The Origins of Quantum Mechanics

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Abstract

A variety of speculations about the nature of quantum mechanics and wave-function collapse. A number of "key principles" are set down; these must surely hold true. Holding onto these, a variety of mathematical effects are explored, to see if or how they might be appropriate for describing QM, and its relationship to space and time.

Although this appeals to a variety of mathematical ideas, this is an attempt to do physics, i.e. to argue with words, instead of writing down formulas. No formulas, just some postulates of things that seem like they are true or should be true. Fairly random and disorganized thoughts.

Many of these here are bad ideas, wrong somehow. The ideas near the end are probably much better than the ideas at the beginning, since they are sharper, more pointed. Its edited in a hap-hazard way; I change my mind as I go along.

1 Intro

So, this is an attempt to unify quantum mechanics with gravitation. Its primarily a discussion of principles that seem like they should guide such a unification. This means few or no equations. Some of these principles may be controversial, and surely some readers will think they're (not even) wrong. They might be, but its the groundwork for my thinking.

This also begins by (almost) completely ignoring the work of others on quantum gravity and strings and Penrose and ER=EPR and AdS/CFT and Schrieber's SuperPoint and so-on. I want to state what seem to be "first principles", whereas these existing theories have already made many explicit or implicit assumptions about first principles that are (possibly?) in contradiction with what I'm pondering. This does not mean that I reject these theories; I partially understand many of them, and kind-of like them (they are interesting, fun, elegant, and worth getting lost in). But rather, if we start with them, then we are forced into arcana almost immediately. I want to start with a clean slate.

Argument from authority: I have a PhD in theoretical particle physics, and have studied gravitation (Riemannian geometry), affine Lie algebras (string theory), and also the various places where these things take you: algebraic topology, category theory, sheaves and many other topics in math. This is a kind of "appeal to authority" (argumentum ad verecundiam) - more accurately, a request to the reader to not immediately dismiss this text as quackery.

To conclude: this is trying to be an informed discussion about mathematics that does not use formulas. At times, this might make it sound simplistic. I am trying to write in the most simplistic way that I can do so. This is intentional. Do not be fooled.

2 Statement of Principles

We take these to be self-evident facts. Or something thereabouts.

Principle: Quantum post facto The concept of quantum mechanics should come out of the unification of QM and gravity, rather than being one of the ingredients going into it. In particular, the Feynman functional integral should be something that comes out of the theory, as an unavoidable result, rather than going into it.

Principle: Quantum is primarily microscopic This is a rejection of the Schroedinger-Cat paradox. I'm not sure, it may be equivalent to a rejection of Many-Worlds. Its a statement about wave-function collapse. The idea is that any quantum effect (e.g. quantum measurement) that causes sufficiently large perturbations in its surroundings causes a "wave-function collapse". The cut-off point seems to be the Planck mass. That is, when the decaying nucleus that kills the cat fires off, it is in a superposition, but only to the point that about one Planck-mass of detector atoms are involved. After that, alternative histories either converge or spontaneously evaporate. So, for example, although there are and continue to be "many worlds" at the quantum scale, once some of these interact with the macroscopic scale, the probabilities of the alternative worlds shrink exponentially to zero.

The argument is that gravitation is too weak to have any significant effect on the microscopic scale, and the fact that an electron might be in many places at once has vanishing gravitational effect, up to the point that the electron has disturbed about 10^{18} atoms, at which point, gravitational effects can no longer be ignored. That is, gravitation remains classical, and there is a single unique space-time geometry at the macroscopic scale. Put another way: if Schrödinger cat is alive, space-time curves one way, and if the cat is dead, then space-time curves another way. There is no superposition of different space-time curvatures. There are not many-worlds of parallel space-times.

This is an idea brazenly stolen from Roger Penrose, who surely states it in some more eloquent fashion than I can muster.

Supporting argument: See 2"Macroscopic states have no phase", below.

Objection: Bose-Einstein states If wave-function collapse occurs at the 10^{18} atom scale, then how does one explain superfluidity, which involves more than that many atoms? Conditional answer: because "true collapse" involves only fermions... See comments about spinc group, below.

