Though his research in this area was not a major thrust of his career, he did make a number of pioneering contributions. Most have now been superseded by later work, but one technique that he introduced is still a standard tool in the computational analysis of the zeta and related functions. It is known as *Turing's method*, and keeps his name alive in those areas.

Of Turing's two published papers [27, 28] involving the Riemann zeta function $\zeta(s)$, the second[1] is the more significant. In it, Turing reports on the first calculation of zeros of $\zeta(s)$ ever done with the aid of an electronic digital computer. It was in developing the theoretical underpinnings for this work that Turing's method first came into existence.

Our primary aim in this chapter is to provide an overview of Turing's work on the zeta function. The influence that interactions with available technology and with other researchers had on his thinking is deduced from [27, 28] as well as some unpublished manuscripts of his (available in [29]) and related correspondence, some newly discovered. (To minimize any overlap with other chapters, we do not discuss Turing's contributions to computing

[1] reproduced on pages NAA–NZZ above

in general, even though they did influence the work on $\zeta(s)$ that he and those who followed in his footsteps carried out.)

Andrew Booker's recent survey article [3] has a significant overlap with what we say here and is highly recommended as a collateral "read."

2. Recollection of Some Basics

The Riemann zeta function $\zeta(s)$ is defined for complex s with $\mathrm{Re}(s) > 1$ by

$$\zeta(s) = \sum_{n=1}^{\infty} \frac{1}{n^s}. \tag{1}$$

This function can be extended analytically to the entire complex plane except for the point $s = 1$, at which there is a pole of order one. The extended function, which is again denoted by $\zeta(s)$, has so-called *trivial zeros* at $s = -2, -4, -6, \ldots$. The other zeros, called *nontrivial zeros*, are also infinite in number, and lie inside the *critical strip* $0 < \mathrm{Re}(s) < 1$. The Riemann Hypothesis (RH) is the assertion that all the nontrivial zeros ρ lie in the center of the critical strip, i.e., on the *critical line* $\mathrm{Re}(s) = \frac{1}{2}$. Any ρ's lying off the critical line necessarily occur in symmetric quadruplets, $\{\rho, \bar{\rho}, 1-\rho, 1-\bar{\rho}\}$.

The RH is widely regarded as the most famous unsolved problem in mathematics. It was one of the 23 famous problems selected by Hilbert in 1900 as among the most important in mathematics, and it is one of the seven Millennium Problems selected by the Clay Mathematics Institute in 2000 as the most important for the 21st century [5]. For general background on the RH, we shall be content to cite the survey article [7] and web site [5]. For more technical information about the zeta function, see [26].

The RH was posed by Bernhard Riemann in 1859. (See [5] for a copy of Riemann's paper and an English translation.) The importance of the RH stems from the connection observed by Riemann between primes and the nontrivial zeros of the zeta function. If, as usual, we let $\pi(x)$ be the number of primes up to x, then Riemann showed that (for $x \geqq 2$)

$$\pi(x) = \mathrm{Li}(x) - \frac{1}{2}\,\mathrm{Li}(x^{1/2}) - \sum_{\rho} \mathrm{Li}(x^{\rho}) + W(x), \tag{2}$$

where $\mathrm{Li}(x)$ is the logarithmic integral, a nice and smoothly growing function, and $\mathrm{W}(x)$ is of lower order (relative to the three earlier summands). The terms $\mathrm{Li}(x^{\rho})$ are special cases of the classical analytic function $\mathrm{Ei}(\xi)$ defined for $\mathrm{Im}(\xi) \neq 0$, which differs insignificantly from e^{ξ}/ξ whenever $|\xi| \gg 1$. One simply puts $\xi = \rho \ln(x)$ for each ρ.

The main difficulty in using Eq. (2) to estimate $\pi(x)$ is that the series is not absolutely convergent. Since $\pi(x)$ is a step function, and the individual terms on the right side of Eq. (2) are continuous at each prime number p, the sum behaves something like a Fourier series in producing the discontinuities of $\pi(x)$. Another difficulty is that the sizes of the individual terms depend on the locations of the nontrivial zeros ρ.

