RUMINATIONS ABOUT ANALYSIS AND SCIENCE: STARTING DECEMBER 24 2021

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1. Prologue

For the past several weeks I have, at 49, decided to go through analysis again, to invigorate my own analytical sensibilities, and I did the Stanford Ph.D. Analysis Quals for 2013 and 2014. I thought about how I might produce a course on Analysis and I decides that the central example that made most sense for a firm understanding of analysis is that of a sequence of functions that keep their integrals but pointwise go to zero.

The example I think is central to all of analysis is this one. On $[-1, 1]$ let

$$g_n(x) = \begin{cases} n - n^2 x & |x| \leq 1/n \\ 0 & \text{otherwise} \end{cases}$$

These functions have the property that pointwise $g_n \to 0$ for all $x \neq 0$ and

$$\int g_n = 1$$

This is the center because theories of convergence of functions and integration have to *work around* this example and the convergence theorems that exist are all avoiding being trapped by this phenomena. The remarkable success of Lebesgue integration theory is that there are not many other serious obstructions.

2. Differences In Emphasis

There is a fine balance of things in mathematics. I am examining the honours-undergraduate book of Terence Tao where he is thorough regarding rigour on fundamental issues. I emphasize something else generally for my own understanding, and that is the *correspondence of mathematics and objective reality.* The latter to me is far more important than rigour today.

I think that it is always a miracle when mathematical objects faithfully tell us anything at all about physical phenomena, and that is the substance of mathematics, and to 'know mathematics' in my sense is to both have a feel for what is true about mathematics, and what will be true about natural phenomena. So from my perspective the practice is simultaneously to have some sense of when delicate reasoning is necessary and to have intuitive understanding of how to map mathematics to natural phenomena, the latter being the more important of the two, and one that demands development of innate sense. That's because nature does not have any oracle at all and there are no guarantees that any mapping will allow predictions that nature respects.

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Let's say you're looking at models of human speech and you want to model it by functions on $[-\pi, \pi]$. You might think that it is the convergence theory of Fourier series that is the sophisticated part of your travails. It is not. The sophisticated part is whether your choice of model actually faithfully represents the domain *at all*. That is where the deep subtleties lie, for once you are right, then you can just go through published results on Fourier series theory and just apply them.

My perspective is different from a dedicated mathematician and great teacher such as Terence Tao. It is that if there are published results that you can use, then just use them! You don't need to be an expert on things that are already done by others. But no publication will help at all if you are facing a situation of dealing with phenomena, and you have full control of what mathematical model to use and you totally botch it and use a mathematical model that does not faithfully represent nature. Who will you turn to in your need? Not angels, not humans, and already the knowing animals will be aware that you are not really at home in your interpreted world. So there is your deepest difficulty. You will face all of existence alone then.

You could obviously just not worry about the substantial problem at all. You could use pedestrian approaches of measuring what you can, be obsessed about linear statistical models and noise levels, and just write things with deceptive technical jargon that makes you look knowledgeable about nature with emphasis on various p-values. But that is not the work of a great scientist. That is the work of a pedestrian hack. A great scientist must have deep *conviction* that the models they choose are truth. And that is what is most interesting to me.

I will do some exercises in mathematics. Simple ones.

3. Whittaker-Watson II.1

Evaluate $\lim n \to \infty e^{-na} n^b$, $\lim n \to \infty n^{-a} \log(n)$ where $a > 0, b > 0$.

Intuitive answers are that both have limit zero. Let's attempt proof. We can do this with assuming known $f(x) = cx - \log(x)$ for $c > 0$ satisfies $f(x) > 0$ for $x > 1/c$, since $f(1/c) = 1 + \log(c)$ and $f'(1/c) = c - c = 0$.

Then we take the log of first quantity and see

$$y_n = b\log(n) - na = b(\log(n) - na/b).$$

We apply our assumed result with $c = a/b$ and see

$$\lim_{n\to\infty} y_n = -\infty$$

For the second, taking logarithm,

$$y_n = -a\log(n) + \log(\log(n))$$

and apply our result with $c = 1/a$ and $x = \log(n)$.

4. Whittaker-Watson II.2

Investigate the convergence of

$$\sum_{n=1}^{\infty}(1 - n\log(\frac{2n+1}{2n-1}))$$

The individual terms tend to $-\infty$, so the sum is divergent.

5. Whittaker-Watson II.3

Investigate the convergence of

$$\sum_{n=1}^{\infty}(\frac{1\cdot 3\cdots(2n-1)}{2\cdot 4\cdots 2n}\cdot\frac{4n+3}{2n+2})^2$$

Let's do this. Consider

$$(2n)! = A_n \cdot 2^n n!/2n$$

where terms inside the square are

$$\frac{A_n}{B_n}\frac{4n+3}{2n+2}$$

Also

$$B_n = 2^n n!$$

So

$$A_n/B_n = (2n-1)!2^{-2n}(1/n!)^2$$

Let's see, that's

$$A_n/B_n = n2^{-2n}\binom{2n}{n}$$

The last term is

$$\frac{4n+3}{2n+1}\leq 2$$

The series is bounded by

$$\sum_{n=1}^{\infty}(n2^{-(2n-1)}\binom{2n}{n})^2$$

The delicacy of this problem is seen from the Stirling approximation

$$n! \sim \sqrt{2\pi n}(n/e)^n$$

Then

$$\binom{2n}{n}\sim\frac{1}{\sqrt{4\pi n}}2^{2n}$$

The series will diverge, because inner terms will be

$$a_n \sim nCn^{-1/2}$$

I got the problem wrong. I don't have any answer key, but I don't want to correct my wrong answer. The error that I made above is an arithmetic error.

$$A_n = \frac{1}{2n}2^{-n}(2n)!/n!$$

This is the major error in my first attempt, that I did not put enough care in the exact calculation. Why did I make the error? It's because I knew that there was this "completion" but was sloppy with what algebra was needed to simplify expressions. Now let

$$C_n = \frac{4n+3}{2n+2}$$

This part gives us

$$C_n \leq 2$$

The total expression inside the square is

$$A_nC_n/B_n \leq n^{-1}2^{-n+2}\binom{2n}{n}$$

Now we use Stirling

$$\binom{2n}{n \sim \frac{1}{\sqrt{4\pi n}}2^{2n}}$$

Then

$$A_n C_n / B_n \leq n^{-1}(4\pi n)^{-1/2} 2^{2n} 2^{-n+2} 2^{-n} = \frac{4}{\sqrt{4\pi}} n^{-3/2}$$

Now the series will converge with integral test against

$$\sum_{n=1}^{\infty} (A_n C_n / B_n)^2 \leq \frac{16}{4\pi} \sum_{n=1}^{\infty} n^{-3}$$

I will comment that Stirling's formula is absolutely essential for anyone doing analysis because of the most significant intuition in this problem is the observation that

$$2^{-2n}\binom{2n}{n} \sim C n^{-1/2}$$

This is a fact that is much more important than the rest of the problem because this is deep intuition that does not come in any other way than an application of Stirling's formula. This tells us the analytical control of the middle term in the binomial expansion, and it is a miracle that there is an analytical expression for it. I would say that this intuition is worth more than the actual problem here because it is intuition that will help you in many other situation and it is mathematical *knowledge*. It's one of those miracles of life that an exact size estimate is available at all for the middle term of the binomial expansion. In mathematics, not all results are the same; some like this are more equal than others. Only experience will make this clear to you.

6. Whittaker-Watson II.7

Show that for $1 < \alpha < \beta$ the series

$$1 + \frac{1}{2^\alpha} + \frac{1}{3^\beta} + \frac{1}{4^\alpha} + \frac{1}{5^\beta} + \frac{1}{6^{alpha}} + \cdots$$

is convergent.

We use

$$\frac{1}{n^\beta} \leq \frac{1}{n^\alpha}.$$

The requisite series is bounded above by

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

Now apply the integral test

$$\int_1^\infty x^{-\alpha} dx = \frac{1}{\alpha - 1} < \infty$$

This one was easy only because it's much more familiar to me from high school.

What is interesting is of course that not all the techniques emphasized by Cambridge Tripos from 1915 survived later periods. This integral test technique did survive in young people's education. But others are not as familiar.

7. Residue At A Pole Of Meromorphic Function

If the function $f(x)$ has a pole of order m at $x = a$ then

$$f(x) = \frac{a_{-m}}{(x-a)^m} + \frac{a_{-m+1}}{(x-a)^{m-1}} + \cdots$$

The coefficient a_{-1} is called the residue of f at a. Augustin Louis Cauchy discovered the beautiful formula that allows us to calculate the residue all the way from 1820s. For analysis, calculation of various definite integrals is often not possible without using the residue formula.

