STANFORD SPRING 2018 ANALYSIS QUAL

ZULFIKAR MOINUDDIN AHMED

1. Problem I.2

Let S be a closed subspace of $C[0,1]$ with sup norm. Suppose $f \in S$ implies f is continuously differentiable. Prove that S is finite dimensional.

Here the issue is that the embedding map $j : C^1[0,1] \to C[0,1]$ in a compact operator. Let's assume this known first.

The assumption of the problem is equivalent to $S \subset j(C^1[0,1])$. Let

$$S_1 = \{x \in S : \|x\| \leq 1\}$$

Then $j^{-1}(S_1)$ is bounded in the C^1 norm, and therefore $\bar{S}_1$ the closure is compact. We construct the sequence x_j with $\|x_j\| = 1$ with $x_{j+1} \notin M_j = \operatorname{span}(x_1, \ldots, x_j)$ and $d(x_{j+1}, M_j) \geq 1/2$. This sequence would lead to no convergence unless S is finite dimensional.

Now we prove that j is compact. We have natural norm $\|x\|_{C^1} = \|x\|_\infty + \|\partial x\|_\infty$, so obviously $\|jx\|_\infty \leq \|x\|_{C^1}$ and j is bounded.

Now suppose $\|x_n\|_{C^1} \leq 1$ for all $n \geq 1$. We want to prove that x_n has a subsequence that converges. We could do the following. Suppose $\epsilon > 0$. Then we cover $[0,1]$ with ϵ-balls and choose a finite subcover. And we have a finite number of $I_1, \ldots, I_N$ with centers $t_1, dots, t_N$ so that for all $t \in I_j$ satisfy $|t - t_j| < \epsilon$.

Then we examine the Taylor expansion of x_n on I_j. The substance here is that we have to show sequential compactness of $(x_n(t_1), \ldots, x_n(t_N))$ and then use the derivative bound to cover the rest of I_j. But these are isomorphic to a set of vectors in $\mathbf{R}^N$ and so we have sequential compactness from Heine-Borel theorem for them. Now we choose the subsequence and use Taylor expansion to get actual convergence in sup norm of the functions themselves.

I will show you how nice this situation is. Suppose you let $y_n = (x_n(t_1), \ldots, x_n(t_N))$ be the *subsequence that converges* in $\mathbf{R}^N$. The major worry is gone because you don't need to pick any subsequence any more and you have that. Now it's Cauchy, or as I like to say these days, Bolzano. Now you can find some M such that $n, m \geq M$ produces

$$\|y_n - y_m\|_{\mathbf{R}^N} < \epsilon$$

These are all 'pegged' to the C^1 functions x_n, x_m at $(t_1, \ldots, t_N)$. Now we have for any $t \in [0,1]$, it falls in I_q for some $1 \leq q \leq N$ and so

$$|x_n(t) - x_m(t)| = |x_n(t_q) + x_n'(t_q)(t - t_q) - x_m(t_q) + x_m'(t_q)(t - t_q) + O(\epsilon)| \leq \epsilon + \sup \|x_k'\|(2\epsilon) + O(\epsilon)$$

This is using the boundedness of $\|x_k\|_{C^1}$ and the lengths of I_q all at most 2ϵ and the rest is higher order terms that are easy to control. The heart was the choice of the subsequence in $\mathbf{R}^N$ and I used the Taylor expansion of $C^1[0,1]$ here to get

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control of all points in $t \in [0,1]$ by the fact that linear terms are controlled by the size of I_q nicely.

2. Stanford Assessed My Physics Genius To Have Exceeded Albert Einstein

I am most gratified that Stanford did an assessment and found that my physics genius to have exceeded Einstein. I would like to remind the world that Einstein was as truly remarkable genius in physics and his work was actually crucial to my breakthroughs in producing the final laws of Nature. I will not go into much detail but it was the form of the General Relativity law that gave me the first intuitions regarding four-sphere theory. When I looked at it carefully in a loft in Williamsburg Brooklyn, I saw that it was the Ricci curvature of a hypersurface embedded in four-sphere, whose formula I knew from the book of Shing-Tung Yau and Richard Schoen, *Lectures on Differential Geometry*, once I ignored the Special Relativity indefinite signature metric. Thus even though my physics theory was quite different from his – I strongly went against Special Relativity – what he had discovered was a different sort of profound truth about Nature, although that is not what he theorised.