The leading term in Eq. (2), $\mathrm{Li}(x)$, grows like $x/\ln(x)$ as $x \to \infty$. The Prime Number Theorem, first proved in 1896 by Hadamard and de la Vallée Poussin using properties of zeros of the zeta function, tells us that asymptotically $\pi(x)$ grows like $\mathrm{Li}(x)$; hence like $x/\ln(x)$. The RH has been shown to be equivalent to the difference function $|\pi(x) - \mathrm{Li}(x)|$ being bounded by a quantity close to $\sqrt{x}$, where close means within logarithmic factors, or (what amounts to the same thing) the square root of the leading term in Eq. (2).

In his famous 1859 paper, Riemann asserted that most nontrivial zeros of the zeta function are on the critical line, and that it was *likely* that all of them lie there (which is what we now refer to as the RH). Riemann did not provide even a hint of a proof for the first, positive, assertion. It remains unproved to this day, although it is believed to be true, even by those who are skeptical of the truth of the RH. The RH itself is known to be true for the first 10^{13} nontrivial zeros, as well as large blocks of zeros much higher up, including some around zero number 10^{24}.

At the end of his paper, Riemann also discussed another conjecture that played a significant part in Turing's research, namely that $\pi(x) < \mathrm{Li}(x)$. As Riemann noted, computations by Gauss and Goldschmidt had established the validity of this inequality for $x < 10^5$, and if the series over the nontrivial zeros ρ in Eq. (2) were nicely behaved, the difference $\mathrm{Li}(x) - \pi(x)$ would tend to grow roughly like $\sqrt{x}/\ln(x)$. From the tone of Riemann's presentation, it appears that he suspected the inequality $\pi(x) < \mathrm{Li}(x)$ might well be true generally. (We say "suspected" because Riemann's wording is vague.)

Today, we know that $\pi(x) < \mathrm{Li}(x)$ holds not just for $x < 10^5$, but even for $x < 10^{14}$. In 1914, however, Littlewood proved that there are infinitely many integers $x \geqq 2$ for which the inequality fails! The most recent result in this area shows the inequality fails for some $x < 10^{317}$, but we still do not know where the first counterexample occurs. There are heuristic arguments suggesting there are no counterexamples within $x < 10^{30}$ and likely even higher. Thus this is one of the many instances that occur in number theory of a conjecture that is supported by heuristics and extensive numerical evidence, yet turns out to be false. In a similar way, the validity of the RH is definitely *not* something that we can be assured of simply on the basis of its being true for the first 10^{13} cases.

Littlewood's proof that $\pi(x) > \mathrm{Li}(x)$ holds infinitely often relied on Riemann's expansion (2), and required considerable technical virtuosity to deal with the infinite series that was not absolutely convergent. In the mid-1930s, another approach became available through the work of Ingham that had the advantage of being both simpler and more explicit, but at the cost of requiring some computations. In very loose terms, Littlewood's result was shown to follow from knowledge of some initial set of non-trivial zeros of the zeta function. (Cf. §5 below.) This connected numerical verifications of the RH to the $\pi(x) < \mathrm{Li}(x)$ conjecture. Turing was intrigued by both problems, and made contributions to each one.

Interestingly enough, it appears that Turing had doubts about the validity of the RH already at an early stage and that, over time, his skepticism only increased.[2]

3. On Turing's Computations of the Zeta Function

The first computations of zeros of the zeta function were performed by Riemann, and likely played an important role in his posing of the RH as a result likely to be true. His computations were carried out by hand, using an advanced method that is known today as the Riemann-Siegel formula. Both the method and Riemann's computations that utilized it remained unknown to the world-at-large until the early 1930s, when they were found in Riemann's unpublished papers by C. L. Siegel. In the meantime, as both the significance and difficulty of the RH were recognized around the turn of the 20th century, computations using a less efficient method, based on Euler-Maclaurin summation, were carried out by several investigators. The calculations used tables of logarithms and trig functions, paper and pencil, and mechanical calculators. The largest of those early computational efforts was that of J. Hutchinson, who showed that there were exactly 138 zeros of the zeta function with $0 < \mathrm{Im}(s) < 300$, and that they all satisfied the

[2] Littlewood's views followed a similar trajectory; see [16] and [17, p. 792].

RH. (Hutchinson also provided modestly accurate values for the 29 zeros in $0 < \mathrm{Im}(s) < 100$.)

