# Appendix 9  A SHORT PROOF OF RIEMANN' S HYPOTHESIS

## Part I

Riemann's hypothesis, notably that the complex zeros of

(1) $\zeta(s) = \Sigma n^{-s} = \Pi(1- p^{-s})^{-1}$

in which *n* runs through the natural numbers 1, 2, 3, 4, … and *p* through the primes 2, 3, 5, 7, …, all have their real parts equal to ½, is true if

(2) $|\pi(n) - \mathrm{li}\ n| < |n^{½}|$

from some *n* upwards. In fact the *equivalent* to Riemann's 1859 conjecture[1] in terms of these formalities is a little weaker than (2), see Littlewood[2] pp88sq, Edwards[3] pp90sq, so I shall have proved his 147-year-old conjecture if I can prove that (2) is in fact true for some natural number *k*, say, and true without exception for all subsequent $n > k$.

I shall in fact prove

(3) $|\pi(n) - \mathrm{li}\ n| < |(\mathrm{li}\ n)^{½}|$

for all $n > 1$, which is more than enough because $\mathrm{li}\ n < n$ for all $(n)$ in this range and, since $\mathrm{li}\ n > 1$ in the range, $|(\mathrm{li}\ n)^{½}| < |n^{½}|$ for all these $(n)$. This will imply that (2) is true for all $n > 1$.

We can express (3) alternatively as, for $n > 1$,

(3A) $\mathrm{li}\ n - |(\mathrm{li}\ n)^{½}| < \pi(n) < \mathrm{li}\ n + |(\mathrm{li}\ n)^{½}|$ or, stated more precisely in words, an absolute lower limit to $\pi(n)$ is determined by li *n* plus its negative square root, and an absolute upper limit to $\pi(n)$ is determined by li *n* plus its positive square root.

Call a value of *n* for which $\pi(n)$* comes to within less than a unit of a chosen limit, but without crossing it, a *touching* of $\pi(n)$ to that limit. The strategy is to choose limits that yield the greatest possible number of touchings, rather like the casino game of blackjack, where a score touching the limit has the best chance of winning, and a score over the limit always loses.

In the summer of 1999, while still resident in London, I decided to try my putative limits to $\pi(n)$ in (3A) by comparing them with the actual prime counts. As expected for theoretical reasons detailed below, I found no touching to my upper limit of $\mathrm{li}\ n + |(\mathrm{li}\ n)^{½}|$, but was rewarded with a crop of no less than 106 touchings to my lower limit $\mathrm{li}\ n - |(\mathrm{li}\ n)^{½}|$, many of the prime counts coming dangerously close to the limit but, miraculously as it seemed, none of them crossing it.

It appeared I had discovered a glass floor to the prime-counting function that nobody hitherto knew existed. 'Glass' because there seems to be no compelling reason why the prime count, on coming so close to this arbitrarily-chosen (but admittedly rather beautiful) limit, should not occasionally go through it. But so many close encounters with never a collision could not have happened by "chance", so there must be a compelling reason for it. In fact, the "glass floor" turns out to be more like a granite rock, with the inexorable instruction to any prime count that approaches it, 'Thou shalt not pass'.

I found I could use this rock-like barrier to predict unambigously all the primes from $p_1 = 2$ through $p_{48} = 223$ on the grounds that if the prime count at *n* was so close to the limit that the same count at $n + 1$ would have taken it through, $n + 1$ would have to be prime to jump up the prime count and prevent this from happening.

The 106 touchings range from $n = 2$, the least, through $n = 568$, the greatest. The closest approach to the limit occurs at $n = 58$.

I give a sample including these salient points below.