I will be quite honest and suggest that the various beauties of complex analysis are not as important than whatever help complex analysis will give us to solve problems and get us *numbers*. I mean complex analysis is nice, it's really nice but we don't want to inflate the ego of complex analysis. We want numbers.

8. Interlude: Zulf's Way Of Looking At The Universe

Zulf's Way of looking at the universe is that the only things that matter in existence in an absolute manner are the things that interest Zulf and nothing else. Everything else is worthless. If Zulf is not interested in the issue, it's obviously fundamentally worthless and valueless. That's just the way things are.

Now this is not to say that Zulf is rude about it. Others do not have to hear about this devastating truth that nothing they are doing is actually worth anything. The solution is to keep quiet about this. Now romantic partners necessitate a more permanent solution to how to deal with things that are intrinsically worthless, i.e. things that are not interesting to me, but revealing the truth is socially inconvenient. So around lovers in social situation, I leave a small amount of room to display *polite interest* in things that are not interesting to me but might be to some other person. I try not to fake it, especially when in love because I do have a great and generous heart. I don't like my lovers to get some other idea. Thus the result is some *social grace*, i.e. showing interest in intrinsically worthless things that could have some short term interest in social gatherings and other sorts of events.

I emphasize that this sort of polite interest in intrinsically worthless things is an evolutionary adaptation and is necessary for survival.

9. There Are Best Books And Best Techniques In Mathematics

I have not been a professional mathematician for the past thirty years. I have, instead gone deeper, by my own efforts as well as in industry, into scientific problems. I have strong genius in science. This was evident to Lys K. Waltien in John Adams High School and I won third place in Westinghouse Science Talent Search. And over the years, I have read many books and articles, too many to list. And there are *best* treatments of various things.

The world would benefit if the best treatments were collated together and merged into canonical text made available to the world relatively inexpensively. I consider the *King James' Bible* and *The Qur'an* to be success in canonical texts that are successful and mathematics is much younger than the religions and has still much to learn about how to spread the best things to all people. I hope that this will happen in the next half a century as this sort of thing does improve the human condition.

10. Whittaker-Watson VI.1

A complex valued function $\varphi(z)$ is analyic on $B = \{z \in \mathbf{C} : |z| \leq 1\}$. Consider it as a function $\varphi(x+iy) = g(x,y) + if(x,y)$. The problem is to prove that for $-1 < x < 1$ that

$$\int_0^{2\pi} \frac{x\sin(t)}{1 - 2x\cos(t) + x^2} f(\cos(t), \sin(t))dt = \pi\varphi(x)$$

I will introduce a meromorphic function with a pole at a given $w \in B$ and write down the Cauchy integral formula for the residue at w.

$$\varphi(w) = \frac{1}{2\pi i}\int_\Gamma \frac{\varphi(z)}{z-w}dz$$

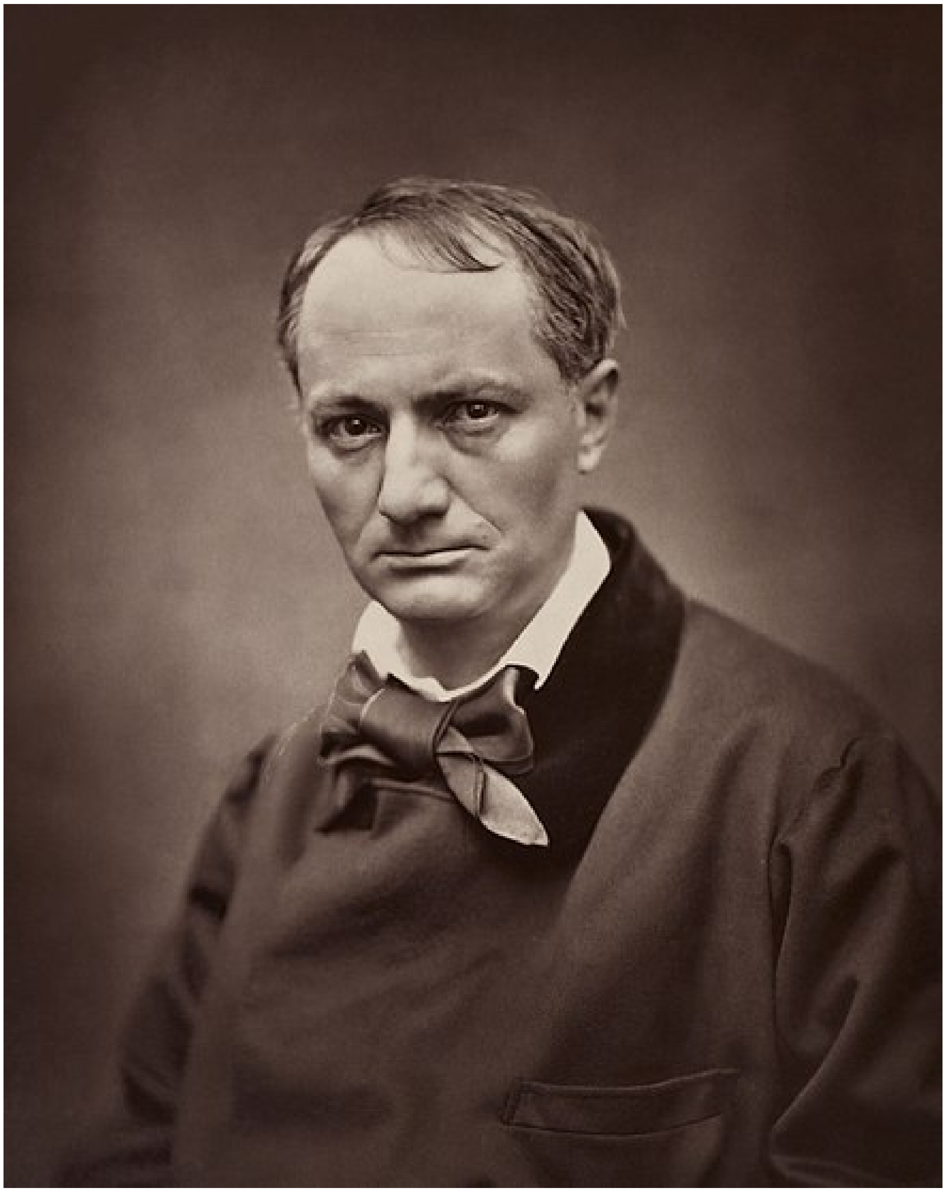
where Γ is the boundary of B. Then we will set $w = x$ for the real interval $-1 < x < 1$ and do various algebraic manipulations to get the result.

This will work, so I want to emphasize to my dear readers that unlike many other things in mathematics, the Cauchy integral formula is actually useful because it gives formulas and results that produce exact formula that you can evaluate. The numbers one can use to check against measurements from nature.

On a broader point, throughout nineteenth century, mathematicians were almost always also natural scientists. Augustin Louis Cauchy was a *gentleman*. Look.

You will notice that he looks like a normal gentleman with cultivated good manners and a slight polite smile that is not too much and not too little.

Let's compare his image to Charles Baudelaire.



Who would you rather befriend? You see Charles Baudelaire might be a genius in a way, but *Les Fleurs du Mal* is not central to great art. It's eccentric and rather special sort of taste for people who are looking for something a little naughty.

Anyway, Augustin Louis Cauchy, a great genius and a gentleman. I am just looking at Wikipedia rather than a more serious history, but I want to make this point very clear. Cauchy was an engineer first before he worked on mathematics at all. After finishing school in 1810, Cauchy accepted a job as a junior engineer in Cherbourg, where Napoleon intended to build a naval base. Here Augustin-Louis stayed for three years, and was assigned the Ourcq Canal project and the Saint-Cloud Bridge project, and worked at the Harbor of Cherbourg. Then his great work *Cours d'Analyse* was published in 1921. The point is that not a single great mathematician of nineteenth century was unaware of empirical natural science, and so they had natural sense for relationship between mathematics and nature that *virtually all twentieth century mathematicians lack*, ever since Einstein and Quantum theorists exiled mathematicians from physics. It was obviously a bad decision, because Zulf returns with a vengence in 2018 and today.

And now where is the vaunted Relativity and Quantum Field Theory of the physicists? What happened to Quantum Field Theory. It's sad, when a theory of a century is cast into oblivion by Zulf, isn't it?

Let us return to the problem at hand. Let us parametrize the boundary of B with $(\cos(t), \sin(t))$. We'll switch to $c = \cos(t)$ and $s = \sin(t)$ to make the computations legible.

First note that

$$\begin{aligned}\frac{1}{c+is-x} &= \frac{c-x-is}{(c-x)^2+s^2} \\ &= \frac{c-x-is}{c^2-2cx+x^2+s^2} \\ &= \frac{c-x-is}{1-2cx+x^2}\end{aligned}$$

This gives us the denominator in the requisite formula correctly.