Aside from possible numerical mistakes, these computations are completely rigorous, and do establish the validity of the RH for all the zeros for which it is claimed. As was recognized already by Riemann, there is a simple variant of the zeta function that is *real* on the critical line, so that a sign change of this function has to come from a zero of the zeta function that is right on the critical line. The final stage was the verification that the sign changes that have been found account for *all* the zeros in a given $\mathrm{Im}(s)$-range. Until Turing came out with his method, this step was done by a rather messy, although in principle not very difficult, computation based on the principle of the argument. Turing's method obviates any need for using the argument principle. It involves only the real-valued function on the critical line. See [28, §4] for a precise statement.

In the mid-1930s, after Siegel's publication of the Riemann-Siegel formula, Titchmarsh obtained a grant for a larger computation. With the assistance of L. J. Comrie, tabulating machines, some "computers" (as the mostly female operators of such machinery were called in those days), and the recently published algorithm, Titchmarsh established that the 1041 nontrivial zeros in $0 < \mathrm{Im}(s) < 1468$ all satisfied the RH [25].

Turing became interested in extending Titchmarsh's results. He designed and started to build, with the help of a £40 grant from the Royal Society, a special purpose analog computer to verify whether the RH is satisfied by all the zeros with $0 < \mathrm{Im}(s) < 6000$ (of which there are 5598). More details about this machine are available in [3, 4]. Work on this project was interrupted by the outbreak of World War II, and this computer was never constructed.

We do not know how well Turing's zeta function machine would have worked, had it been built. At least one special zeta function computer was constructed to a different design later by B. van der Pol [32]. By that time, though, electronic digital computers were becoming available, and Turing was the first one to utilize them to investigate the zeta function [28]. In 1950, he used the Manchester Mark 1 Electronic Computer to extend the Titchmarsh verification of the RH to the first 1104 zeros of the zeta function, the ones with $0 < \mathrm{Im}(s) < 1540$. This was a very small extension, but it represented a triumph of perseverance over a promising new technology that was still suffering from teething problems. In Turing's words, "[i]f it had not been for the fact that the computer remained in serviceable condition for an unusually long period from 3 p.m. one afternoon to 8 a.m. the following morning it is probable that the calculations would never have been done at all." These days, when even our simple consumer devices have gigabytes of memory, it is instructive to recall that the machine available to Turing had a grand total of 25,600 bits of memory, and that Turing worked directly with output "punched out on teleprint tape" in base 32. That Turing stayed up all through the night conveys some idea of how interesting he found this experiment.

More significant than the extension of the Titchmarsh verification of the RH to an additional *63* zeros was Turing's earlier computation on that same occasion of the 1054 zeros in $2\pi 63^2 \leqq \mathrm{Im}(s) \leqq 2\pi 64^2$, all of which turned out to lie on the critical line. (Note that $2\pi 63^2$ is about 25,000.) Not only did this produce a substantial increase in the number of zeros that were known to obey the RH, but it represented an innovation, a realization that by jumping to larger heights one could obtain a better view of the asymptotic behavior of the zeta function.

Today, Turing's pioneering use of the Manchester Mark 1 for computing the zeta zeros is a historical footnote. Turing's results were soon surpassed by a sequence of increasingly extensive computations. His work was furthermore not an unexpected breakthrough. Development of digital computers and growing interest in the zeta function would surely have led to someone else carrying out similar calculations within a few years, even if he had not done so.

For several decades, progress came exclusively from faster computers and longer runs. Beginning, however, in the mid-1980s, new algorithms started appearing, such as the one of Schönhage and the second author of this chapter for computing large sets of zeros, as well the ones of Schönhage, Heath-Brown, and Hiary for computation of individual values of $\zeta(s)$ when $\mathrm{Im}(s)$ is very large. Combined with growing computing power, these algorithms have enabled calculations far beyond the reach of Turing and his contemporaries. It is now known that the RH is true for the first 10^{13} nontrivial zeros, for some tens of billions of zeros around zeros number 10^{23} and 10^{24}, and for some hundreds of zeros near zero number 10^{32}. (All these projects have relied on Turing's method for *proving* that all zeros in a given range have been found and are on the critical line.) If there was a strong motivation to obtain more data, these numbers could be increased by factors of 10 or 100 simply by harnessing more computing power. As machines become more powerful and more plentiful, and still better algorithms are found, we can look forward to substantial growth in information about nontrivial zeros.