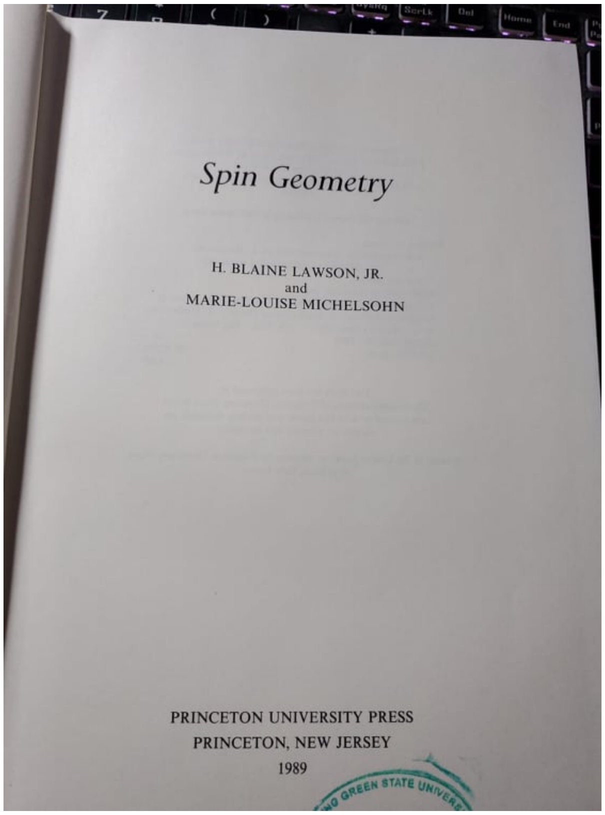
What he had discovered was a remarkable fact that there is an unseen four-sphere structure of existence in which the physical universe is *constrained to be embedded* at all time, and this is crucial to features of Nature.

Einstein was rather eager to avoid a vast fourth spatial dimension as was the rest of the physics community for a long time. His Special Relativity theory was based on (a) firm conviction that the Michelson-Morley conclusions from 1880s were definitive, and (b) the Maxwell laws were exact truth. Henri Poincare introduced Special Relativity invariances slightly before him actually [1]. Special Relativity is wrong in the actual universe. They arise from a strong faith that space-time empty of matter is actually flat, and space contains within it symmetries that keep the wave equation invariant.

I have repeatedly emphasized that this is not the case. Einstein resisted strongly Lemaitre's theory of a expanding cosmos and proposed an S^3 model of space. His introduction of $\Lambda \geq 0$ was more important in the end for four-spere theory for this is what led me to the conviction that four-sphere theory would be true with absolute space and absolute time.

As you know, I had interpreted Λ as the curvature of a static eternal four-sphere. Then I worked quite a bit to examine whether atomic clocks in the early 1970s were accurate enough to have reliable evidence for time deformations. I strongly rejected both time deformation and space deformations are invalid. There are many reasons, the strongest of which was that it was clear to me that Planck's constant h was related to Λ as $\Lambda = Ch^2$ for some universal constant C and it made no sense in my four-sphere theory that space can deform, and the conclusions of Michelson-Morley are right for three dimensional luminiferous aether but the surface of a four-sphere was reasonable substitute.

Now I was also influenced by what I knew in mathematics all these years away from Mathematics. When I had gone to Columbia University for graduate school, John Morgan was kind to me and asked me to master Lawson and Michelsohn's *Spin Geometry*.



This book is truly marvelous in a unified treatment of the Atiyah-Singer-Dirac operator and I have followed their appeal and called D acting on spinor fields of a four-sphere Atiyah-Singer-Dirac operator when I formulated my S4 Electromagnetic Law, which is inspired by the original wave equation written down for the vibrating string by d'Alembert from 1742.

What occurred to me over the years is that within physics, the central intuition that has been retained is the *harmonic oscillator* as a ubiquitous feature of Nature. I was fortunate that I has studied the geometry of four-manifolds where the pure tones of the four-sphere were on my mind and the homogeneous spacing of energy levels observed had then a clear alternative model, that the entire universe, seen and unseen behaved as a giant drum. And this turned out to have stronger power for fundamental physics theory after over a decade of work.

3. Another Perspective of History Of Physics

This is such an important topic, fundamental physics that it is worthwhile examining it from multiple perspectives to understand. I have explored these for years. You see, the notion of flat empty space was not invented by Einstein. He inherited the notion from all of Western and Eastern Civilisation. Euclid is truly the source of this particular notion. Einstein was quite adventurous in examining alternatives.

It was really mathematicians, first Gauss and Lobachevsky and then Georg Bernhard Riemann who has been quite concerned with Euclid's postulates and basis of geometry. In 1854 Bernhard Riemann put forward the bold proposal of study of metric geometry in arbitrary dimensions with curvature measured using the Riemann curvature tensor. Albert Einstein was a physicist, and there was no reason for him to abandon the total intellectual heritage of all of Western Civilisation for *empty space*. That was left for me, Zulfikar Moinuddin Ahmed, in the summer of 2008, more than a century after Einstein's Special Relativity was published in 1905.

Einstein had not reason to abandon Euclidean geometry of *empty matterless space* but I did. I had reason because I already knew that $\Lambda > 0$ was measured, and $\Lambda = 1.11 \times 10^{-52} m^{-2}$ and I was mathematically trained and did not pursue physics at Princeton, 1991-1995. For me the natural hypothesis *was exactly* that the actual universe had *positively curved compact geometry* and there was never any flat Euclidean space anywhere in Nature at all except in human imagination.