Speculation: Entanglement has something to do with the spin group. The spin group (see below) is constructed from the unit-length of a Clifford algebra, and naturally factorizes into Weyl spinors and anti-spinors (which, by the way, anti-commute correctly). A single point on the spin group seems to describe N fermions, and a change of basis implies that these fermions are entangled. Note that these spinors are described without any reference whatsoever to any ambient space-time.

This is perhaps an absurd and premature speculation, since one can get wave-function entanglement simply by considering any complex projective space; the spin group is not at all required for this. Later on, some support for this speculation is provided.

Speculation: Wave-function collapse is like spontaneous symmetry breaking! The standard formulation of relativistic quantum field theory formulated on flat Minkowski space connects up several distinct things: First, the spin group is constructed, and the various half-spin and spin representations are noted. Next, a frame bundle is build, not just any frame bundle, but a spin structure. This is used to build a spin manifold. These are fancy words, given that the background is flat Minkowski space – but it gets the point across. Next, the observation is made that the whole construction is invariant under the Poincaré algebra. This allows an identification of spin representations as the same thing as the angular momentum.

Textbooks always handle Poincaré invariance on the tangent spaces of these bundles. There is an interesting paper by Ohanian (see later, below) that performs the same identification, but on the bundle itself, rather than on the tangent space. Which is an interesting confirmation that we've correctly integrated on the tangent space (i.e. that we've correctly taken the Lie derivatives into the corresponding exp maps).

The observation is that, at the Planck-mass scale, and larger, the assumption of global Poincaré invariance fails. This can trigger collapse in two ways: one way is that its is not energetically favorable to have multiple trajectories, multiple histories any more; the other way is that the integrability conditions on the spinor bundle get screwed up in some way. That is, the tangent vector (spinor) fields are no longer integrable, or that the integral curves converge into one. Perhaps some topological obstruction – some Stiefel-Whitney type cohomology cycle pops up. Perhaps its both...

Speculation: Wave-function collapse is like QCD confinement! The speculation here is that wave-function collapse, driven by gravitation, is essentially driven by the same mechanism as QCD confinement, except that the characteristic scale is completely different: systems with more than 10^{18} atoms break up into domains where classical physics dominates, whereas smaller systems can have QM superposition effects run freely inside of them.

The argument here is that QCD confinement occurs for *any* Yang-Mills theory for which the structure group is more complicated than U(1). This certainly should then apply to a spin-2 gauge particle (graviton) with a Yang-Mills type self-coupling in the field-strength term in the action, viz. with structure group SO(3,1) or Spin(4). However, since gravitation is so weak, the scale of the confined region is much much larger, in proportion to the inverse of the coupling.

We seem to kill two birds with one stone: this provides both the mechanism of the collapse, and also to characteristic scale for it. Gravitation provides a scale factor of the appropriate size, confinement provides the mechanism.

Note that QCD confinement remains a Millennium-prize problem.

Speculation: Entanglement is like superconductivity Specifically, that the entangled state of two qubits is like the entangled state of BCS pairs. Long-range order.

There is not one, but two plausibility arguments for this. First, we know from RHIC experiments that the QCD plasma behaves like a superfluid. By analogy, if wave-function collapse is like QCD confinement, then the entangled-state needs to be superfluid-like.

A second plausibility argument is the recent work on treating the surface of black holes as a superfluid; this was in pop press, need to track down the reference.

What is the order parameter? The phenomenology of the 2 two-state vector formalism suggests that the order parameter is some ratio of qubits to classical bits. See, for example, SIC-POVM (symmetric, informationally complete, positive operator valued measure).

Practical: Entanglement is conserved Entanglement is in some sense quasi-conserved, up to the point where its broken.

Is there an "entanglement current"? i.e. what if entanglement is like a conserved charge, is there a corresponding current, where the entanglement "leaked away" during a measurement?