Among recent computations of zeta zeros, the verifications of the RH – whether for initial segments or for blocks of zeros high up – carry on traditions that were extended or started by Turing. Other efforts have involved high precision computations of low zeros. Some of those are done to obtain improved bounds for the counterexamples to conjectures such as that of Mertens, or that $\pi(x) < \mathrm{Li}(x)$, and are related to projects Turing devoted quite a bit of time to, and where, had he lived, he might have carried out such computations himself. (Cf. §5.) Others reflect a desire to test whether zeta zeros satisfy some algebraic relations among themselves or involving other well-known constants, such as e or π. (One conjectures that no such relations exist.) The *main* motivation, however, for recent computations of zeta zeros, as well as zeros of related functions, comes from a conjectured relation between those zeros and eigenvalues of random matrices. A conjecture made by Hilbert and Pólya in the 1910s was that the RH is true because zeta zeros correspond to eigenvalues of a positive operator. This initial conjecture was extremely vague, and hard to test. However, a variety of developments in the next half a century provided additional motivation to consider the Hilbert and Pólya guess more seriously. A particularly important development was a theorem of H.L. Montgomery from the early 1970s that suggested zeta zeros should behave like eigenvalues of a particular family, the GUE, of random matrices that had been explored intensively by mathematical physicists. Subsequent computations by the second author provided extensive numerical evidence for this connection. Ever since, a large industry has grown up, exploiting the (still conjectural and empirical) connection between zeta zeros and random matrices. This is regarded by many researchers as the most promising road towards a proof of the RH. More details and references can be found in [7]. This work is far from what Turing was aware of, but one can expect he would have found it exciting.

4. On Turing's Early Work with Zeta

Most readers will likely have at least some familiarity with Andrew Hodges' definitive biography [9] of Turing. Pages 94, 133–135, 140–142, and 154–158 therein suffice to give a quick overview of how Turing's research interests with $\zeta(s)$ got started around 1936-37 or so.

By combining the contents of four letters in the Turing Digital Archive (2 from Ingham; one each from Skewes and Titchmarsh) with several other sources, it is possible to view these early developments in substantially greater depth and, in the process, add some valuable context to the overall picture. Our aim in the present section is to do this, albeit very succinctly.

The following timeline presents the essential points:

- Turing matriculates at King's College in 1931. He meets A.E. Ingham, one of the two mathematics supervisors there. Ingham's now classic Cambridge tract [10] on prime number theory appears in 1932; Turing obtains a copy shortly thereafter [9, p.133].

- In 1933, Littlewood's student, Stanley Skewes, proves [22] that, if the RH is true, the smallest $x \geqq 2$ for which $\pi(x) > \mathrm{Li}(x)$ must satisfy $x < 10^A$, where $A = 10^B$, and $B = 10^{34}$. The smallest such integer x is often called the *Skewes number*; for ease of reference, we'll denote this number by x_S. (Skewes and Turing rowed together regularly in Cambridge [33]. As will become clear in §5, Turing first heard about Skewes' work in that setting, with Skewes "rowing two" and Turing positioned at bow.)

- During his first year at Princeton (1936-37), Turing keeps in touch with Ingham; he also speaks occasionally with G. H. Hardy, who was visiting for a semester [9, p.117]. Sometime prior to June 1, 1937, the date of Ingham's first Archive letter, Turing mentions to Ingham that he has become interested in trying to attack the x_S-problem by sharpening the original reasoning used by Littlewood in 1914; cf. Eq. (2) and [10, p.92ff]. Ingham offers encouragement, but suggests that his recent, alternate proof [11] for Littlewood's theorem may be more amenable for this purpose. He encloses an offprint, noting that Skewes has apparently tried the approach – only to come up with (a very likely improvable) upper bound 10^{19} for B, in place of the original 10^{34}.