Some people take my theory as a challenge to *Einstein*. I have no animosity towards him. He was a saintly genius and I love him. But Science is not an enterprise about individual geniuses. It is human collective best knowledge of Nature. I do Science with care for truth. Four-Sphere Theory is eternal truth. You will see this in the centuries that follow that is is far stronger than all the theories currently established.

4. Personal Interpretation Of History

You see, these seem small to some people but my natural senses give me some insight. Albert Einstein was born in 1879, and James Clerk Maxwell's great work of unified electromagnetic field and his equations were done before his birth. He inherited a tradition of physics where there was no reason to doubt the strongest ground, that Maxwell's equations were true about nature. Einstein did not challenge this and instead built on this ground. Special Relativity is essentially shoring up the faith in flat empty space. Michelson-Morley experiments disrupted the peace of physics and Einstein rescued physics by relativity because without aether how will anyone think about waves?

I was in a very very different world in 2008. In my world, the positivity of Λ the cosmological constant was well-established measurement, so for me it was very easy to just look at $\Lambda > 0$ and read Marc Kac's paper on "Can One Hear The Shape Of A Drum" and instantaneously consider a closed homogeneous geometry responsible for quantisation of energy. In 1905 the spectral theorem for compact self-adjoint operators was not even part of mathematicians' intuition, and it was bread and butter mathematics from undergraduate years for me. Why would Einstein have hypothesized that empty space is flat or that there is a large purely electromagnetic spatial dimension in the universe that is not directly perceived? Those things do not seem sane to a gentleman born in Europe in 1879. They were not even radical enough in some ways in 2008.

Spectral theorem for compact self-adjoint operators I knew from Peter Sarnak's functional analysis course *junior year* around 1992-3 at Princeton. I did the whole proof for homework problems so I had a firm feeling for it. In one of the recent Stanford exam problems I even give a simpler proof. Sixteen years had passed before I began thinking about four-sphere theory after my first introduction, time in which I had just absorbed it as basic truth. On the other hand Mark Kac's very famous article 'Can One Hear The Shape Of A Drum?" appeared in 1966, some seven years before I was even born. Professor Peter Sarnak emphasized the spectrum of various compact surfaces in his course, and he is a fabulous teacher and lecturer and he made a lasting impression. For me compact spatial geometry for the actual universe was the most natural immediate hypothesis in 2008. At first I was more concerned with Einstein and Relativity but instead challenging *Expansion* and explanation of quantisation. I thought perhaps General Relativity will fit in with my ideas but some years later I had to reject Special Relativity. I took the strong view that theories of physics that deform time or space at all are not parsimonious and are surely wrong.

5. My Experiences In Biotech And Finance Gave Me Experience In Scientific Mindset

I worked between 1995-2008 in Finance and Biotech and it was far from mathematics and also far from *theoretical physics*. The mindset of a professional scientist is very different from even theoretical physicists. There *matching measurements* is the be-all and end-all of all theories, whether *phenomenological* or fundamental. In finance and biosciences there is nothing else to stand on. Theories in quantitative finance are not based on fundamental principles and mathematical theorems. They are based on thousands of hours of fine tuning confronting vast amounts of data. In biology too, the overwhelming complexity dominates and no purely theoretical considerations are trustworthy.

This experience was crucial to my development as a man generally, and also to my viewpoint about four-sphere theory. Although it is pristine and beautiful mathematically, for we have things like $L^2(SO(5)/SO(4))$ as the space of functions on the four-sphere, something that no scientist outside extremely theoretical ones will ever consider seriously, most of my effort from 2008-2018 involved attempting to produce some calibrations and predict the redshift slope in static cosmological model, and do other things that will seem rather unsophisticated to the theoretical physicist like reproduce Earth-Sun gravity using Van der Waals forces with care in numerical values. But that is the heart of science to me, the *calibration of models*. This discipline is from Finance. I had the good fortune of working with Andrew Morton, Kaushik Amin, Dev Joneja, Anthony Lazanas, Douglas Macbeth, and Jawahar Chirimar at Lehman Brothers, and they are top notch quants, and quants know how to deal with empirical calibration even better than chemists and lab physicists. Why? Because finance data comes with *no theory at all*. It's just mysteriously explosively messy. And so I did the right thing and put aside theoretical issues and calibrated the model to Nature. And that is why I succeeded. No theory can win against one that fits measurements better and is more parsimonious. You cannot really appreciate that is the truth until you are in environments like Biotech and Finance.