* or any other count of primes over a finite sequence of $(n)$, such as my limits for primes between squares.

| $n$ | $\pi(n)$ | lower limit<br>li $n - \lvert(\text{li } n)^{½}\rvert$ | margin $\pi(n)$ – floor |
|---|---|---|---|
| 2 | 1 | 0.023 | 0.997 |
| 28 | 9 | 8.903 | 0.097 |
| 58 | 16 | 15.950 | 0.050 |
| 222 | 47 | 46.934 | 0.066 |
| 568 | 103 | 102.010 | 0.990 |

Table 1

Let us formalize, and if possible generalize, what we did. We chose a known asymptote to $\pi(n)$, in this case li $n$, and made it a median $M(n)$ to two curves at equal distances $d$ above and below the curve of $M(n)$. And we chose $d$ to be some more or less simple fraction of $M(n)$. This we can call the first phase of the procedure. The second phase is to adjust $d$ (if necessary) to include between the two curves $M(n) - d$ and $M(n) + d$ all the prime counts we know about, say up to $\pi(k)$, $k$ being the largest number for which the prime count, and that of all $n < k$, is known. Thus

(4) $M(n \le k) - d < \pi( n \le k) < M(n \le k) + d$

in which $\pi(n \le k)$ includes all the prime counts known so far.

It is evident that if we choose a median that is "on average" greater than $\pi(n)$ and adjust $d$ so that $M(n) - d$ is about right, $M(n) + d$ will be too high. $M(n)$ = li $n$ is known to be "on average" greater than $\pi(n)$, see Ingham[4] pp 105sq, and this is confirmed by the fact that we get no touching at the upper limit. Similarly if we choose a median, such as $n/\log n$, known to be "on average" lesser than $\pi(n)$, and adjust $d$ so that $M(n) + d$ is about right, $M(n) - d$ will be too low.

In fact we can use my limits, originally designed for primes between squares (see Appendix 7), $A \pm (B - 1)$ where $A$ is $n/\log n$ and $B$ is $A/\log A$, as a ceiling and floor* to $\pi(n)$. Now $M(n) = A$ and $d = B - 1$. We get 8 touchings to the ceiling and none in this stretch of ($n$) to the floor.

| $n$ | $\pi(n)$ | upper limit<br>$A + B - 1$ | margin<br>ceiling – $\pi(n)$ |
|---|---|---|---|
| 19 | 8 | 8.914 | 0.914 |
| 109 | 29 | 29.620 | 0.620 |
| 110 | 29 | 29.824 | 0.824 |
| 113 | 30 | 30.434 | 0.434 |
| 114 | 30 | 30.637 | 0.637 |
| 115 | 30 | 30.839 | 0.839 |
| 199 | 46 | 46.960 | 0.960 |
| 283 | 61 | 61.985 | 0.985 |

Table 2

It is clear that the more centrally we place the median in relation to the prime count, the narrower the band of errors between the upper and lower extremes of the count will become, so it would be possible to approach the most central median experimentally, by continuous readjustment. In 1932 Ingham[4], without the assistance of electronic computers or calculators, did better than this and published a conditional formula for the most-central median, which we might call the *mediant*, to $\pi(n)$. But since I can complete a proof of the Riemann hypothesis by employing as a median li $n$, skew** though it is, I propose to do so before considering the above and other possible refinements.

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* We go through the floor at $n = 2$, but the ceiling is so good that we can accept this one flaw in the floor.

** It would be more-appropriate to attribute the skewness to $\pi(n)$, which is increasingly on the low side of the asymptotes that are its proven representatives as we approach 1 from above. This is because the asymptotes are continuous functions, whereas primes can exist only in the discontinuous medium of the natural numbers, so the

Let us summarize what we have found so far. We have taken asymptotes to the prime count $\pi(n)$ and employed simple fractions of these asymptotes to create bands wide enough to contain all the prime counts so far known. We have noticed that the touchings of the prime count to any one of the limits imposed by these bands always appear to be finite in number, and occur only when *n* is quite small, *n* in all known cases being less than a thousand.