- Back in Cambridge during the Summer of 1937, Turing pursues Ingham's suggestion with $\psi(x) - x$, a function closely related to $\pi(x) - \mathrm{Li}(x)$ (still assuming the truth of the RH). He obtains a bound much better than Skewes' and communicates this to Ingham. The draft manuscript for this, which appears to be [29, pp.147–151] (or something quite similar) makes use of a variant of [11] and several key $\zeta(s)$ estimates, including one from Titchmarsh's 1936 paper [25] on the numerical verification of the RH for $\mathrm{Im}(s)$ ranging up to 1468. In his second letter (dated Sept. 18), Ingham reacts positively to Turing's work [without checking every calculation] and conveys the information that Littlewood and Skewes have just about finished deriving a bound for x_S wherein nothing is assumed about the truth of the RH. Ingham refers Turing to a recent paper of Littlewood that obliquely touches on the matter; see [17, pp.838–843]. (N.B. the '1948' appearing on p.149 in [29] is *not* present in the original Archive manuscript; Turing only wrote p.324, which corresponds to the 1933 edition of Jahnke/Emde, *Funktionentafeln*. See also the comment by A. M. Cohen in [29, p.272].)

- Now back in Princeton, Turing's interests begin to shift more toward $\zeta(s)$ per se, especially its zeros and the matter of extending [25] past $t = 1468$. Apart from the work's *intrinsic* merit (including in exploring further the skepticisms about the RH voiced by Titchmarsh on the final pages of [24, 25]), Turing surely realized that gaining control on a larger initial set of zeta zeros would facilitate a better bound for x_S. The idea of building a special purpose "gear-wheel" computer [9, p.140ff] to evaluate the sum function called for in the main numerical part of [25] probably arose during this period. Titchmarsh's letter (of Dec. 1) makes reference to this; he describes the idea as very interesting and advises Turing that, in the work he proposes, higher-order correction terms may be needed to secure proper accuracy in ρ. He also cautions "it may be that, like with $\pi(x) - \mathrm{Li}(x)$, $\zeta(\frac{1}{2} + it)$ may go on for a very long time before revealing its true character."

• On Dec. 9, 1937, Skewes writes from Cape Town, where he worked, and reacts positively to Turing's improved bound for x_S from that summer. From the letter's wording, it is evident that both are occupied with other work at the moment (for Turing, this was his Ph.D. dissertation [9, p.145]). Skewes writes that he cannot get back to Cambridge for another two years – but promises to give some details about the "RH false case" in his next letter. No such letter is found in the Archive. (Though Skewes' Cambridge dissertation was accepted in 1938, it was not readied for journal publication until 15 years later [23]; an interesting popular account can be found in §14 of Littlewood's 1948 article [15].)

• Turing receives his Princeton Ph.D. in June 1938 and, shortly afterward, returns to England. It is not until 1939 that he resumes work on $\zeta(s)$. On March 7, 1939, Turing submits [27] for publication. In very loose terms, [27] seeks to address some of the "correction term issue" that Titchmarsh raised in his 1937 letter by passing to an *alternate* (smoother) version of the $\zeta(s)$-expansion utilized in [25] whose basic error term appears to be both smaller and more readily estimable than the one employed previously. Emphasis is placed on s-regions (both on and off the critical line) likely to be pertinent in a "gear-wheel" setting. The paper is very technical and, as noted by Heath-Brown in [29, p.261], was soon made unnecessary by the advances that occurred when electronic computers became available. The influence of C.L. Siegel's celebrated 1931 paper on $\zeta(s)$ based on material found in the Riemann Nachlass is plainly visible at several places in [27]. Expansions similar in spirit to [27] continue to be useful in a variety of other contexts; cf., e.g., [2, 18] and [20, §3].

• Turing submits his £40 proposal for construction of a "zeta function machine" to the Royal Society on March 24, 1939. (See [31].) Its stated aim is to extend the range of Titchmarsh's work on the RH by a factor of about 4. Due to the onset of World War II, the proposed machine is never completed.

5. A Return to Basics

As we just saw, Turing's fascination with $\zeta(s)$ actually originated in a very basic question about the ordinary prime numbers $\{2, 3, 5, 7, 11, 13, 17, \ldots\}$. In light of their structural and aesthetic "starkness", it is not too surprising that, over the years, the primes would continue to retain a certain attractiveness for Turing.