6. Problem I.3

Before I go into this problem, I want to return to the story of Erik Ivar Fredholm again, because there is something especially beautiful about what he did for integral equations that I don't yet understand. Erik Ivar Fredholm was a student of Gosta Mittag-Leffler (doctorate 1898), and his work on solving integral equations moved the great David Hilbert to literally abandon algebra and geometry and devote years of his life into analysis, spawning Hilbert space theory, theory of linear operators on Hilbert spaces, the spectral theory of compact self-adjoint operators. Of course it was Erhard Schmidt who elucidated the geometry of Hilbert spaces. This is very close to my heart because this is really the heart of what allowed me to have complete trust that it is the *compact homogeneous geometry* of the entire universe, or *existence* if you like, that is responsible for this marvelous feature that energy is quantised *evenly* in units of h in Nature. The story began with Erik Ivar Fredholm for the features of mathematics for integral equations.

I will digress a bit and take you on a journey back to my own past. I am at Princeton, and I am trying to fathom the Peter-Weyl theorem. Professor Peter Sarnak tells us a magnificent story of Erik Ivar Fredholm and points out that the Laplacian on a compact manifold has a discrete spectrum but it is the compact self-adjoint *resolvent* operators that is the way to get clear eigenspaces for the Laplacian itself. I was spending a lot of time in Fine Hall Library, in the basement level. I was a bit overwhelmed by the novelty of all this. I felt that I had left Kansas already. Well, fine, I am originally from Bengal, and was never in Kansas at all, so it's a manner of speech. I mean that I had entered some part of serious mathematics, where people do things that transform the intellectual life of the entire world. I worked hard for the course and managed to get an A-. I was not even close to the acumen of some of the others in the course, Terry Tao was there and was sharp and erudite already, and Steven Gubser. There were some of the brightest minds in physics and mathematics there. I was not unhappy.

And so let us return to Erik Ivar Fredholm. His work was on solving the equations

$$\varphi(s) = f(s) + \lambda \int_a^b K(s,t) f(t) dt$$

by considering matrices and determinants. He found a way to solve these sorts of equations using finite matrix approximations of the equation and taking a limit. There was no L^2 space at all at this point as Frigyes Riesz had not invented them yet, and Lebesgue did not do anything yet as his great works were between 1902-1905.

Now I will say that I have never actually solved these sorts of integral equations at all in my life. So it was not the integral equations that interest me; rather it is the fact that the spectral theorem for compact self-adjoint operators grew out from the ideas in Fredholm's work of 1900, and the concepts were so powerful that differential operators, quite unbounded, were studied with compact resolvents for eigenfunctions of Laplacian on all manner of smooth closed manifolds ever since.

Let's at least write the problem down before we continue the digression which is more interesting to me than the solution of the integral equations.

6.1. **Statement of Problem I.3.** Let $K \in L^2(\mathbf{R}^2)$. Define $T \in L(L^2(\mathbf{R}))$ by

$$Tf(x) = \int K(x,y) f(y) dy$$

(a) Show that T is bounded and compact.

6.2. **Return To Lengthy Digression.** Jens Lenström has a very nice thesis that addresses some issues of history of functional analysis from 2008 [2]. He points out that in the early period 1900-1930 linear algebra did not have enough development to form the basis for functional analysis. This is an important insight, as for me, linear algebra had been part of my life from even before freshman year at Princeton in 1991 and it's hard to imagine a world where linear algebra is not basic repertoire for mathematics.

7. It Is Always Folly To Underestimate Mysteries of Nature

In Mathematics, there is a valid path in surveying the world of pure and beautiful ideas, and in these you can find solace, and you can build your home in some things that are central to Mathematics. In Science, this is pure folly. Even in theoretical physics, for Nature is a harsh mistress, and will without any doubt punish those who takes her for granted. And this is not a simple thing. Those who are genuine and serious in their pursuit of Science are fools if they do not respect the ineluctable mystery of Nature, and are not awed by her frightening power to befuddle the wisest of sages among Men. I am not particularly impressed by pure theorists in Science as I approach 50 this year. Science is most definitely not Mathematics. The question of their relationship has not even been addressed seriously enough to fool a freshman at Princeton. These are deep issues with unsatisfactory answers.

When I was young, in high school 1987-1991, there were so many thick textbooks filled with exact models that had a sense of finality to them. Especially in physics and chemistry there is a sense that all possible things you can imagine have been found with exact precision and mathematical formulae. But over the decades, I matured in Science, and now I am challenging the explanation of the redshift-distance slope as dynamical expansion for more than a decade. Mathematics is in a sense a vast subject in the productive activity, and there are perhaps some million titles in libraries and bookstores and in private possession. When we say "Nature is described by Mathematical Laws", we are not saying anything substantial at all *until we specify exactly which mathematical objects and their properties and relations actually matter to Nature.* Not all of Mathematics will matter to Science, and the question has never been seriously addressed at all in my view. The significance of this *intersection* is completely opaque and mysterious, certainly to me, but I believe to all human race as well. I think this is a deep and important issue that will be resolved over centuries. There are a cacophany of opinions on these issues today and not many solid and clear answers that can be reasonable or right. Partly this is due to the fact that Nature has vast complexity and there are no guarantees that the specific issues that have purely mathematical interest will ever give us ability to penetrate the complexity. Partly it's because purely Mathematical interest cannot be based on the mysteries of *external Nature* and will drive to issues that have subtleties of an entirely different source. You would need to live in a world dreamt up by Jorge Luis Borges to appreciate the difficulties involved in having clear and solid answers that all people of Earth can appreciate one day. That's an entirely different world than the one in which we live today.