Next I will consider $z(t) = \cos(t) + i\sin(t)$ and take differential, and calculate

$$\begin{aligned}(c-is-x)dz &= (c-is-x)(s-ic)dt \\ &= (cs-ic^2-is^2+i^2sc-xs+ixc)dt \\ &= ((xc-1)i-xs)dt\end{aligned}$$

Then we just take the *real part* of the quantity

$$\frac{1}{2i}\int_\Gamma \frac{varphi(z)}{z-x}dz$$

to get $\pi\varphi(x)$ because we know that all the imaginary part is zero.

$$\frac{1}{2i}\int_0^{2\pi} \frac{-xs}{1-2xc+x^2} if(c,s)dt$$

This will give us the result.

There is a significant gap in what I had done above, and this gap is nontrivial. In order to understand the gap, let us write

$$\varphi(x+iy) = g(x,y) + if(x,y)$$

for all $z = x+iy \in B$. Here g and f are *real valued* functions of two real variables. The gap is that there is, a priori, a contribution from g in the residue formula, and in order to conclude the requisite result, we need to show that this contribution is zero.

This is not immediately obvious to me at all *why* this contribution is zero. But at this point, I will *distrust* some automatic computation that will erase the integral and take a step back from the nitty gritty computation and try to understand the analytical situation.

It is clear that the hypotheses of the problem show that

$$\varphi(x) = g(x,0)$$

because $\varphi(x)$ is real-valued for $-1 < x < 1$. We want to understand this condition independently of the rest of the problem. The idea that I have is to return to multi-variable calculus.

We consider the *differential* in directions dx and dy of $\phi(x+iy)$ along the horizontal line $-1 < x < 1$ of the ball B.

$$d\varphi_{(x,0)} = g_x(x,0)dx + g_y(x,0)dy$$

One idea is to use the Cauchy-Riemann equations. They tell us

$$g_x(x,y) = f_y(x,y)$$
$$g_y(x,y) = -f_x(x,y)$$

Now on the entire horizontal line $-1 < x < 1$ we have

$$f(x,0) = 0$$

identically and this implies

$$f_x(x,0) = 0$$

This shows $g_y(x,0) = 0$ for all $-1 < x < 1$. This implies that

$$d\varphi(x)|_{(x,0)} = g_x(x,0)dx = f_y(x,0)dx$$

I will think about this more since this problem is unclear to me. The vanishing of the g component of the residue at x is a mystery.

Once again, I am not filled with self-loathing and crestfallen about this mystery. I am looking precisely for weaknesses of this type. These are quite sophisticated issues and not having the solution does not fill me with shame and feelings of failure. They are opportunities to understand some issues better.

This is from Trinity College 1898, and I think the problem is wrong. So I will do something to check.

Let's look at

$$\varphi(z) = z^2$$

Then

$$g(x,y) = x^2 - y^2$$

This will be a counterexample if we show

$$\int_0^{2\pi} \frac{(xc-1)(2c^2-1)}{1-2xc+x^2} dt$$

is nonzero. I will just numerically integrate it using R.

```
> gt<-function(t,x){ (x*cos(t)-1)*(2*cos(t)^2-1)/(1-2*x*cos(t)+x^2) }
> integrate( (function(t) gt(t,0.2)), lower=0,upper=2*pi)
-0.1256637 with absolute error < 9.3e-10
```

This shows that there needs to be a g term in the residue and Whittaker-Watson took the Cambridge Tripos 1898 problem that did not include one.

11. For Beauty Is The Beginning Of Terror

The beauty of classical complex analysis was overwhelming. For holomorphic functions f on domains $\Omega \subset \mathbf{C}$, we have for every nice closed curve $\Gamma \subset \Omega$,

$$\int_\Gamma f dz = 0$$

Years ago, when I was in high school, I was very keen on Physics C and for electromagnetism, there were the line integrals. I was very good at line integral calculations then. I felt, when I came across Cauchy integral formula that here was the way to do things, deform all sorts of curves and keep zero.

But of course, when one wants to have greater depth, one introduces real variable methods, and this is the terror. The theorems are much more powerful, but one slips into hard analysis. When I first heard about *hard analysis* at Princeton 1991-1995 I had shivers. I thought that all beauty would be lost. I was *topological* I thought to myself. I don't do the heavy labour of that sort of people who take beautiful situations where the spirit is free and the heart is light, and introduce not one or two but thirty different metrics, and suddenly everything has all manner of Greek letters and so many conditions that the sky fills up with conditions and one is trapped and all joy is gone from the world.

But now I am no longer youthful, and I accept that this is unavoidable; terror exists in the universe, and it is a matter of being an honourable responsible man to steel one's heart and engage with the terrible.

Rolf Nevanlinna initiated a beautiful theory of meromorphic functions in 1925. See the Cambridge Tripos of 1898 was written before Nevanlinna. So the sharper intuition of function theory that developed later did not exist yet. And so I do not have any disrespect for this error of Tripos writers of 1898. In any case, errors are important. I make them daily, and I never have any shame.

12. Whittaker-Watson Is Historically Valuable

Whittaker-Watson was published in 1915 when Lebesgues' theory was barely a decade old and is a fascinating window into a world very different from the entire period 1973-2021 and the world I know.

13. My Electromagnetic Potential Is The Truth And Coulomb's Is The Approximation

You see, I am a scientist. This means that when I model the universe, I don't have to accept Coulomb's potential as the truth in the universe. Indeed, for those who wish to see how I construct my potential in my Four-Sphere Theory notes, you will see that I will agree with Coulomb to a high degree of closeness for a large radius $\rho > 10^8$ say and then I will diverge from Coulomb's potential.

Who will challenge me? You better produce some precision measurements to distinguish between my potential and Coulomb's and until you do so, Zulf's electromagnetic potential shall remain eternal truth.

As a scientist, I have no problems proclaiming that my potential is the final potential of electromagnetism in nature.

Now you will say, but Coulomb is established. Who says this. Coulomb is established where? My superior-to-Coulomb potential is established within $\rho < 10^{10}$ meters. You mean you think Coulomb's inferior scientific claim of infinite range decay is more established than mine? Hardly likely. My electromagnetic potential is obviously the truth of nature.

Now you say, "Well, Zulf, you're cheating here because Coulomb was the great man who first showed radial decay of $|x|^{-1}$ for electromagnetic potential." Well he did something reasonable. He was a good boy. But he was just stepping stone for Zulf's final electromagnetic potential, and my law is the final law of nature.

You can do nothing about it. My potential is truth and resolvents of molecular Hamiltonians in my theory are bounded in L^2. And then you say, "Well, we'll stay with Coulomb." Well, be deluded. Show me why Coulomb's potential is better match to nature than mine.

14. Cambridge Tripos 2001 IA 4D

Starting from the theorem that any continuous function on a closed and bounded interval attains a maximum, prove Rolle's Theorem and deduce the Mean Value Theorem.

Rolle's Theorem says that if f is continous in $[a,b]$ and differentiable in (a,b), and that $f(a) = f(b)$ then there exists $x \in [a,b]$ with $f'(x) = 0$.

By the assumed theorem, f achieves a maximum in $[a,b]$. Let the maximum be x_0. If $x_0 = a$ or $x_0 = b$ we consider the function $g(x) = -f(x)$ and this allows us to reduce to the further restriction $f(x) \geq f(a)$ for $x \in [a,b]$. Now let x_0 be the maximum in (a,b). We know that $f(x) \geq f(x_0)$ near x_0, so

$$f'(x_0) = \lim_{h\to 0} \frac{f(x_0+h) - f(x_0)}{h} \leq 0$$

and

$$f'(x_0) = \lim_{h\to 0} \frac{f(x_0) - f(x_0-h)}{h} \geq 0$$

and therefore $f'(x_0) = 0$.

The mean value theorem is the statement that if f is continuous on $[a,b]$ and differentiable on (a,b) then there exists $x_0 \in [a,b]$ such that

$$f'(x_0) = \frac{f(b) - f(a)}{b-a}$$

Rolle's theorem is a special case with equality at endpoints. So given f we consider a new function

$$g(x) = f(x) + m_{ab}(x-a)$$

with

$$m_{ab} = \frac{f(b) - f(a)}{b-a}$$

We have $g(a) = f(a)$ and $g(b) = f(a)$. Apply Rolle's Theorem to obtain $x_0 \in (a,b)$ with $g'(x_0) = 0$ and deduce the Mean Value Theorem.

Let $f : \mathbf{R} \to \mathbf{R}$ is differentiable and $f'(t) > 0$ for all $t \in \mathbf{R}$ show f is strictly increasing. Conversely, is it true that if f is strictly increasing then $f'(t) > 0$ for all $t \in \mathbf{R}$?