Is there an index-theorem for entanglement? Some generalized Atiyah-Singer-ish thingy?

See above, paragraph 2"Collapse is Confinement": Entanglement is "conserved" just like superconductivity is "conserved" inside of the region of space where the temperature is low enough. In this case, entanglement "flows freely" in regions of 10^{18} or fewer atoms, and is broken for larger regions.

The analogy suggests that, just like superfluids are a mixture of "normal" and "superfluid" states, then perhaps quantum states can be mixtures of classical and quantum states. Entanglement is the "superfluid" state.

Principle: Space-time is emergent This is kind of a reversal of Verlinde's ideas. The concept of Lorentz invariance isn't possible, until one is working at a scale where space-time exists. The core issue here is that all of our clocks and rulers are microscopic and quantum mechanical: we measure time by vibrations of some quantum device. We measure space by counting wavelengths. The redefinition of mass (via Planck's constant) in the SI system makes it quantum based.

Speculation: Relativity requires that space-time is a sheaf The mathematicians already know that space-time is a sheaf; no surprise there. The speculation here is that the gluing axioms of a sheaf are exactly where the quantum effects live. So: the issue here is what do do about special relativity with respect to quantum mechanics. If we envision that the present is like an advancing wave-front, where everything in the past is frozen and fixed, while everything in the future is unknown and fluid, then the present is like a wave of freezing or of crystallization sweeping through space-time. The problem is that such a surface is manifestly not covariant; one cannot time-order points outside the light-cone. The solution to this seems to be to treat space-time as a sheaf, but to allow the gluing axioms of a sheaf (that convert a pre-sheaf into a sheaf) – allow those gluing axioms to only hold within the past-lightcone. That is, the past becomes frozen precisely because this is where the sections of a pre-sheaf are finally glued together into an actual sheaf, and become correlated and firm. Quantum mechanics exists exactly where things are not yet glued together. Quantum measurement is (very roughly, misleadingly) the act of gluing. More precisely, space-like separated measurements of entangled states (Bell states, etc.) are in a fluid state until the results of those measurements are brought back to a common location, thus placing both measurements in a common past-lightcone. It is here where the many-worlds "sections" get glued together in such a way that there are no violations of QM. Thus, in a sense, QM in space-like separated regions behaves like a pre-sheaf; the sections of the presheaf glue together along the edges of past light-cones.

To put this more precisely: the different histories from a many-worlds scenario encounter one-another on the surface of a light cone. It is there that the histories sort themselves out, kind-of-like the gluing of the gluing axioms joining different sections together. The actual gluing is actually dynamic, given by amplitudes - e.g. zero amplitude means "can't glue" - so in normal physics textbooks, you never see sheaves or gluing, you just see amplitudes and propagators. But those amplitudes are just stating what can be glued to what, and what cannot be. Insofar are propagators are light-like, that's where the gluing actually happens. They hyopthesis is that by the time you get to the planck mass (per Penrose), the requirement of "consistent histories" of a so-called "wave function collapse" means that most pre-sheaf sections have been glued together in a consistent way, and the number of possible "many worlds" has decreased in number: the only ones left are the one that have consistent histories in the past light-cone.

How many world are there in the MW interpretation? If the above hypothesis holds, then the naive answer would be "one per point in a space-like surface". Unlike the Everrett hypothesis, here, the number of possible worlds is not increasing at some exponential rate, but is holding steady (at some small uncountable cardinal, presumably at the cardinal of the continuum).

More precisely, there is one world of many-worlds per 3-D plank volume. By poincare duality, there is one world per planck mass. The total number of worlds in many-worlds is then the mass of the universe, measured in planck-mass units. So its not an uncountable cardinal; its much much smaller (*viz.* finite) and its holding steady.

This speculation is in naive conflict with the Feynman path integral; there, the action is obtained from Lagrangian over all space-time (and not over the past light-cone) and the functional integral is over all of the many-worlds (and not over just the yet-to-be-glued-together ones). There might be a way of making the above speculation fit with this standard-issue QFT, but it requires some subtlety.