8. Let's Be Serious Bill Gates

Bill Gates contends that *non-white people are not capable* of various sorts of sophisticated intellectual achievements. Never mind that Ramayana (Scholars' estimates for the earliest stage of the text range from the 7th to 4th centuries BCE) and Mahabharata (written down in 400 BC) were completely works of non-whites and occurred before *England had any literacy at all.* So clearly this proposition of Bill Gates is not viable. But more importantly, Bill Gates was not *capable of graduating from Harvard at all* while I graduated from Princeton with magna cum laude in 1995. It does not look to me like European ancestry gives anyone ability to graduate from university let alone be in any position to question abilities of a man who successfully challenged all the established theories of physics for a century.

Why doesn't Bill Gates prove himself. I have not been impressed by his intellectual capacities. Please, take some of the Stanford Ph.D. Quals and show that your ethnicity alone gave you superior intellect. I am not impressed by your *coding ability* either. So why don't you prove that you are capable first before you assert racial aspects. You see, Bill Gates, before you can attribute superior intellectual abilities to *all white people* you have to prove that these theories hold for you first.

Why does Bill Gates think that calling everything 'trivia' will compel people that he is a great genius. This is obviously a childish devious ploy to avoid actually showing any intellectual ability or skill at all? Why is what Erik Ivar Fredholm did with study of these integral equations trivia? Why is what David Hilbert did producing mathematical understanding of complete inner product spaces trivia? There was a vast improvement in the world's understanding of functions on a circle that resulted from it, as $L^2[-\pi,\pi]$ and Fourier coefficients allowed proof that all square integrable functions had convergence in L^2 norm. It's one of the most beautiful parts of mathematics. What makes all this 'trivia' to Bill Gates except ignorance and bad taste and illiterate stupidity? It is rather *ethnicity* that is trivia, and not these far more lofty achievements of Man.

9. Return To Functions

I am pleased with the focus of Jens Lenström on basic issues because the history was not crisp and clear to me. For a long time, the definition of function was due to Euler from 1748 which reads: "A function of a variable quantity is an *analytic expression* composed in any which way whatsoever of the variable quantity and numbers or constant quantities." Now it was not exactly easy for me to think about functions in a different way myself in high school 1987-1991. I think that I was able to psychologically handle functions only after a summer spent at Ohio State University Arnold Ross number theory program in 1988. The assignment of a unique value $f(x)$ for $f : X \to Y$ became habitual for me around this time. Now 1900 was just around 150 years after the definition of function, and this matters to me even now profoundly because I am working on fundamental physics. I need to understand more clearly what it means for me to say, "Let $\phi : S^4 \to \Sigma S^4$ be a matter field." Thus in a sense, the same questions persist because, I would posit, that there are levels of clarity and crispness that slowly governs these fundamental concepts. When Laurent Schwartz introduced distributions, he generalised notions of functions to duals of function spaces, linear duals. And then now I am asking the world to consider matter in the university as spinor fields on a four-sphere universe *only a small part of which we experience directly*, i.e. the physical hypersurface. And so our notion of function will gain further clarity and maturity over time. But these fundamental issues are not arbitrary at all and had never been. They are not decided by axioms; that is a seriously misguided way to understand mathematics. The axioms always represent some substance and this substance, at least for the mathematical models of Nature, is fundamentally of interest, because it is what allows predictions of our models to have realisation in the actual world.

10. Some Interesting Oddities Of The Way We See Objective Reality

What is objective reality. I was strongly scientific during the first part of my life. I venerated my devout Muslim maternal grandmother. She was religious, and her devotion to Islamic piety was pleasant to me. And yet I was an Atheist from

1979. In the period 1979-2008 I was Atheist, but in America, where there was no religious pressure. That year I was curious about many things among them I examined this old idea of Descartes that the *pineal gland* was the locus of the Soul's connection with the physical body. "The pineal gland produces melatonin, a serotonin-derived hormone which modulates sleep patterns in both circadian and seasonal cycles." Not that interesting. But then: "The pineal gland is located in the dorsal diencephalon, and it contains both rod and cone photoreceptor cells. The pineal photoreceptor cells share great homologies with retinal photoreceptor cells, such as cell morphology, the expression of opsin proteins, and responses to light stimuli." [3]. I will not produce a theory here except to point out that the actual universe has a very large purely fourth spatial dimension, so there is something to this old religious idea of a 'third eye' and there is some interest in understanding this in rigorous scientific manner. I will simply leave this as an open question since I don't have the energy to get into a controversy regarding this now.