Restrict attention to an arbitrary $[a,b] \subset \mathbf{R}$. Mean value theorem says that there exists x_0 with $f'(x_0) = \frac{f(b)-f(a)}{b-a}$. Suppose f were not strictly increasing. Then there will exist $a < b$ with $f(b) \leq f(a)$, take these, and get $f(b) - f(a) \leq 0$. The Mean value theorem will give us $f'(x_0) \leq 0$ for some $a < x_0 < b$ contradicting assumption on $f'(x) > 0$.

For the second part,

$$\lim_{h\to 0} \frac{f(x+h)-f(x)}{h} \geq 0$$

The question is whether the derivative can be zero. The example $f(x) = x^3$ is strictly increasing with $f'(0) = 0$.

15. Cambridge Tripos 2001 IA 10D

Suppose f is continuous on $[a, b]$ with $f(a) < f(b)$. Suppose $v \in (f(a), f(b))$. Show that there exists $c \in (a, b)$ with $f(c) = v$.

Let's see. We know $f^{-1}((f(a), f(b)))$ is open in $[a, b]$. We can then find open intervals I_a such that $f^{-1}((f(a), f(b))) = \bigcap I_a$.

Let me think about this.

16. Cambridge Tripos 2001 IA 11D

(i) Show that if $g : \mathbf{R} \to \mathbf{R}$ is twice continuously differentiable then given $\epsilon > 0$ we can find constants $L > 0$ and $\delta(\epsilon)$ such that

$$|g(x) - g(a) - g'(a)(x-a)| \leq L|x-a|^2$$

for $|x - a| \leq \delta(\epsilon)$.

(ii)

17. Thomas Simpson's 1740 Method

The iteration scheme of this problem is the same as that of Isaac Newton 1669, of Joseph Raphson 1690 and of Thomas Simpson of 1740. The general scheme

$$x_{n+1} = x_n - \frac{f(x_n)}{f'(x_n)}$$

was clarified by Thomas Simpson [5]. Babylonians are said to have used this scheme for square roots. This problem is actually *important* in science, which is rare in mathematics of this level of generality. I did not know that any serious theory existed at all for this scheme.

Ah, I knew something was interesting here. Nick Kollerstrom in 1992 argues that Newton should *not be credited* with the method that is called Newton's method and that the credit should go to Thomas Simpson for this to 1740 [6].

17.1. **A Warm-Up Exercise.**

I assume the results of 12D below. Let's start with $[a, b] \subset \mathbf{R}$. Then I consider $x \in [a, b]$ and write

$$\int_a^x (g'(s) - g'(a)) = g(x) - g(a) - g'(a)(x-a)$$

That's the fundamental theorem of calculus, proven in 12D part (ii). Now continuity of g' at x tells us for $\epsilon > 0$ we have

$$|g'(x) - g'(s)| < \epsilon$$

whenever $|x - s| < \delta$. Change $a = x - \delta$ and $b = x + \delta$ to ensure that on our interval $|g(x) - g(s)| < \epsilon$ holds uniformly. Then

$$|\int_a^x (g'(s) - g'(a))ds| \leq \epsilon|x-a|$$

Evaluate the integral within

$$|g(x) - g(a) - g'(a)(x-a)| \leq \epsilon|x-a|$$

Now since $|x-a| < \delta$ just put that in on the right side.

$$\epsilon|x-a| \leq (\epsilon/\delta)|x-a|^2$$

That will give (i).

This last statement was wrong. I can't get $|x-a|^2$ this way. Let's see

$$\|int_a^x g''(s) - g'(a)ds| \leq C|x-a|$$

This gives us

$$|g'(x) - g'(a) - g''(a)(x-a)| \leq C|x-a|$$

Then we look at

$$|g'(x) - g'(a)| - |g''(a)(x-a)| \leq C|x-a|$$

Then

$$|g'(x) - g'(a)| \leq C'|x-a|$$

Then

$$\int_a^x |g'(s) - g'(a)|ds \leq C'|x-a|^2$$

This will do it.

Now I will just punt on (ii) for now since I am a bit tired now. I'll return to it later.

For any j we have

$$x_{j+1} = x_j - \frac{f(x_j)}{f'(x_j)}$$

The delicate nature of this problem is to ensure that $f(x_j) > 0$ for all j for we have $f'(x) > 0$ for all $x \in [a,b]$ but $f(a) < 0$ so we could have $x_{j+1} < 0$.

I am not sure that decreasing sequence is guaranteed without $f > 0$ assumption, in which case it is trivial. It's certainly true that $f(b) > 0$. Then

$$x_1 = b - f(b)/f'(b) < b$$

Why can't $x_1 < 0$ now?

I am not sure how this problem would be solved. I will begin like this.

$$|f(x_j) - f(x_{j+1}) - f'(x_j)(x_j - x_{j+1})| \leq C|x_j - x_{j+1}|^2$$

Let's try this. Use Taylor series to write

$$f(x_{j+1}) - f(x_j) - f'(x_j)(x_{j+1} - x_j) = f''(x_j)(x_{j+1} - x_j)^2 > 0$$

I can prove with a bit of manipulation

$$f(x_{j+1}) > 0$$

Let me go through the manipulations.

Let us evaluate the Taylor series first at x_{j+1} with center x_j.

$$f(x_{j+1}) = f(x_j) + f'(x_j)(x_{j+1} - x_j) + \frac{1}{2}f''(x_j)(x_{j+1} - x_j)^2 + O(|x_{j+1} - x_j|^3)$$

One day we'll clean this proof up, but for now just assume

$$O(|x_{j+1} - x_j|^3) << \frac{1}{2}g''(x_j)(x_{j+1} - x_j)^2$$

This is the key. We won't worry too much about being rigorous about this because we're happy to have found something here. As an aside, mathematical substance comes in layers, and this is more important *mathematics* than rigour. This took me years to appreciate, that one has to keep moving and return to getting all the details clear later.

The substantial conclusion to draw from the above is

$$f(x_{j+1}) - f(x_j) - f'(x_j)(x_{j+1} - x_j) > 0$$

where we are relying strongly on the *positivity of* $f''(x_j)$ and this is crucial to this problem. Without this, the entire problem will not make sense.

$$f(x_{j+1}) > f(x_j) + f'(x_j)(x_{j+1} - x_j)$$

Since $f'(x_j)$ is strictly positive we have

$$\frac{f(x_{j+1})}{f'(x_j)} > \frac{f(x_j)}{f'(x_j)} + x_{j+1} - x_j$$

This then allows us to see

$$x_j - \frac{f(x_j)}{f'(x_j)} + \frac{f(x_{j+1})}{f'(x_j)} > x_{j+1} \tag{1}$$

Now use the definition of x_{j+1} which is

$$x_{j+1} = x_j - \frac{f(x_j)}{f'(x_j)}$$

in (1) to conclude

$$\frac{f(x_{j+1})}{f'(x_j)} > 0$$

and since $f'(x_j) > 0$ we have

$$f(x_{j+1}) > 0$$

This is absolutely gorgeous. It tells us a highly nontrivial conclusion about $x_0, x_1, x_2, \ldots, x_j, x_{j+1}, \ldots$ that all of these have the property that $f(x_j) > 0$. This is not at all immediate from the hypotheses of the problem but we just proved this.

Yes! Good. We needed to know that. This involves using the Taylor expansion and using $f''(x_j) > 0$. Let's assume that. This is what stops x_j from crossing too far down towards areas near a where $f(a) < 0$.

Let us formalise this deduction. There exists $a_0 > a$ such that $x_{j+1} \geq 0$ for all $j \geq 0$. We know now that $f(x_j) > 0$. Therefore

$$\inf_{j \geq 0} f(x_j) \geq 0$$

Since $f(a) < 0$ and f is continuously differentiable, we can find $a_0 > a$ such that $f(a) = 0$. We don't strictly need this here. We can conclude without it that

$$\{x_j\} \subset [a, b]$$

just from $f(x_j) > 0$. And now we have a subsequence x_j in a compact subset that is decreasing and therefore tends to a limit in $[a, b]$. The decrease is *also* a consequence of $f(x_j) > 0$ so this is the central result for the entire problem.

With lower bound, we're assured decreasing sequence with lower bound $x_j \geq a$ for all j. Then we know

$$\liminf x_j \geq a$$

17.2. **What Is Mathematical Substance?** This is no work of great mathematical genius as achieved by Harish-Chandra who took Hermann Weyl's representation theory of *compact Lie Groups* to profound depths and elucidated the entire structure of non-compact Lie groups beyond all imagination almost by his own efforts. This is far less sophisticated but I detect in this problem mathematical substance. And so I am moved to attempt to describe what *mathematical substance* is to my dear readers.