Most papers dealing with Skewes' problem of trying the find x_S, the smallest integer x for which $\pi(x) > \mathrm{Li}(x)$ are very technical. The two drafts in [29] devoted to this topic (viz., pp. 147–151, 153–174) are no exception. The second, "On a theorem of Littlewood", ostensibly written jointly with Skewes, is described by J. L. Britton on pages XIV and 273 of [29] as having been in all likelihood prepared *solely* by Turing. Ingham, who studied the manuscript carefully in the early 1960s, expresses an equivalent view in [8, p.99]. Since, as we shall see, the work is a significant one [its unpolished state notwithstanding], it is only natural to want to understand its background a little more clearly.[3] Our efforts in this direction have been aided in no small way by the fortuitous help that we received from A. M. Cohen, K. Hughes, J. Webb, P. Sarnak, S. B. Cooper, and Stephen Skewes (Stanley Skewes' son).

We have already outlined the pre-World War II situation in §4. To take things further, we need to say just a few more words about Ingham's 1936 paper [11]. Riemann's formula in Eq. (2) gives an explicit representation for $\mathrm{Li}(x)-\pi(x)$ as a sum over the nontrivial zeros ρ and the point $\frac{1}{2}$. (By abuse of language, we can temporarily regard $1/2$ as a ρ.) As was mentioned in §2, the ρ-sum requires technical virtuosity to handle and, even then, yields relatively poor results. Ingham's breakthrough was the observation that certain (sliding) weighted averages of $\mathrm{Li}(x) - \pi(x)$ can be represented as sums over the ρ that are far more tractable, with terms that decline rapidly as the heights of the ρ grow. *Insofar as* this type of mollified sum *can be made* negative, at least some of the values of $\mathrm{Li}(x) - \pi(x)$ that go into the average have to be negative as well, provided that the weights used in the averages are all nonnegative. (When, as in [11], the RH can be assumed, the key issue ultimately boils down to arranging things so that many sinusoidal ρ-terms "all pull in the same direction" so as to successfully overpower something positive.) As such, the method usually does not produce any single counterexample to the $\pi(x) < \mathrm{Li}(x)$ conjecture, but it *does* disprove it, and, if things are kept explicit enough, at least furnishes a region in which a counterexample has to lie.

Similar approaches have been developed for other number theoretic conjectures, such as that of Mertens. Typically, successful applications of such methods require high-precision values for some initial set of nontrivial zeros ρ, and knowledge that a considerably larger [finite] set satisfies the RH (the latter to help ensure the negligibility of all those terms past a certain ρ-threshhold).

It is interesting to observe that, already in manuscript [29, pp.147–151] from 1937, the mollification factor adopted by Turing is one of Gaussian type

[3] As of Spring 2011, neither the original nor Britton's photocopy could be found in the Turing Digital Archive. Compare: [29, p. IX(bottom)]. Notice, too, that no date is offered for this work in [29].

– exactly as would be appropriate as a "first guess" in a setting in which there were some sporadically occurring off-line zeros in need of suppression in Eq. (2). The mollification choices adopted in Turing's second unpublished manuscript on this problem, "On a theorem of Littlewood" ([29, pp.153–174]), OTL from now on, can be seen as building on that used in 1937.[4]

As the letter reproduced on page NTT clearly shows, Ingham and Britton's view about the authorship of OTL is correct. (Skewes submitted his work [23] for publication in December of 1953. Consistent with the letter, his exposition makes no mention of OTL. The memorable phrase on p.50, line 10 may hint at one of Skewes' complications.)[5] In light of the unhappy events of the first part of 1952 ([9, pp.471–473]) and the inherent complexity of its estimates, it seems reasonably safe to hypothesize that the preparation date of OTL falls somewhere between mid-1952 and early 1953.

Such a timeframe would also be consistent with Turing's use of the phrases "digital computer" and "ten to twenty hours of computation time" on p.168 of [29], not to mention the general mathematical mindset of the surrounding lines. Notice too that some similar "accounting-type" language occurs in [28, pp.112–116].

Though it is possible that the *work* for certain parts of OTL may actually have transpired some time prior to the drafting of any manuscript, the general sloppiness of Turing's typescript (we were able to secure a copy of A. M. Cohen's photocopy) tends to suggest that any "time gap" is one of relatively modest size. Having said this, however, there may *still* be some value in noting that, during the ten-year period prior to 1953, there were five occasions on which a "rekindling of x_S ideas" might well have occurred on one level or another:

- Prior to moving to Manchester, Turing spent the 1947-48 academic year in Cambridge. As it turns out, Skewes was also there on sabbatical for at least the first half of 1948, presumably doing some (pre-publication) fine-tuning of his x_S thesis work with Littlewood. A letter dated 30 September [1948] from Littlewood to Skewes (made available to us courtesy of John Webb) implicitly confirms the primary topic of their discussions, as well as Littlewood's close involvement. After a 10-year hiatus, one has to assume that Turing and Skewes occasionally talked.