We can note further that the last (or what we suppose is the last) touching in any case as *n* proceeds upwards is the last time a zero appears in the integer part of the margin between the prime count and the error-limit we have chosen. It is evident, as the reader may discover, that we can repeat the procedure to find the last time the integer part of the difference is 1, and then the last time it is 2, 3, 4, and so on. All this suggests we are observing something that is very regular and lawful.

This is to be expected since I pointed out[5] in 1969 that a thing and what it is not must share the same definition, and so are in this respect mathematically identical. It has long been (and still is in some quarters) the fashion to think of the primes as somehow lawless and devilish. It was indeed this very thought that stopped Littlewood[2], in 1907, from continuing his attempt to prove Riemann's conjecture. He began correctly, in the same way that I do, but gave up after six days in the false belief that the 'devilment' in the primes would make his task impossible.

There is no devilment in the primes. Their complement is the set of multiplication tables, which may be considered perfectly lawful. The primes are merely numbers that don't appear (except trivially as multiples of 1) in multiplication tables and so, thus sharing their definition with the composites, must be exactly equally lawful.

It appeared that, in seeking to prove Riemann's conjecture, I had to find ways of measuring this extreme lawfulness of the primes that the text books of arithmetic have for the most part missed. It evidently occurs in prime counts. In fact the primes make adjustments, that appear almost like a conscious effort, to keep their count within bounds. Just before every interval with very few primes, which I call a prime desert, they build up their numbers by producing what I call a prime forest, an interval containing more primes than usual. Just before the desert between 199 and 223, containing only one prime, there appears a forest of primes between 190 and 200 to build up the count so that it won't be too depleted at the end of the ensuing desert. Thus

| $n$ | $\pi(n)$ | lower limit<br>$\text{li } n - \lvert(\text{li } n)^{1/2}\rvert$ | margin<br>$\pi(n)$ – floor |
|---|---|---|---|
| 199 | 46 | 42.932 | 3.068 |
| 222 | 47 | 46.934 | 0.066 |

Table 3

Without this buildup of a safety-margin before the beginning of the desert, the prime count at the end of it would have gone through the floor.

The power of my procedure is that we can adopt any limits we like, based on proven asymptotes, for the errors in the prime count. Whatever limits we adopt, we shall still find a regular procession of last times for the integer parts of the margins. We can check that this phenomenon continues on a regular basis in respect of all the numbers for which we already know, or can calculate, the prime count. If it continues to continue this way for all the numbers for which we do not yet know, or can never calculate, the prime count, then Riemann's hypothesis is true. If his hypothesis is false, then we can be sure that in some far-distant region of the number-system, somewhere beyond the $10^{24}$ or so numbers for which we already know, or can calculate, the prime count, the regularity of the errors will have gone wrong, not just a little bit, so as to breach one of my limits in (3) or (3A), but terribly wrong, enough to breach the Riemann limit in (2), not just once, for if we could show that any such breach were the last time it could happen, the hypothesis would still be true – no, to falsify Riemann's

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chances of a "calculated prime" hitting a natural number get worse and worse as we go down the scale. As we approach unity the comparison between the calculated approximation, on the basis of $x/\log x$, to the number of primes up to, shall we say $x = 1+ 10^{-6}$, which is a million and one and a half very nearly, and the actual number, which is zero, becomes quite ludicrously inept.

hypothesis this substantial breach of the regularity of the prime counts would have to go on happening an infinite number of times, again and again, for ever and ever amen.

This seems, in the light of what we have discovered already, unimaginably unlikely, but 'unimaginably unlikely' is not a proof. A proof is to produce some good-enough reason *why* the scenario I have just described *cannot possibly happen*.

If it is true, as we have found so far, that the touchings of any prime count to any asymptotic limit we devise for it, are always finite in number, and we can say similarly for the integer parts 1, 2, 3, … of the margins between these counts and whatever limits we have devised for them, then this is equivalent to saying that the maximum errors in the prime counts, compared with the kinds of limit we have devised for them, must shrink as $n$ grows larger. They do not need to shrink for the hypothesis of Riemann to be true, as long as they don't expand. But if we can find a proof that they shrink and, having shrunk, stay shrunk, this will be more than good enough.