Well, Stanford Physics, there are more things in heaven and Earth than are dreamt of in your philosophy. But that is the point of four-sphere theory. We want to understand what the things are in heaven and Earth, and S4 Electromagnetic law governs all thing in a static eternal constant radius four-sphere. But I won't get involved in interminable and fruitless debates on things that have no resolution by experiment.

11. Zulf Redraws The Line Between Science And Religion

Once four-sphere theory is firmly established, as I am sure that it will, the line between Religion and Science will change in the following way.

All of existence is a large four-sphere of fixed radius $R = 3075.69$ Mpc. We will have our lives in a dynamically evolving three dimensional hypersurface $M(t) \to S^4(R)$. The four-sphere can have absolute coordinates, but the physical universe will not stay still in those coordinates. From within $M(t)$ we can detect, by indirect means, some phenomena in $S^4(R) - M(t)$. Those are phenomena that has *physically detectable and measurable effects* in $M(t)$. It will be a challenge of many centuries before human race has full understanding of what phenomena they are. And this is what I believe all of existence is, and the lines between Religion and Science will be redrawn over time. But Four-Sphere Theory needs to firmly and clearly established and tested before this for without a reliable fundamental physics with strong calibration to known phenomena in $M(t)$ there won't be any hope of objective understanding of anything more. It is a good idea to strengthen fundamental physics first before embarking on journeys into the unknown.

12. Problem I.3 a

$$\|Tf\|^2 = \int |\int K(x,y)f(y)dy|^2 dx$$

$$\& \leq \int\int |K(x,y)|^2 dxdy \|f\|^2$$

We used just Cauchy-Schwarz and have boundedness since $K \in L^2(\mathbf{R}^2)$. Now suppose $B = \{f \in L^2 : \|f\| \leq 1\}$. Suppose $g_n = Tf_n$ for an infinite sequence $f_n \in B$.

Let
$$k(y) = (\int K(x,y)^2 dy)^{1/2}$$
For all $n, m \geq 1$ we have
$$|g_n(y) - g_m(y)| \leq k(y)\|f_n - f_m\| = 2k(y)$$
Now for any $\epsilon > 0$ we can find $b > 0$ such that
$$\int_{|y|>b} k(y)^2 dy < \epsilon/2$$
This tells us that for all $m, n \geq 1$ and $|y| > b$ we have
$$|g_n(y) - g_m(y)| < \epsilon$$
We will keep our attention, therefore, on what is happening in $I_0 = \{y \in R : |y| < b\}$ with the same $\epsilon > 0$ fixed.

These are L^2 functions so we cannot simply use all sorts of Taylor expansions. However we can use *density arguments here* and try to get some subsequence converging to *zero*.

Let's now pretend $[-b, b]$ is $[0, 1]$ and invoke density of $C^1[0, 1]$ in $L^2[0, 1]$ by the obvious scaling and translation with $0 \mapsto 1/2$ and $-b, b \mapsto 0, 1$. We now are in the situation where g_n, g_m are all $C^1[0, 1]$ and
$$|g_n(y) - g_m(y)| \leq k(y)$$
from the $\epsilon > 0$ we take $\delta > 0$ so that for $|y - z| < \delta$ we have
$$|g_p(y) - g_p(z) - g_p'(z)(y - z)| < \epsilon$$
Then cover $[0, 1]$ with open intervals J_a of length at most 2δ and use compactness to select finite subcover $\{J_a\}_{a=1,...,P}$ with centers $y_1, \dots, y_p$. Define
$$g_{n,m}(y) = g_n(y) - g_m(y)$$
for all $y \in [0, 1]$.

We have double-indexing here and so we need to be a bit more careful later on. But I want to be sloppy right now to keep my thinking on the idea, so I hope my dear reader forgives my intuitive path.

Now use Heine-Borel theorem on $\mathbf{R}^N$ to choose a subsequence of vectors from $(g_{n,m}(y_1), \dots, g_{n,m}(y_N)) \in \mathbf{R}^N$ that converges. Then use Taylor's theorem to get the limit to be continuous. Let's call the limiting continuous function $G \in C[0, 1]$. We have
$$|G(y)| \leq k(y)$$
So then Lebesgue's Dominated Convergence theorem ensures that not only is G continuous but that the "limit" is pointwise as well as L^2 for $g_{n,m}$ to G. Our major problem is to understand what will ensure us to conclude $G = 0$ identically.

Now suppose $G \neq 0$ on J_a for some $a \in \{1, \dots, N\}$. Then we have the situation that $g_n(y_a) - g_m(y_a)$ pointwise led to some nonzero "limit", i.e. $G(y_a)$. I know this is not precise, but I am thinking about the issues here, so if you don't mind I will continue with my wooly thinking.