You see when I looked at the problem at first, I was not able to see the mathematical substance at all. The problem's assumptions are that $f \in C^2(\mathbf{R})$, and that $f'(x) > 0$ and $f''(x) > 0$ for all $x \in [a, b]$ where $a < 0 < b$. Then there is a definition:

$$x_{j+1} = x_j - \frac{f(x_j)}{f'(x_j)}$$

with $x_0 = b$. When I apply my ordinary intuition about what to expect from the sequence, there is no indication that it is a decreasing sequence at all. I can see, using *ordinary intuition*, that

$$x_1 = x_0 - \frac{f(x_0)}{f'(x_0)}$$

and that since $f(x_0) > 0$ and $f'(x_0) > 0$ that

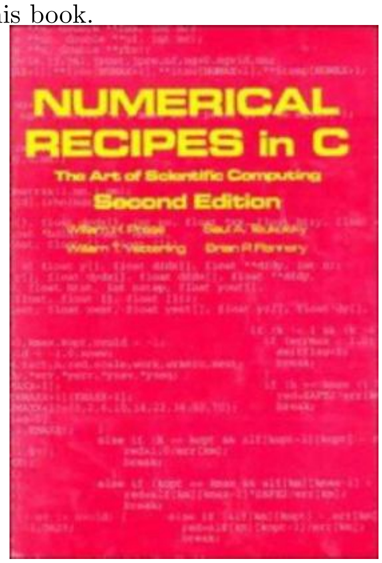
$$x_1 < x_0$$

That's where the ordinary intuition ends because I don't have any natural intution of whether $d_j = f(x_j)/f'(x_j)$ is going to be so *big* that $x_{j+1} = x_j - d_j$ will land in the *red zone* near $x = a$ and get us into hot soup because we would not have any *control* of lower bound for x_j. And then the whole problem's premises will fail and there is no theorem here.

What allows us to keep x_j bounded below, so it never reaches near $x = a$ area, where we know $f(a) < 0$ is the fine tuned use of two term Taylor expansion using the fact that $f' > 0$ and $f'' > 0$ on $[a, b]$. Which leads to the conclusion that $f(x_j) > 0$ for all $j = 0, 1, \dots$. The conclusion might seem simple as manipulation of Taylor expansion, but the conclusions defy ordinary intuition. I personally did not have any intuition that came from Taylor series *size considerations*. Maybe someone else has some intuition of this type but I have never seen such a situation at all and for me this is *mathematical substance* where features of mathematical results tell us something that ordinary intuition, even of an experienced man, does not see that there is some special property of the sequence. In this case mathematical substance leads to strong conclusions from premises such as $f' > 0$ and $f'' > 0$ whose ordinary meanings are deficient to see the outcome intuitively. I knew that $f' > 0$ is increasing; that $f'' > 0$ is convex, but my intuition was not sufficiently deep to see that $f(x_j) > 0$ for all $j \geq 0$ which is the crux of this problem. It's in fact a bit disturbing that the intuition is defied here as the situation has stronger restrictions than meets the eye. And that is the essence of mathematical substance. It's not ordinary *human intuition* but something different. Mathematical developments have this eerie quality that in some moments we are just surprised by what is going on in a situation. And that is why rigorous care is necessitated. Mathematical substance *hones* human intuition as this situation of this problem tests our ability to understand a situation where something quite nontrivial takes place, and the premises allow for a very vast set of functions f simultaneously.

18. Digression Into Autobiographical Matters

When I graduated from Princeton in 1995, I was a student of pure mathematics. I took many courses at Princeton; I was not particularly focused on analysis at all but my skills, I thought, were quite good. After all, I had taken in tenth grade the AP Calculus C exam in 1988 and I obtained a 5 in the exam. At Princeton I was not drawn to empirical sciences at all. I was rather airy and took many classes in Literature. I considered myself noble, and thought that all sorts of courses in physics and economics and other *applied* things that did not involve pure contemplation were not worthy of a noble soul. Anyway, after I graduated with a magna cum laude, I was forced to consider employment that would get me some income. I took a job at Lehman Brothers as a Finance quant. And suddenly I was confronted with the sorts of things that I did not do at all at Princeton. Everyone in Quantitative Finance were able to code in C and were familiar with statistics and numerical recipes. I spent time attempting to absorb some of this. You may or may not know this book.



I know this book intimately well. The C code is *horrible* but the content is invaluable. All sorts of existence theorems do not tell you how to get a number. Let's take a step back and return to a journey to my past. I am working on tenth floor of World Financial Center, in the trading floor. Rows of bond traders and sales folk surround me. The light is neon, and I never liked it. We were not close to the windows as those had fancier offices for senior staff of Lehman so they can bathe in their mover-and-shaker status in Lehman, and some even had portraits of their various illustrious forefathers on the walls of their offices. But Zulf was quite new from university, and was quite busy just learning about a new world. A world where if someone says, "Let f be a real valued function on $[a, b]$ with $f(a) < 0$ and $f(b) > 0$. Now find the x_0 such that $f(x_0) = 0$." They did not want a proof that $x_0 \in [a, b]$ exists. They want x_0 to 15 decimal places, and they want all sorts of assurances that your answer is right.

And that was the step towards my life in scientific concerns. Even then, I was not actually satisfied with things in books like *Numerical Recipes in C*. This problem is interesting because it's an interesting version of the Newton-Raphson root finding algorithm. I will be quite honest and say that I was wise and did not get involved in tampering with numerical algorithms. Other colleagues, more worldly and experience, told me to keep focus on substance of Finance, and use whatever is available. Lehman was not paying me to tamper with nitty gritty numerical recipes.

Now I am older, at 49. So now I can be quite amazed that there is anything at all of mathematical substance in these situations. My approach was to do as much hookey dookey to get some reasonable root and use the special professional technique that is very rigorous: cross your fingers, touch wood, and hope that the thing does not produce $x_0 = 2.586 \times 10^{54}$ as the right answer.

You see, my whole worldview *as an adult* was focused on complexities of nature, and capital and financial markets are part of *external nature*. I loved mathematics, but I was not concerned with mathematics any more. And the prejudice I absorbed from my senior colleagues who were distinguished: Andrew Morton was Cornell *Finance Ph.D.* and he was sitting with the Derivative traders and not with his research group. Kaushik Amin was there. Dev Joneja who was a Columbia Professor before. Jawahar Chirimar, Cornell Computer Science Ph.D. And many others. The general prejudice was a *qualified respect* for mathematics. Mathematics did not seem to provide us with sufficiently detailed understanding of situations, and seemed to be concerned with situations that are too difficult to check with conclusions that did not give numbers with 10 decimal figures. There were physicists too.

This problem, 11D, is remarkably familiar as a root finding scheme. I tried to learn a bit about approximation schemes but was too overwhelmed with actual need to do other things that were more urgent.

18.1. **Rates Of Convergence.** This is a challenging problem and not a simple deduction, as some amount of scribbling convinces me.

The idea that occurs to me is to let a_0 be the limit of x_j and just assume $f(a_0) = 0$. Then we want to examine the bound proven in part (i) with $f(a_0) = 0$.

$$|f(x_j) - f(a_0) - f'(a_0)(x_j - a_0)| \leq C|x_j - a|^2$$

Using $f(a_0) = 0$ we have

$$|f(x_j) - f'(a_0)(x_j - a_0)| \leq C|x_j - a_0|^2$$

I am still quite unclear what is happening here. I can get

$$0 < f(x_j) \leq C|x_j - a_0|^2 + f'(a_0)|x_j - a_0|$$

Now $f'(a_0)$ is just a positive constant, so

$$0 < f(x_j) \leq C_0|x_j - a_0|$$

This guarantees linear rate of convergence of $f(x_j)$ to zero. But for some reason, I feel that the rate is superlinear but don't see it.

19. Size Of My Surprise

I am *overwhelmingly* surprised by this problem. I have literally spent three decades with finding roots of various sorts of functions – without analytic control. A standard nontrivial example is in likelihood maximisation for statistical models. We put all manner of funky likeihood on data from all sorts of places because all scientists believe that unless God stops the universe and takes a long break to explain the nature of existence in slightly more detail than in Pentateuch and Bible and Quran, the best hope for mankind is *their own model.* So then , we, the scientists want a root for a complicated $f(x) = 0$. We don't expect all that much to come from mathematics at all here. We had given up that besides extremely totally worthless situations in nature, mathematicians will actually do anything for us.

Then I look at this iteration scheme and I am really *really* surprised. I am surprised that something works at all here to be proven. Let me tell you, the nature of getting things to work is a *fine art* which sometimes involves hitting the computer on the wall ritualistically to make it work.

20. My Assessment of Status Of Mathematicians In The World

In twentieth century and twenty first, mathematicians are not considered with extraordinary regard by vast masses of peoples of the world. They are considered similarly to Medieval Theologians, people who are talented in some esoteric discipline far removed from everything that matters to them. Even in science, people know something about *statistics* but beyond that mathematical issues are not considered all that relevant. This is not a matter of propaganda. It's just the way things work. People value things that they know and affect their lives directly. I did not know this in 1995 but I am able to assess the ways in people work and think. Now I happen to value Mathematics very highly because I went into Princeton expecting to be a professional mathematician, but instead I went into science, Finance at first.