But this phenomenon has been known since Tschebycheff, and was proved by him in the middle of the 19th Century. For an example I can take Sylvester's 1892 improvement on Tschebycheff's upper limit to $\pi(n)$, determined at $n/\log n \times 1.04423$. So between them they proved that

(5) $\pi(n) < 1.04423(n/\log n)$

for all $n$ greater than some largeish number $k$.

I remember many years ago I determined exactly what $k$ is, but we do not need to know this for my present illustration. It is certainly greater than $n = 10^{10}$, because $\pi(10^{10}) = 455\,052\,511$ and $1.04423(10^{10}/\log 10^{10}) = 453\,503\,326.839$ and this is smaller. All we need to know now is that the Tschebycheff-Sylvester inequality in (5) will eventually become true, and, more importantly, true for ever after, when $n$ has reached $k$, which is I guess in the region of about $10^{12}$, it doesn't matter.

This proves that the maximum positive error of the prime count from its mediant definitly shrinks and stays shrunk as $n$ grows larger, so all that remains is the proof of a similar shrinkage of the maximum negative error*. Again I use the Tschebycheff-Sylvester formula. I could equally well use Tschebycheff's limits, but I don't have them to hand.

(5A) $\mathrm{Ts}{\downarrow}n = 0.95695(n/\log n) < \pi(n) < 1.04423(n/\log n) = \mathrm{Ts}{\uparrow}n$

for all sufficiently large $n$.

This time to discover what order of ($n$) is 'sufficiently large' is much easier.

| $n$ | $\pi(n)$ | $\mathrm{Ts}{\downarrow}n$ | within limit? |
|---|---|---|---|
| 2 | 1 | 2.761 | no |
| 3 | 2 | 2.613 | no |
| 4 | 2 | 2.761 | no |
| 5 | 3 | 2.973 | yes |
| 6 | 3 | 3.205 | no |
| 7 | 4 | 3.442 | yes |
| 8 | 4 | 3.682 | yes |
| 9 | 4 | 3.920 | yes |
| 10 | 4 | 4.156 | no |
| 11 | 5 | 4.380 | yes |
| > 11 | | | yes |

Table 4

* It is of course strictly unnecessary to prove the shrinkage on both sides of the mediant, since the shrinkage on one side will shift the mediant to make a similar shrinkage on the other. Thus a more-elegant lemma to my proof would be to choose just one known uniquely high peak (or uniquely deep trough) in the prime count, such as the peak at $n = 113$ in $\pi(n)/(n/\log n) = 1.225\,058\,713$, that has already been independently proved can never be repeated or approached again.

We see from Table 4 that there are only five breaches of the Tschebycheff-Sylvester bottom limit for $\pi(n)$, and that 'sufficiently large' in this case means $\geq 11$. That there are no further breaches for $n > 10$ is confirmed by Rosser and Schoenfeld[6].

This proves that the maximum error of the prime count on both sides of its mediant shrinks and stays shrunk as $n$ grows larger.

To complete my proof of the Riemann hypothesis I had to find proven limits to the prime count that are independent of mine, and that would establish the principle of shrinkage on which mine are based. I would blush to admit how long I spent trying to compose an independent proof of the principle of shrinkage before realizing that it had already been done for me 150 years ago.

So the permanent shrinkage of the maximum errors on both sides of the mediant value of the prime count has been proved, and this implies that the prime count must stay within my limits of (3) and (3A), and this in turn implies that (2) is true from $n = 2$ upwards, and this in turn implies the truth of the Riemann hypothesis QED.