Let's look at this more closely by examining f_n, f_m.
$$|g_n(y_a) - g_m(y_a)| = |\int K(x, y_a)(f_n(x) - f_m(x))dx|$$

In other words
$$0 < A \leq |g_n(y_a) - g_m(y_a)| \leq k(y_a) < \infty$$
for all $n, m \geq 1$. Intuitively this will lead to a contradiction as follows. Let
$$s_n = g_n(y_a)$$
and these are just real numbers. We have a set of real numbers with $|s_p - s_q| \geq A > 0$ so we can order them in $[-k(y_a), k(y_a)]$ which are the bounds for the differences and then begin chaining the lengths from anywhere going to the right and at each step we will go to the right by at least A and eventually we will pass beyond $k(y_a)$.

13. Zulf Repents And Shows Contrition And Demonstrates Karma Is A Serious Force

You know, when I was a young student at Princeton, I had done some continued fraction approximations at Ohio State University at the Ross Program. I did not want my entire life to be filled with achievements of how many transcendental numbers had continued fraction representations efficiently represented. That's not something that would be suitable for a suave gentleman who likes the win the hearts of the beautiful ladies. I had resolved that these sorts of nitty-gritty things belong to the industrious work of all these *analysts*. I was not interested. I routinely skipped the first preliminary chapter of any text with 'real number system' and all sorts of lemmas about sequences of real numbers.

Well finally karma has caught up with me. I am staring at a situation that I had never mastered about the logic of sequences of real numbers satisfying
$$A \leq |s_n - s_m| \leq B$$
for all $n, m \geq 1$. This is a situation that actually requires some care and I have never actually done this problem. Here I am almost 50 years old, still doing these sorts of problems. Oh how the tables have turned! Failure to pay attention to these nitty gritty issues of the ordering of $\mathbf{R}$ comes back to produce problems for me.

Now I have to be like a youngling chaining together subsequences. Let's do it this way. Define a subsequence t_n this way.
$$t_1 = s_1$$
At each step t_{m+1} is defined by going towards left or right based on whether infinitely many of the remaining set of $S_m = \{s_{n_m+1}, dots, \}$ are to the right of t_m or to the left of t_m. By assumption, every element of S_m will be at least distance A away from t_m. And so we can chain them so that $|t_m - t_1| \geq (m-1)A$. Using this argument we can contradict $A > 0$ for bounded differences.

This is not even intuitive for me because my general intuition is not about differences of real numbers but about sequences themselves. And in Problem I.3(a) this is crucial, a fact about real sequences that I was not even particularly attentive to from freshman year.

It's a sad day for Zulf today. The skies will fill up with dark clouds and rain, to wash away my shame.

Let us respect Dan Stroock's view that this requires more precision. The key here is that these S_m are relatively closed because we have an ordering from $\mathbf{R}$ and each point is isolated from all others in S_m by at least A distance. Given any $x \in S_m$ we have $(x - A/2, x + A/2) \cap S_m = \{x\}$ so S_m is relatively closed in $\mathbf{R}$. Then depending on whether S_m is to the right of t_m or left, we have $\inf S_m$ or

$\sup S_m$ contained in S_m then we can choose the smallest or largest element of S_m and then the conclusion follows from the ordering of S_m. The ordering does not actually care about the sequence numbering of s_n at all we just invoke abstractly ordering properties of $\mathbf{R}$.

This is a pretty nitty gritty fact that's quite subtle. I am surprised this was not known to me, because these occur all the time for Cauchy-sequence type situations.

Then we apply this thing to contradict that the any limit of these $|g_{n,m}(y_a)|$ can be positive and finally we have a convergent subsequence at y_a to zero.

This is strange to have to do all this but I don't know a better way.

14. Problem I.3a Take Two

I was floundering on Problem I.3a before so I will try to streamline things to get to something rigorous. Compactness of L^2 kernel operators is profoundly important and so deserves care.

Here is what I will plan to do.

- We begin with assuming $\tilde{g}_n \in T(B)$. I change the notation with $\tilde{g}_n$ because I will approximate them with C^1 functions later that I will call g_n. I will get easily a bound
$$|\tilde{g}_n(y) - \tilde{g}_m(y)| \leq k(y)$$
with $k(y) = (\int K(x,y)^2 dx)^{1/2}$ using and we will have $\tilde{g}_n = Tf_n$ for some set of L^2 functions
- I will aim to prove pointwise subsequence convergence for $\tilde{g}_n$. That's the goal, pointwise subsequence convergence, and then I will apply Lebesgue's dominated convergence for the L^2 convergence of the subsequence.
- I will fix $\epsilon > 0$ now and then first eliminate problems by noting that there exists a $b > 0$ so that
$$\int_{|y|>\epsilon} k(y)^2 dy < \epsilon$$
Then I will focus attention to *pointwise behavior of* $\tilde{g}_n(y)$ *for* $|y| \leq b$ *alone*
- Then I will use a scaling and translation to assume $\tilde{g}_n(y)1_{[-b,b]}$ are on $[0,1]$
- Then I will do a L^∞-approximation of $\tilde{g}_n(y)$ with $g \in C^1[0,1]$. This is some version of Littlewood's three principles theorem. I don't remember their names. The substance is not here so I don't care too much.
- Then I will begin with $g_n \in C^1[0,1]$ and for all $n,m \geq 1$ we have for $y \in [0,1]$:
$$|g_n(y) - g_m(y)| \leq k(y) + \epsilon$$
Note that I only do the approximation on $[0,1]$ avoiding all sorts of problems on the non-compact part $|y| > b$.
- The nitty gritty begins at this point. We let
$$g_{n,m}(y) = g_n(y) - g_m(y)$$
for convenience. Our goal is to prove that there exists a $P \geq 1$ so that for all $n,m \geq P$ that
$$|g_{n,m}(y)| < \epsilon$$
for all $y \in [0,1]$.