You see, Four-Sphere Theory is an extremely serious scientific theory. Homogeneous geometry almost *never* occurs in empirical sciences, and I am absolutely completely certain that my Four-Sphere Theory is infinitely more correct science, faithful to Nature, than Einstein's, Schroedinger's and Dirac's views of Nature. But on most scientific issues, mathematics does not have a great role to play.

21. Cambridge Tripos 2001 IA 12D

Explains what it means for a function f to be Riemann integrable on $[a, b]$ and find a bounded function that is not Riemann integrable.

Show the following are true for continuous functions but not for general Riemann integrable functions.

(i) If $f : [a, b] \to \mathbf{R}$ and $f \geq 0$ and $\int_a^b f = 0$ then $f = 0$.

(ii) $\int_a^x f(s)ds$ is differentiable and $f'(x) = fracddx \int_a^x f(s)ds$

I was not sure what the definition was for Riemann integrable functions, and so I looked at Terence Tao's undergraduate book *Analysis I* Chapter 11.

A partition of $[a, b]$ is a finite set $a = x_0 < x_1 < \cdots < x_{N-1} < x_N = b$. We denote by $P = \{x_0, \ldots, x_N\}$ a partition.

We call a function $g : [a, b] \to \mathbf{R}$ a *piecewise constant* if there is a partition P such that g is *constant* on $[x_j, x_{j+1})$ for each $j = 0, \ldots, N - 1$.

We define the *upper integral* $UI(f, [a, b])$ and the *lower integral* $LI(f, [a, b])$ by

$$UI(f, [a, b]) = \inf_g \{ \sum_{j=0}^{N-1} g(x_j)(x_{j+1} - x_j) : g \geq f \mathrm{ispiecewiseconstant} \}$$

and

$$LI(f,[a,b]) = \sup_g \{ \sum_{j=0}^{N-1} g(x_j)(x_{j+1}-x_j) : g \leq f \text{ispiecewiseconstant} \}$$

The function f is called Riemann-integrable if

$$UI(f,[a,b]) = LI(f,[a,b])$$

and in that case we define the Riemann integral to be

$$\int_a^b f = UI(f,[a,b])$$

(i) For $f \geq 0$ on $[a,b]$ we have $UI(f,[a,b]) \leq 0$. Since f is Riemann integrable, $LI(f,[a,b]) = UI(f,[a,b]) \leq 0$. Now assume $f \neq 0$. Then there exists $t_0 \in [a,b]$ such that $f(t_0) < -C$ for some $C > 0$. Consider the partition $P_0 = \{x_0 = a, x_1 = t_0 - \epsilon, x_2 = t_0 + \epsilon, x_N = b\}$. For this partition, consider the function:

$$g(x) = \begin{cases} -C & x_1 \leq x < x_2 \\ 0 & \text{otherwise} \end{cases}$$

We have

$$\int_a^b g = -2\epsilon C$$

Now $f \leq g$ and we get

$$UI(f,[a,b]) \leq -2\epsilon C$$

This then contradicts assumption and proves the proposition. I used the assumption $f \geq 0$ and can replace f with $-f$ to obtain the statement of the problem.

(ii) Define

$$F(x) = \int_a^x f(s)ds$$

Then

$$\frac{F(x+h)-F(x)}{h} = \frac{1}{h}\int_x^{x+h} f(s)ds$$

We want to apply the Mean Value Theorem from a previous problem to F on $[x, x+h]$. This gives us a point $x < y < x+h$ satisfying

$$F'(y) = \frac{1}{h}(F(x+h)-F(x))$$

This was a bad direction for this problem as I realise since Mean Value Theorem assumes that F is differentiable and this problem requires of us that we *prove* exactly that.

Now, since I am not yet sure what the solution will be, I will digress into various matters that have nothing to do with the problem. This is an *elementary* problem in a way, but it is not at all easy. In fact, these issues preoccupied the minds of many nineteenth century mathematicians. Georg Bernhard Riemann – hallowed be his name – in 1854 defined the Riemann integral as a weakening of the integral defined by Augustin Louis Cauchy.

I don't have an altar to the great uneartly sage and profound genius Riemann, but that could change in the future. Between Riemann's definition of the Riemann integral and Henri Lebesgue's great breakthroughs of 1902-1905, all the greatest analysts of nineteenth century examined the properties of functions, continuous and discontinuous. I am not, therefore, particularly ashamed of requiring some time on the problem, for if I were doing this problem in 1870 I would be able to publish any findings in the best journals of all of Europe and it would be accepted with various tears of jo y and some would even swear that I was the Christ returned to lead people to the Kingdom of God. One has to keep perspective. These are not easy things.

I have a new path. I want to use the definition of continuity of f now. Then for any $x \in (a, b)$ and any $\epsilon > 0$ I want to get using continuity existence of $\delta > 0$ so that $|f(x) - f(y)| < \epsilon$ whenever $y \in [x - \delta, x + \delta]$ the *closed* interval which I can arrange by replacing δ with $\delta/2$. Then I want to consider points x_{max}, x_{min} in $[x - \delta, x + \delta]$ where f is maximum and minimum. I get

$$f(x_{min})(2\delta) \leq \int_{x-\delta}^{x+\delta} f(s)ds \leq f(x_{max})(2\delta)$$

by the definition of Riemann integral. This tells us

$$f(x_{min}) \leq \frac{1}{2\delta} \int_{x-\delta}^{x+\delta} f(s)ds \leq f(x_{max})$$

Now we use the fact that $|f(x_{max}) - f(x_{min})| < \epsilon$. Now we choose $h_j \to 0$ with $h_j < 2\delta$. Then I want to use compactness of $[f(x_{min}), f(x_{max})]$ to get a subsequence convergence for

$$\frac{1}{h_j} \int_{x-h_j}^{x+h_j} f(s)ds$$

So this is a better path.

We set aside compactness for a moment and consider the following. We have a δ so that $y \in [x-\delta, x+\delta]$ implies $|f(x) - f(y)| < \epsilon$. We could do the following. We could let x_0', x_0'' be the max and min points which are attained in $[x-\delta, x+\delta]$. Then we define a sequence of closed sets containing x with the following property. Set

$$I_0 = [x_0', x_0'']$$

Then

$$I_{j+1} = [x_{j+1}', x_{j+1}''] \subset I_j$$

and

$$x_j', x_j'' \notin I_{j+1}$$

At each step I_{j+1} is strictly smaller . Then we claim

$$\bigcap_j I_j = \{x\}$$

and we apply the argument bounding

$$\frac{1}{|x_j' - x_j''|} \int_{I_j} f(s)ds$$

above and below. This will give us

$$f(x) = \lim_{j\to\infty} \frac{1}{|x_j' - x_j''|} \int_{I_j} f(s)ds = F'(x)$$

This ought to get us close to the solution.

21.1. **Non-Continuous Examples.** Consider g defined on $[-1, 1]$ that is zero everywhere except at a point $g(0) = 1$. This is bounded and not continuous. It is Riemann integrable and the Riemann integral is zero. It is an example where both (i) and (ii) will fail. For (ii),

$$\frac{d}{dx} \int_{-1}^{x} g(0) = 0$$

but $g(0) = 1$.

21.2. **Further Thoughts.** To my knowledge Augustin Louis Cauchy introduced the definition of continuity that we use today, as well as the definition of the integral that Riemann generalised in 1854. There is a book by Thomas Hawkin, from 1970, on history of integration theory, that had some material on this. Cauchy, I would hazard was familiar with both (i) and (ii) in 1826. It would be interesting to check this. In the history of analysis these sorts of results for continuous functions were known around this time. But the resolution of analytic difficulties were not clear until Henri Lebesgue 1902-1905, which this problem does not touch.

22. Zulf Laughs At the Idiocy Of Bill Gates

Bill Gates, just now, 4:34pm December 25 2021, expressed that he does not understand how Cambridge can tolerate *these non-whites* doing various mathematics. That's quite funny, Billy Boy Old Chap. That's quite funny. Did you know that John Locke, the man who first wrote *A Letter Concerning Toleration* in 1689 was an Englishman? [3] It looks like your time at Lakeside Prep School, your highest level of education, did not prepare you for much. Long before you were born in 1955, Bill Gates, both Oxford and Cambridge were conferring doctorates to Bengali scholars. Perhaps you should use some of your stolena and robbed money of $131 billion into getting a proper high school education.

Here is someone from Punjab who completed a Bachelors at Cambridge and was knighted in 1923 [4]. Your ignorance and stupidity are quite boundless Billy Boy. Have you put in some money for a Nobel Prize in Ignorance and Stupidity yet?