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## Part II

Now that I have proved it, it is time to take a closer look at what the Riemann hypothesis is about. It is about the errors (that is, the wanderings) of the prime count *from itself.*

Since the plotted curve of $\pi(n)$ is a bit irregular, the most plausible way to measure the errors is to plot a curve of the peaks of $\pi(n)$ on the one side, and another curve of the troughs of $\pi(n)$ on the other. Now the mediant of these two curves is the curve that runs exactly equidistant between them, and this we can call, for short, the mediant of $\pi(n)$.

To calculate this mediant for any finite $n$ is easy enough, but what we really want to know is the mediant of $\pi(n)$ for all $n$ to infinity, and this looks as if it might be more difficult to find. It is important to find it if we can, because it is the errors as measured from this, what we might call the *absolute mediant* of $\pi(n)$, that are what Riemann's hypothesis* is really about.

Fortunately Riemann's hypothesis is rather weak, which is why I could use a proven asymptote, li $n$, to this absolute mediant, and treat the asymptote as if it were the mediant itself. Although the asymptote li $n$ is some distance from the absolute mediant, it is close enough for the weak RH to fall within its ambit. Hence my easy proof.

Ingham[4], pp 105, etc, performs a series of remarkable calculations to show, conditionally, "that li $n$ – ½ li $n^{1/2}$ is 'on the average' [his quotation marks recognize the imprecision of his language for what I have detailed more precisely in terms of median curves] a better approximation to $\pi(n)$ than li $n$". The condition for his theorem is that the Riemann hypothesis be true, so by proving the hypothesis I promoted his theorem from conditional to absolute.

Ingham's theorem is in fact more informative than his careful provisos would suggest, for I can easily show that it computes exactly the absolute mediant to $\pi(n)$ that we have been seeking, and will serve, therefore, as a basis for my next series of theorems, which will impose much narrower limits on the possible errors of $\pi(n)$ than Riemann's did.

Ingham's theorem is not intuitively obvious, at least not to me, but I had meanwhile devised two more functions, which I call bli $n$ and $\lambda(n)$, that must compute values that are close to the absolute mediant of $\pi(n)$, if not spot on, and these functions share the advantage of being intuitively obvious.

What are now called brownian logarithms (see Appendix 7, note at the end), were devised by me in 1999 to yield a better estimate than li $n$ of the mediant value of $\pi(n)$. The brownian logarithm $\log_b n$ is the logarithm of $n$ to the dependent base $b = (1 + n^{-1/2})^{n^{1/2}}$. It is written lob $n$, and the brownian logarithmic integral bli $n$ is computed in an exactly similar way to the traditional logarithmic integral li $n$, notably by

$$(6) \qquad \text{bli}\, n = \lim_{\varepsilon \to +0} \left( \int_0^{1-\varepsilon} + \int_{1+\varepsilon}^{n} \right) \frac{dt}{\text{lob}\, t}$$

Define the *density point* dp($x$) to be the real number $r$ at which the density d($x$), in primes per integer, at the real number $x$, is exactly $\log^{-1} r$. According to Gauss's guess gg, the density of primes (presumably in primes per integer, although he didn't say so, and presumably at $x$, although he didn't this either) is $d(x) = \log^{-1} x$ and we should have no need to introduce the new number $r$. But in practice in all these comparisons of prime counts with the exponential functions we have chosen to represent them, we find that the representation, particularly in the early reaches of ($x$), is not quite exact. Since all the natural numbers, i.e. 2 and 3, in the neighbourhood of $e$ are prime, the prime density here is 1, and the density point is $e$. From here, considering it in relation to the centre point of successive integer squares, it rapidly rises to the higher of the two squares, from which it sinks asymptotically towards the half-way point between them.

* I suppose it should be called Riemann's *theorem* now that I have proved it, but the epithet *hypothesis*, having been used for so long, is likely to stick to it for ever, like Goldbach's *conjecture* (which I proved in 1998) and Bertrand's *postulate* (which Tschebycheff proved in 1848).