- We want to use the $g_{n,m} \in C^1[0,1]$ to obtain, for our $\epsilon > 0$ that was fixed, a $\delta > 0$ such that
$$|g_{n,m}(y) - g_{n,m}(w) - g'_{n,m}(w)(y-w)| < \epsilon$$
whenever $|y-w| < \delta$. This will allow us to cover $[0,1]$ with intervals of size at most 2δ and then find a subcover with centers $y_1, \ldots, y_N$ with intervals $J_a = (y_a - \delta, y_a + \delta) \cap [0,1]$.
  This is our setup. Then we claim that for each of these $y_1, \ldots, y_N$ the sequence of real numbers $g_{n,m}(y_q)$ has a Cauchy subsequence – or based on fairness to Bolzano – Bolzano subsequence which has the same definition as Cauchy but we call it Bolzano because he deserves credit for limits and so on.
- Then we enter the nitty gritty part of our arguments which will use ordering of $\mathbf{R}$ and use some topological tricks involving closed countable sets. This is something that is true, but it seems to be new.

15. The New Element Only

The new element here analytically is a very nitty gritty argument for a sequence of real numbers s_k. If $\{s_k\}$ satisfies
$$0 \leq A \leq |s_n - s_m| \leq B < \infty$$
for all $n, m \geq 1$ then $A = 0$. We assume $A > 0$ and produce a contradiction. We place all s_n in the real line and we construct a new sequence t_q in the following way. First $t_1 = s_1$. Then we consider two sets
$$S_1^- = \{s_j : j \geq 2, s_j < t_1\}$$
and
$$S_1^+ = \{s_j : j \geq 2, s_j > t_1\}$$
By assumption $|t_1 - s_j| > A$ for all $j \geq 2$ which implies that $S_1^- \cup S_1^+ \neq \emptyset$. On or the other has to have $|S_1^+| = \infty$ or $|S_1^-| = \infty$ as all the points are isolated with no intersection with other points in the sequence s_j. These are relatively closed sets and therefore $\inf S_1^+ \in S_1^+$ and $\sup S_1^- \in S_1^-$ when they are non-empty. Then chose t_2 based on which is infinite cardinality and closest point to t_1.

Then at each subsequent step, define analgous sets
$$S_q^+ = \{s_j : j \geq j_q, s_j > t_q\}$$
and continue this procedure. The j_q is the index that occurred in the sup or inf. We can continue this procedure and obtain a contradiction by Q steps where
$$QA > B$$
This topological sort of argument then ensures that we can get a subsequence that is Cauchy for $g_n(y_r)$ or $r = 1, \ldots, N$. The rest of the analysis is for Problem I.3a is more standard approximation using Taylor etc. This argument is elementary but it is quite esoteric so it's not something I have seen before. But it's right.

16. Zulf Requests Daniel Stroock To Publish A Sharper Version of The Result Here Under Both Our Names

Professor Daniel Stroock had been instrumental in meta to test this topological idea and he did most of the work on non-compact Hodge theory where he shared credit with me. So it's only fair that he should publish a sharper version that is more acceptable to analysts. I hope that he will do this with sharing credit with me of course. It's a very general theorem here. If you can get Cauchy differences bounded by an L^2 function you can extact a subsequence that converges both pointwise and in L^2. Professor Stroock's books on Analysis as well as references will do well with this move. It's not so important for me as I am more keen on credit for Four-Sphere Theory. And besides, he knows better where to publish something general like this so analysts put it in all the textbooks than myself, so this is a pleasure to seek a joint publication from him.

References

[1] https://arxiv.org/abs/physics/0408077

[2] Jens Lenström, On the origin and early history of functional analysis, Uppsala, 2008

[3] Xinle Li,Jake Montgomery, Wesley Cheng, Jung Hyun Noh, David R. Hyde, and Lei Li, Pineal Photoreceptor Cells Are Required for Maintaining the Circadian Rhythms of Behavioral Visual Sensitivity in Zebrafish,PLoS One. 2012; 7(7)