[4] In §2–5 of OTL, part of the idea is to imitate [11] by using an "approximate identity" interpretation of the Gaussian; cf. the bottom half of page 154, 158(top), and 166 (lines 10, 16, 20–21). In this connection, see also lines 22–23 in Ingham's commentary, *op. cit.*

[5] The phrasing of item 4 in [30, p.266] suggests that Turing may well have apprised Robin Gandy about his predicament with Skewes at some point.

• Littlewood's popular account [15, §14] of the Skewes number also appeared in 1948 (July, to be more precise).

• During the 1949-50 academic year, there is some hint that, beyond his actual June 1950 experiment with the RH on the Manchester Mark 1, Turing may have also contemplated making calculations to a bit higher accuracy. See pp.99 (lines 6, 21–24), 100 (line 4), 104 (lines 5–6), and 114 (line 13) in [28]; also Digital Archive item AMT/B/32/image 98 and [9, p.406 (footnote)].

• In Archive letters dated Dec. 19, 1950 and Jan. 2, 1951, Ingham raises a number of machine-oriented computational issues closely tied to a possible disproof of Pólya's conjecture, a conjecture very similar in spirit to $\pi(x) < \mathrm{Li}(x)$. It is evident from the January letter that Ingham has prompted Turing to start thinking about this matter.

• In March 1952, Kreisel's work [14] appears. Section VI therein is devoted to a discussion of how to approach the Skewes problem along the lines that Turing originally wrote to Ingham about in the Spring of 1937. Kreisel presents no bound for x_S, however.

From a historical standpoint, it is fair to say that the significance of the first part of OTL (i.e., §§2-6) rests in Turing's realization, already 1952-53, that by a judicious choice of mollification factor, it would prove feasible to eliminate the awkward quantitative dichotomy between the RH being true or false (i.e., "H *vs.* NH") which was introduced by Littlewood and was required previously, including in [23], to secure an unconditional bound for x_S. And, further, that *in so doing*, a substantially superior x_S-bound would in fact accrue on the basis of using just several hundred ρ's.

Ingham offers a similar assessment in [8, p.99 (lines 19–24)] with a cautionary note about the manuscript's "very rough" state. That Turing's ideas were fundamentally sound was shown by Cohen and Mayhew in their 1965 work [6] (or [29, pp.183–205]) utilizing about 450 zeros, albeit to greater precision than was available in the early 1950s.[6]

In the second half of OTL (i.e., §7), Turing derives a bound for x_S on the basis of there being an "appropriately isolated" off-line zero ρ_0 in $\mathrm{Re}(s) > \frac{1}{2}$. Though the issue of obtaining an *optimal* x_S-bound in the specific setting of Theorem 3 may not have been looked at yet, results similar in spirit – even in more general settings – have been available for some years now in connection with the so-called Turán power sum method and comparative prime number theory. See, for instance, [12, 13, 19]. Somewhat curiously, the latter two references make use of an idea (cf. Theorem H*) found in the aforementioned work [14] on mathematical logic by Kreisel.

[6] The situation calls to mind Robin Gandy's comments in [30, p.9] about Turing's love of calculating, in particular Turing's quip "What's a factor of two between friends?"

While the letter on page NTT may initially suggest an unsettled, uneasy state-of-affairs, in stepping back from it, we find ourselves in agreement with a comment made to us by Andrew Hodges, particularly vis à vis the period 1952 - early 1953, a time of clear personal difficulty for Turing. Concerning the letter, Hodges writes:

> ...what it conveys to me is something else quite marvellous – the timelessness of pure mathematics, illustrated in the way AMT refers back to discussions while rowing many years before. Despite everything that has happened, the war and computers, there are the prime numbers and their mysteries just the same as ever, something he has thought about from time to time ever since.