**Theorem (density point theorem)**

*The density point of the half-way point between successive perfect squares is bounded by the higher of the two squares and the half-way point between them (i.e. it stays exactly in the top half of the interval between the squares).*

My function

$$(7)\qquad \lambda(n) = \sum_{k=1}^{[n^{1/2}]-1} \frac{2k}{\log(k^2+k+½)} + \frac{\langle n^{1/2}\rangle 2[n^{1/2}]}{\log([n^{1/2}]^2+[n^{1/2}]+½)}$$

where <*x*> is the fraction part of *x*, and [*x*] the integer part, calculates what the absolute mediant of $\pi(n)$ would be if the density point of the concentration of primes in the interval between successive perfect squares were always at the exact half-way point between the squares (i.e. if gg were exactly true).

My Table 5 is remarkable in showing the near-identity of Ingham's conditional curve $R_2(n) = \text{li } n - ½\text{li } n^{½}$ for the absolute mediant of $\pi(n)$, the condition being the truth of the Riemann hypothesis, compared with my two curves bli *n* and $\lambda(n)$ which are both unconditional (i.e. they must indicate the mediant whether the RH is true or not). The fact that neither of my curves differs from Ingham's by as much as a unit even at $n = 10^9$ is startling evidence that the RH must be true, but of course no amount of empirical evidence constitutes more than a pointer unless it is backed by some principle ensuring that the prime count will always stay within certain limits.

| $n$ | bli $n$ | $R_2(n) = \text{li } n - ½\text{li } n^{½}$ | $\lambda(n)$ |
|---|---|---|---|
| 4 | 2.426 834 | 2.445 003 | 2.182 713 |
| 10 | 5.123 627 | 5.011 680 | 4.705 190 |
| $10^2$ | 27.356 750 | 27.043 342 | 26.670 954 |
| $10^3$ | 171.304 251 | 170.861 627 | 170.498 086 |
| $10^4$ | 1 231.611 320 | 1 231.074 145 | 1 230.678 850 |
| $10^5$ | 9 594.837 009 | 9 594.226 236 | 9 593.839 265 |
| $10^6$ | 78 539.415 762 | 78 538.744 333 | 78 538.341 943 |
| $10^7$ | 644 687.647 215 | 644 686.924 672 | 644 686.531 202 |
| $10^8$ | 5 761 587.073 71 | 5 761 586.307 11 | 5 761 585.901 09 |
| $10^9$ | 50 847 518.781 5 | 50 847 518.000 1 | 50 847 517.604 2 |

Table 5. Ingham's conditional mediant $R_2(n)$ to $\pi(n)$ compared with Spencer-Brown's unconditional mediants bli *n* and $\lambda(n)$.

I elaborate my formula (0) in Appendix 7 to

$$(8)\qquad \mathrm{R}_j(n) = \sum_{k=1}^{j} \frac{\mu(k)\text{li}\, n^{1/k}}{k}$$

where *j* is the number of iterations (i.e. successive values of *k*) we rquire to get a decent answer. My calculator is programmed to take *k* up to *j* = 11, i.e. to calculate $R_{11}(n)$, which differs only by a decimal place or so from $R_7(n)$ so is probably more accurate than we need*. Any further degree of accuracy

* Both $R_7(n)$ and $R_{11}(n)$ are accurate enough to show that the figure given in Lehmer's List of Prime Numbers under the heading 'Riemann' at $n = 10^6$ is wrong in the units column. For this *n* I compute

li *n* = 78 627.549 16

$R_{11}(n)$ = 78 527.402 17

both rounded and exact to ten significant digits. My figure for li *n* shows that Lehmer's column headed 'Tchebycheff' are all calculations of li *n* rounded to the nearest unit, and not false values obtained by beginning the integration in the wrong place (at 2 instead of zero) which Lehmer mistakenly insists that they are. Why has it taken 92 years for anyone to notice this major catastrophe? But there is worse to come. Edwards[3] p 2, gives a table of computations by Gauss that includes, for $n = 10^6$, $\int \frac{dn}{\log n}$ = 78 627.5. Edwards whinges that 'Gauss does manifests only in some remote decimal place, and is entirely swamped by the errors in the prime count, so the extra labour of taking *j* higher contributes nothing of any interest to our approximate knowledge of the prime count. I mention this merely to explain to the reader my otherwise mysterious use of the sign $R_2(n)$ to represent li $n$ – ½li $n^{½}$. It merely takes the formula in (8) up to the second iteration of *k*.