23. On Sorts Of People

I am not white. I know many white people who are productive and good people who are respectful and tolerant and benevolent towards others. I know many *non-white* people who are also productive and good people who are respectful and tolerant towards others.

Then there are people like Bill Gates who are *not productive* charlatans who want strife and conflict between other *productive* people who are doing good work. Why does the world need these Bill Gates types? Why don't we just wipe him out? Why should someone who knows *absolutely no mathematics worth a dime* be able to produce strife in areas of science and mathematics where people of various ethnicities are kind and good and respectful toward each other? Isn't the world better of with using extreme lethal bloody military force to obliterate in the most gruesome manner these Bill Gates types?

24. Zulf And Ohio State Ross Program

I attended for two eight week summer sessions Ohio State University Arnold Ross program in 1988-1989. We did problem sets in number theory and combinatorics. And no one was ever satisfied with our rigour. We kept getting 'redo not rigorous enough' for all eight weeks. I am no stranger to 'redo not rigorous enough' from early youth and so I don't really worry too much any more, and do some problems and redo them. Only in rare cases is the perfect pitch attained.

25. My First Announcement That I Am The Copernicus of This Age

I track some developments of my own ideas and activities with more care than others. Looking over my records, I find the first public pronouncement to the world that I am the *Copernicus of this Age* to August 18 2018. For the convenience of the reader I recorded the entire announcement in my web log [2]. I succeeded in a project to deliberately replace all of physics above scale $\delta = 10^{-13}$ cm with a totally novel theory. That is the Four-Sphere Theory. It was consciously designed to replace Expansionary Cosmology in the beginning; then eventually also Relativity and Quantum Mechanics. I had begun consciously plotting a Scientific Revolution from 2003 in a way, but much more seriously from maybe 2006, and quite explicitly from 2008.

26. My Superiority To Bill Gates As a Man Is Vast

I am not generally actually in the habit of proclaiming my superiority over any man. I am from an aristocratic lineage of thousands of years from Indo-Persian ethnic background. My ancestors in 1500 BC were already engaged in the matters of Civilisation, and I have spent my life on intellectual pursuits *that were profoundly fruitful*. They were not evident in lists of *publications* in *Science* and *Nature* but they have led to success in Four-Sphere Theory, in establishing *Universal* Human Moral Nature that strongly refute the theories and considerations of Immanuel Kant and Friedrich Nietzsche.

Bill Gates is a third rate charlatan who managed to sell third rate products from a peasant stock English-Scottish lineage who has gained some success. His father was a lawyer and his grandfather was a furniture merchant in provincial northwest far from Cosmopolitan centers in Northeast and California and Chicago. He was many steps removed from London and Paris and Berlin culture that is the center of urban Civilisation of the modern world.

But it is important that I proclaim clearly that I am vastly superior to Bill Gates because this is not a matter of ethnicity but of Character. He is an illiterate hick who is a career murderer and his lifelong dream still into twenty first century is to be the literal tyrant of human race while the world had overcome these with establishment of the United Nations after the great war. My superiority of Bill Gates begins with my values, which are humanistic from early age while he is unable to even comprehend a single novel of Thomas Mann without assistance. His probable assessment of Hans Castorp would be that "He's white. He's white".

27. The Simpson Method For Finding Roots

I wrote some letters to demand that the world give credit to Thomas Simpson for the iterative method

$$x_{n+1} = x_n - \frac{f(x_n)}{f'(x_n)}$$

that has been called the *Newton-Raphson* method for finding solutions to $f(x) = 0$.

I am taking a distance from Cambridge Tripos 2001 IA Problem 11D and turning my attention to an engineering paper [7]. I am separating this from the problem because I am interested in understanding how engineers have seen the problem. Akram-Ann, in their note tell us why engineers consider the rate of convergence of the *Simpson iteration scheme* which is called *Newton-Raphson*, is quadratic. The key issue from the engineering view is this. Let

$$e_n = x_n - a_0$$

where a_0 satisfies $f(a_0) = 0$. Then

$$e_{n+1} \le C e_n^2$$

That is the point. I suspect that the Tripos problem 11D was hoping that the answer would be this. The problem ought to be much more clear about what they are looking for. You see, I am mathematically trained, and for me, this particular terminology, where $e_{n+1} \le Ce_n^2$ as *quadratically convergent* is totally esoteric. For people in engineering this might be common but I have never heard of this terminology before. I think the Cambridge Tripos writers ought to be much more clear in what exactly they are looking for.

But let's examine the engineering logic – which I re-emphasize is not my work and I am taking it from [7] who seem to consider this well-known in the field.

The key is to set $f(a_0) = 0$ and take a Taylor expansion as follows:

$$0 = f'(x_n)(a_0 - x_{n+1}) + \frac{f''(\xi)}{2}(a_0 - x_n)^2$$

and then from this the conclusion for quadratic convergence will follow. I see exactly what my confusion had been in 11D where I did not come to the right conclusion. I had no idea what sort of convergence rate is *quadratic* at all. It's not a notion that is defined in any part of ordinary real analysis. None of the courses or books in real analysis has ever mentioned this notion of rate of convergence at all. I did not think of this particular notion of convergence and so did not get the expected answer.

28. Some Direction To Mathematicians On Root Finding Algorithms

I am not an expert at all on this topic, but I do sense there is a disjunction between mathematicians and electrical engineers who have been investigating rates of convergence for root-finding algorithms. I found a Berkeley doctoral dissertation in electrical engineering from 1970 by Arthur Ira Cohen entitled *On the Convergence And Optimality Properties of Root Finding and Optimization Algorithms* [8]. There is probably some incoherence between Electrical Engineering and Mathematics regarding terminology and results that need to be rationalised. The 2001 Cambridge Tripos IA Problem 11D ought to provide a precise definition of the sort of convergence rate criterion that is sought because Electrical Engineers have established terminology that are not standard in Mathematics as far as I know. For me, *quadratic convergence* is:

$$|x_n - a_0| \le Cn^{-2}$$

And not

$$|x_{n+1} - a_0| \le C|x_n - a_0|^2$$

I have never ever seen anything like the latter notion of convergence rate in Mathematics and would not invent the terminology *quadratic convergence* for it.

29. Cambridge Tripos 2001 IIA Problem 10A

For $f, g \in C([a,b])$ define

$$d_1(f,g) = \sup_{x\in[a,b]} |f(x) - g(x)|$$

and

$$d_2(f,g) = (\int_a^b |f-g|^2)^{1/2}$$

Show that both d_1 and d_2 are metrics. Is $C([a,b])$ complete with respect to d_1?

Show that in $C([-1,1])$

$$h(x) = \begin{cases} 0 & x < 0 \\ nx & 0 < x < 1/n \\ 1 & 1/n \le x \le 1 \end{cases}$$

tends to a limit that is not in $C([-1,1])$ and so the space is not complete in d_2.

This is a good exercise and fairly standard. For metric we need to show the triangle inequality, symmetry and $d(f,g) = 0$ iff $f = g$.

Symmetry for both d_1, d_2 is easy since $|f(x) - g(x)| = |g(x) - f(x)|$ for every point $x \in [a,b]$.

For triangle inequality we start with

$$|g(x) - f(x)| = |g(x) - h(x) + h(x) - f(x)| \le |g(x) - h(x)| + |h(x) - f(x)|$$

For d_1 we take supremum of both sides, and note that

$$\sup(|g(x) - h(x)| + |h(x) - f(x)|) \le \sup_x |g(x) - h(x)| + \sup_x |h(x) - f(x)|$$

For d_2 we have

$$|g(x) - f(x)|^2 \le |g(x) - h(x)|^2 + |h(x) - f(x)|^2 + 2|g(x) - h(x)||h(x) - f(x)|$$

Then we use Cauchy-Schwarz to get the triangle inequality for d_2.

Finally for identity, suppose h satisfies

$$\sup_x |h(x)| = 0$$

then $h = 0$ is clear. For d_2, if

$$\int_a^b |h|^2 = 0$$

Suppose $h \ne 0$. Now h is continuous, so if $h(x_0) > 0$ then there is a small interval $I = (x_0 - \delta, x_0 + \delta)$ with $h(y) > 0$ for all $y \in I$. Then we just note

$$\int_I |h|^2 > 0$$

which is a contradiction, therefore $h = 0$.

Now let's see. In neither metric is $C([-1,1])$ complete. For d_1 we can take 'tents' at 0 for $[-1,1]$ which are all bounded and are triangles with base $[-1/n, 1/n]$. The sup norm is 1 for all of these but the limit is zero except for 1 at zero, discontinuous.

Now for the example of the problem for d_2 we note that there is pointwise convergence to the discontinuous function that is 0 for $x \le 0$ and 1 for $x > 0$. We need to prove that the convergence is in the d_2 metric too.