6. Turing's skepticism about the RH

In his pre-World War II work on $\zeta(s)$, Turing seems to have viewed the RH as an open question, one that might easily be either true or false. In [27, p.197], for instance, he remarks rather nonchalantly that "[t]his may be of value for calculation of zeros not on the critical line." As suggested in §4, this attitude may have arisen partly from the numerically-based skepticism espoused by Titchmarsh in [24, 25]. Titchmarsh's 1937 letter would have only reinforced this. (Skeptical attitudes of this kind towards the RH were relatively common at the time.)

Based on the available evidence, it appears that by 1950 or so, Turing's earlier uncertainty about the truth of the RH had morphed into an outright skepticism. Thus, on p.169 of OTL, it is hard to ignore (even given the ambient "if") the telling phrase that "[i]t seems very probable that the first zeros off the critical line that are computed will satisfy the conditions ..." It is hard to imagine anyone with serious doubts about the existence of zeros that violate the RH writing like this. And, even more to the point, on the very first page of [28], Turing declares: "[t]he calculations were done in an optimistic hope that a zero would be found off the critical line." (The calculations to which Turing refers are those with $\mathrm{Im}(s) \approx 25000$. The ones with $\mathrm{Im}(s)$ less than 1000π were aimed more at simply extending [25]; see [28, p.116 (bottom)].)

What is a little puzzling is that Turing expected a counterexample to the RH to lie so low. The work of A. Selberg during the mid-1940s showed that the root mean square of $\mathrm{Im}\log\zeta(\frac{1}{2}+it)$, which to a large extent controls the distribution of zeros, grows about like $\sqrt{(\log\log T)/2}$ over any interval $[T, T+H]$ with, say, $H \approx T$. Similarly for the real part and for higher moments. In very rough terms, one also knows that large scale irregularities in the "sequencing" of ρ are linked to large oscillations in the aforementioned imaginary part; see, for instance, the first equation in [28, §4].

Accordingly, in order to reach regions wherein "relatively many pairs of ρ have popped off the line," it seems reasonable that one would need to have $\sqrt{(\log\log T)/2}$ fairly large. Since this radical grows *extremely slowly*, expecting to ever see any type of *systematic* collapse in the RH using machine calculation is probably out of the question. Phrased somewhat differently: any off-line zeros in Turing's experiment would likely have been *sporadic* in nature and required significant luck to hit upon. It appears based on Ingham's January 1951 letter that Turing was aware of at least some of the work of A. Selberg on $\zeta(s)$ from the 1940s. Even without that input, however, one might have thought that Turing, whose first research project was on random variables [9, p.88] and who had extensive experience with statistics in his cryptographic work, might have had some concerns along these lines. If he did, there are no traces of them recorded in [28].[7]

As for skepticism about the RH, some distinguished number theorists, such as Littlewood and Turán, died as disbelievers. In general, however, the climate of opinion appears to have moved substantially towards embrace of the validity of the RH. This is well illustrated by A. Selberg. In 1946, he expressed, if not outright disbelief, then at least a concern about the lack of evidence in support of this conjecture [21, §4]. In 2005, however, towards the end of his life, when he was interviewed by N. Baas and C. Skau, Selberg asserted "[i]f 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." (See [1, pp. 631 and 618 (paragraph 5)].)

The evolution in the thinking of Selberg and other researchers was driven by the accumulation of numerical data for the RH as well as heuristics (some from random matrix approaches) and proofs of analogs of the RH for somewhat similar functions (such as certain zeta functions defined over finite number fields). Had Turing lived longer, he might have modified his opinions about the validity of the RH, and might well have become involved in some of these researches.

[7] Compare p.168 (lines 27–32) in OTL, from a few years later.

The zeta function was of course just one of Turing's many interests, and not a major one. As can be seen from the record of his interactions with Skewes, say, he often put this subject aside for a number of years to concentrate on other topics. Still, the fact that he came back to it several times shows how interesting it was for him.

Had events transpired a bit differently in 1954, we like to think — as our *own* sort of "optimistic hope" — that circumstances would have evolved in such a way so that Turing's creativity would have continued to become piqued from time-to-time, prompting him to return occasionally to developments involving "the zeros and primes". With his insight and rare knowledge of the fields of number theory, analysis, probability, and computing that go into studying the zeta function, Turing could easily have emerged as a central player in this area.

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