My proof of Riemann's hypothesis confirms Ingham's conditional inference that $R_2(n)$ is a reliable indicator of the absolute mediant to the peaks and troughs of the errors of $\pi(n)$, and the fact that it falls smack in the middle of my calculations of the independent functions bli *n* and $\lambda(n)$ suggests that $R_2(n)$ really is the absolute mediant to the errors of $\pi(n)$, or if not can differ from it only microscopically.

We can therefore in practice safely take it to be so. Now to get close enough to my original "glass floor" for $\pi(n)$, the new error limit must be exactly $\pm½(R_2(n))^{½}$, or in full $\pm½(\text{li } n - ½\text{li } n^{½})^{½}$, so that

(9) $R_2(n) - ½|(R_2(n))^{½}| < \pi(n) < R_2(n) + ½|(R_2(n))^{½}|$ for all $n \geq 2$, that is, for all positive prime counts without exception.

This, for example at $n = 10^8$, is more than four times stronger than Riemann's hypothesis, and the ratio gets better as *n* gets bigger*. And since we are now at the proper centre of the peaks and troughs of the prime count, we can expect to find touchings of the prime count to both limits.

There are just four touchings to my top limit, which I detail in Table 6, compared with 81 touchings to my bottom limit, a sample from which I list in Table 7. The first is at $n = 2$, the last at $n = 556$, and the closest to the limit is at $n = 28$.

It should be bourne in mind that since the primes can be defined only in the natural number system ($n$), they strictly have no existence in the continuum of real numbers, so the values of li $x$ and the other asymptotes at points between integers can have no meaning in respect of the prime count. See footnote on p 2.

| $n$ | $\pi(n)$ | top limit S↑$n$ = $R_2(n) + ½\|(R_2(n))^{½}\|$ | margin S↑$n - \pi(n)$ |
|---|---|---|---|
| 2 | 1 | 1.621 | 0.621 |
| 3 | 2 | 2.538 | 0.538 |
| 5 | 3 | 3.814 | 0.814 |
| 7 | 4 | 4.827 | 0.827 |

Table 6. The four touchings of $\pi(n)$ to Spencer-Brown's absolute top limit to $\pi(n)$.

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not say exactly what he means by the symbol $\int \frac{dn}{\log n}$'. Well, a simple computation would have settled the matter, wouldn't it? But Edwards doesn't wish to let on that he doesn't know how to do this, so he turns to the figure of 78 628, wrongly labelled by Lehmer as $\int_2^x \frac{dt}{\log t}$ for $x = 10^6$, and stupidly concludes that Gauss began the integration at 2 instead of correctly at zero which all 19th Century gentlemen were taught to do. And there is yet still worse to come. Every 20th Century author of arithmetic, including Ingham, and of course Hardy and Wright, has copied this mistake. It has become a religious shibboleth, accepted as gospel truth by referees who should know better but don't. This slipshod and slapdash approach to computing by so many 20th Century practitioners is undoubtedly among the reasons why the RH remained unproved for so long. Without the ready ability to compute li $x$ exactly to at least 3 decimal places, which members of former generations (including my father, who suffered under the same teachers as Littlewood) could all do by hand, my microscopically sensitive limits to the prime count could never have been discovered.