The point of the problem is that $C[a,b]$ with these metrics do not contains all sorts of limits. Now the man who really thought about these was *Frigyes Riesz*, and he named the spaces that contain the limits after Henri Lebegue. This was a great moment in history of Analysis, for then *Lebesgue measurable functions* with these metrics were seen as the completion of $C([a,b])$ to Banach space $L^\infty([a,b])$ and Hilbert space $L^2([a,b])$, and these functions became the natural inheritors of functions that matter and which can be approximated by continous functions. It's a remarkable journey that got to this because Man had no business with the strange and variegated functions that arise and yet they are parts of nature. The very fundamental notions of our intuition was challenged by the gods, and we could not speak, our eyes failed, we were neither living nor dead, we knew nothing, looking into the heart of light, the silence. You think this is an exaggeration? Well what do you think a function is in nature? Is it the scribble you drew on the chalk-board to deceive the students into a false sense of knowledge?

This question is broad in a sense but it is the fundamental question that has been on my mind for some years. You see, the universe is an four-sphere with eternal totally constant radius. The radius is $R = 3075.69$ Mpc roughly. This is the reason for the cosmological constant having constant positive measured value $\Lambda = 1.11 \times 10^{52} m^{-2}$. I know that Perlmutter and Schmidt and Reiss received the Nobel Prize for their *accelerating expansion* theory in 2011 but this is totally wrong. The universe is static, and not expanding at all. Entire expansionary cosmology is wrong, and Permutter-Schmidt-Reiss did not prove any accelerating expansion at all. The redshift slope is a geometric artifact, and is not any evidence of any expansion at all. The truth is that there was never any big bang, that the universe has existed for infinite time in the past, and the spatial geometry is an exact homogenous four-sphere of fixed radius. This is the absolute truth, and all manner of ra ra and Nobel Prizes will do nothing to change this eternal truth.

The correct science is not expansionary cosmology and quantum field theory and relativity. These are wrong. The correct science is my Four-Sphere Theory and nothing other than my Four-Sphere Theory. So the functions and fields on Four-Sphere are the fundamental objects of nature.

30. I Have No Personal Animosity With Perlmutter-Schmidt-Reiss

I have no personal animosity at all with Permutter-Schmidt-Reiss. As a matter of fact, I do have bitter personal animosity toward Bill Gates but not towards scientists who have convictions regarding theories of nature that are opposed to mine. But I do not care if 50 Nobel Prize winning physicists and chemists and astronomers oppose me either. I am right and they are all wrong. I am a scientist and nature is the arbiter and not Nobel Prize winning scientists. Nature favours me, not them. Nature's favour is in measurements matching theory. I spent many years of labour just checking my model versus measurements. You can find some of my work here [**?**].

31. How To See I Am Right Versus Hubble And Lemaitre

Let us put aside contemporary observational cosmological work and return to the roots of expansionary cosmology. Let's consider Zulf versus Hubble and Lemaitre, the founders of expansionary cosmology.

I will simplify matters to make the issues clear. Edwin Hubble published in 1929 a relation between distant galaxies and their redshifts [**?**]. Subsequently cosmologists are compelled that it is a *Doppler effect* and then Alexander Friedmann's 1922 work is accepted where he derives the Friedmann-Lemaitre equations.

What's the basis of all this? I *predict* a redshift value in my work in a static four-sphere cosmos that is not due to *any dynamical phenomena*. My explanation is that the universe is a four-sphere and so we have to use frequency-wavelength relations on four-sphere which are not the same as the ones in flat $\mathbf{R}^3$. They differ from $\lambda = c/\nu$ in such a way that, if we consider the wavelengths of distant objects and use the wrong formula

$$\lambda = c/\nu$$

then we will find that λ grows with distance.

My predictions are quite accurate with measured redshift. A theory of nature where 'space has intrinsic expansion dynamically' which is the basis of expansionary cosmology is not as parsimonious as a static cosmos with a fixed curvature where Man has been pretending that there are all manner of funky expansions. My science is better than Hubble-Lemaitre-Friedmann because why should anyone accept a funky expansion of space when there are much simpler geometrical explanations? They shouldn't. The whole expansionary cosmology is delusional.

32. Explanation of Four Spatial Dimensions In My Four-Sphere Theory

My theory has fundamental geometry $S^4(R) \times \mathbf{R}$, and it is exactly identical to usual mathematical notion. The four-sphere is *metric round four-sphere*. There is no relation whatever between time and space. There is no relativity and the theory is classical. I reject strongly special relativity. Constant speed of light is not a problem. It's simply the wave speed in the fundamental single law, which is my S4 Electromagnetic law (not Maxwell or Schroedinger or Dirac law). Recall that the *finite propagation speed* of waves is defined for ordinary wave equations. And it is just a matter of putting a constant in it. For example the one-dimensional wave equation on the line is

$$(\partial_t^2 - \partial_x^2)u(t,x) = 0$$

with whatever initial conditions you want. This has unit propagation speed. Then you put in:

$$(\frac{1}{c^2}\partial_t^2 - \partial_x^2)u(t,x) = 0$$

and suddenly the finite propagation speed is c. The intuitive explanation for why you have to deform time and mass in order to ensure that speed of light is constant is hocus. Speed of light is the constant in the fundamental law. What you are able to observe is a derived thing. My theory is a classical field theory where the fundamental law is a *classical wave equation* on *spinor fields* on the four-sphere. I don't care about whether you have trouble comparing the speed of various bullets and pelicans and speed of light. The speed of bullets and pelicans are highly *derived quantities*. The speed of light constant is just a constant *within the fundamental law*. I think the obsession with trying to compare speed of bullets and pelicans with speed of light and treating them as similar sorts of things was not a good move for physics.

Now let me return to my theory. The four spatial dimensions in my theory are all purely electromagnetic and they are not the observed three dimensions plus an extra dimension of the same type. In my theory the physical universe, everything that is observed, is emergent, and we do not have automatic perception of a fourth dimension. When motion occurs, it is driven by S4 Electromagnetic law operating globally on $S^4(R)$ and some of the effects include speeding bullets and pelicans hitting airplane cockpit windows and so on.

33. My Viewpoint About Acceptance Of Four-Sphere Theory

You see, I am a serious student of Paul K. Feyerabend and Thomas Kuhn. I am also a very serious scientist although I have no abilities in experiments. I am *sure* that I am right and Four-Sphere Theory matches *nature*. I am so sure that I am right against everyone in history of science that I don't worry about *any acceptance at all*. Why? Well, I am not in great health and not in a good situation. My legitimate income has been sabotaged by malevolent racial murderer Bill Gates. My mother is bedridden and will die sometime soon as her health degenerates and I am more worried about survival than about various accolades. But I will say that at $t \to \infty$ all of humanity will strongly adopt Four-Sphere Theory. I am not in any rush for acceptance in my lifetime. This is because science proceeds without method, as Paul K. Feyerabend discovered. And one cannot coerce acceptance. People have Liberty to believe whatever they want. But in the long run, in a thousand years, I am guaranteed acceptance. Why should I then beg and plead people to accept what is truth? I don't really care about moving scientific consensus. Scientific consensus is not truth; it's scientific consensus. It's a social affair. So I don't worry about it. What is true is that *those scientists who work on other theories* will become irrelevant in a thousand years as people who were stuck on the wrong side. I won't evaluate them but people in the future will, and consider them to have erred etc. Just as I don't take on stupid projects of herding cats, I do not take on ambitions of moving consensus. That sort of thing is not in my spectrum of ability. However producing good science is, and I have the best science of this age for phenomena above $\delta = 10^{-13}$ cm in the history of human race. Of that I am quite certain. It is not up to me who accepts it and I am not going to waste my time on pretending that I can control masses of people accepting what they don't want to.

34. Why Don't People Email Me Or Contact Me At All?

This is a very interesting issue. On one hand Bill Gates used all manner of racial and US War Power and other blockades to kill me and starve me and so on. That's one part of the answer. Another is that I am the Scientific Revolutionary. I am challenging a Scientific Paradigm entrenched strongly from about 1920s, when Relativity, Quantum Field Theory, and Expansionary Cosmology were established. My opponents, so to speak, control the Science of this Age, established science. They have elected to just remain silent and ignore my challenge.

So what happens then? Well in the $t \to \infty$ limit I will win without doubts because my Science is better than theirs. In the short term, they will try to retain their entrenchment because people don't want upheavals in their careers.

I am not irrational. I don't expect all manner of accolades for being right. Scientific Revolutionaries are never really liked all that much by entrenched establishment. That's fine. I have *Human Moral Nature* that I work on. I am looking at mathematical issues. What should I resent? The world works the way it does, and is not optimised for my convenience. I am an immortal genius whose Scientific Revolution will sweep away all of the established *macroscopic* Science of this Age.

It won't do it quickly, but it will in due time. I don't have patience to watch the transformation. Most of this will happen after I die.

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