* In general, at $n = 10^{2k}$, my theorem is more than *k* times stronger than Riemann's.

| $n$ | $\pi(n)$ | bottom limit S↓$n$ = $R_2(n) - \frac{1}{2}\|(R_2(n))^{1/2}\|$ | margin $\pi(n)$ – S↓$n$ |
|---|---|---|---|
| 2 | 1 | 0.573 | 0.427 |
| 3 | 2 | 1.176 | 0.824 |
| 28 | 9 | 8.900 | 0.100 |
| 58 | 16 | 15.824 | 0.176 |
| 222 | 47 | 46.700 | 0.300 |
| 556 | 101 | 100.065 | 0.935 |

Table 7. A sample from the 81 touchings of $\pi(n)$ to Spencer-Brown's absolute bottom limit to $\pi(n)$.

In this context it is interesting to compare, for various values of $n$, my 'at most' figure for the amplitude of the errors of $\pi(n)$ with Littlewood's 'at least' figure for their amplitude at around the same value of $n$. These are given in Table 8. (My calculator works to twelve visible digits, the first ten of which are reliable* for calculations involving $n < 10^{12}$. For larger numbers the number of reliable digits may be less, but rather than guess I print all twelve and let the reader decide, or check by recomputing.)

Before my researches began, we knew of no appropriate 'at most' limit to the amplitude of the errors of the prime count, hence no proof of Riemann's hypothesis. But my limit of $\pm\frac{1}{2}(R_2(n))^{1/2}$, or in full $\pm\frac{1}{2}(\text{li } n - \frac{1}{2}\text{li } n^{1/2})^{1/2}$, is so much stronger than Riemann's hypothesis, and arrived at by more powerful methods that can be made entirely independent of Riemann's, that it must be classed as an altogether different theorem. The importance of Riemann's paper[1] is its ground-breaking nature, wherein even its troublesome conjecture pointed the way to theorems that, without it, might never have been noticed.

| $n$ | Littlewood's 'at least' value for the amplitude of the error $\eta(\pi(n)) > \pm\text{li } n^{1/2} \cdot \text{logloglog } n$ | Spencer-Brown's 'at most' value for the amplitude of the error $\eta(\pi(n)) < \pm\frac{1}{2}(\text{li } n - \frac{1}{2}\text{li } n^{1/2})^{1/2}$ |
|---|---:|---:|
| $10^{18}$ | 66 862 009.574 6 | 78 644 968.303 5 |
| $10^{24}$ | 52 249 153 086.2 | 67 888 879 369.0 |
| $10^{30}$ | $4.307\,877\,647\,62 \cdot 10^{13}$ | $6.060\,610\,302\,90 \cdot 10^{13}$ |
| $10^{36}$ | $3.675\,331\,864\,96 \cdot 10^{16}$ | $5.525\,602\,749\,35 \cdot 10^{16}$ |

Table 8. Littlewood's 'at least' figure for the amplitude of the errors of $\pi(n)$ compared with Spencer-Brown's 'at most' figure for their amplitude, calculated at $n = 10^{18}$, $10^{24}$, $10^{30}$, and $10^{36}$.


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* To check the accuracy of my program for li $x$, I used it to calculate Soldner's constant, which it finds between
    1.451 369 234 88 and
    1.451 369 234 89.
Weisstein gives
    1.451 369 234 6
but I don't trust his 10th decimal place. Do you?

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References

1. Riemann, Georg Friedrich Bernhard, Ueber die Anzahl der Primzahlen unter einer gegebenen Grösse, Monatsberichte der Berliner Akademie, November 1859.

2. Littlewood, J E, Miscellany, Cambridge 1986.

3. Edwards, H M, Riemann's Zeta Function, New York 1974.

4. Ingham, A E, The Distribution of Prime Numbers, Cambridge 1932.

5. Spencer-Brown, G, Laws of Form, London 1969.

6. Rosser, J B, and Schoenfeld, L, Approximate formulas for some functions of prime numbers, Illinois J. Math, **6**, 1962, 64-94.

References to work published before 1914 can be found in Dickson's History.