This speculation also seems to be at odds with the Hawking result that entropy is proportional to area (one-fourth the area). The area in turn goes as the mass-squared. By adscft this implies that the number of worlds should go as mass-squared of the universe not as the mass. So something is being mis-counted. Is it possible that there is a square-root in here? Viz: entropy is about thermodynamics and bits; many worlds is about qubits; in some sense perhaps this sneaks in the needed square-root to go from counting thermodynamics bits, to counting the number of many worlds? Something like this, perhaps? "for N large, N qubits can be approximate by N^2 classical bits." Well, except that is exactly the number of (classical) bits needed to quantum-telelport N qubits. So in fact, the Bekenstein-Hawking bound is in perfect agreement with the estimate of the number of qubits.

Phenomenology: Two-state vector formalism The work on weak measurements suggests that the two-state vector formalism (TSVF) seems to be the correct formalism for describing quantum mechanics. The explicit dependence on the future seems to be consistent with the "transactional interpretation of quantum mechanics", where a measurement at the current time seems to reach back in time, to twiddle and reconcile qubits in the past.

From what I can tell, the correctness of the TVSF is embraced in the quantum-computing world, where the positive-operator valued measure (POVM) is the *defacto* technique by which quantum computing is formulated.

This is meant to provide support for the 2 principle that space-time is emergent. That is, quantum phenomena are not localized in either space, nor in time. The former, we've gotten used to, the latter, specifically, the acausal-like nature of wave-function collapse, still grates on many, but we have to accept it; we even have to accept a "strong" version of it: the future alters the past. However, this "acausality" is limited explicitly to qubits only. Future measurements alter qubits in the past, but cannot alter classical bits, which still flow in a causal, time-like fashion.

Phenomenology: there are no electrically charged bosons Well, there are: W+/-, but that's beside the point, the point being that electrons have spin half and that the spin is the ultimate source for the quantum effects, when coupled to the electric field. Viz spin is a twisting up of space. Rather than treating spin as a representation of the rotational symmetry of space, we should treat spin as a certain tangling up of multiple paths (e.g. paths in the sense of functional integral, or in the sense of Hamilton-Jacobi variational principle) so that we should envision these paths as a kind-of-like Hopf fibration – the paths are the fibers, the resulting fibration is an object that belongs to the 2 representation of SU(2).

That is, we treat quantum paths as if they were fibers, and then ask: how can we tangle them up, so that the grand-total holonomy behaves as if it were a spin-1/2 object? I suspect that such a tangling is impossible, without also pairing the object with a second one, i.e. in the sense of "quantum entanglement".

What does electric charge have to do with this? The idea here is that the electric charge at large distances looks like central-force attractive/repulsive, and so one can ask about the geodesics of the free-falling electrically-charged observer. The free-falling observer does fine, until he gets within a Compton-length of the electron, in which case there's a fibration of multiple paths that become possible.

That is, the point-like nature of the observer/geodesic becomes replaced by a spray(??) so that instead of saying "a single thing is in multiple locations" (as is normally done in QM), we should instead say "there are multiple locations associated with a single thing" (which seems to be the same sentence, but is not): i.e. that space-time no longer has a simple structure at the microscopic level. The concept of "location" becomes flawed in some certain way.

Nonetheless, Minkowski space seems to provide a valid description between the merely microscopic, and the Planck length, which is truly minuscule. Why?

Phenomenology: Macroscopic states have no phase There is a persistent confusion about the Schrödinger Cat state, which can be resolved by observing that there is no practical way of changing the phase of macroscopic states. There is no device that can do this.

That is, it is commonly suggested that the Schrödinger cat state can be written as

$$|Z\rangle = \frac{1}{\sqrt{2}}\left(|\psi_{\text{dead}}\rangle + |\psi_{\text{alive}}\rangle\right)$$

but the problem/fallacy of the above expression is that there is no practical way of rotating the phase of either "wave-function", e.g. by applying some quantum gate. This implies that there is no way to construct the states

$$|X\rangle = \frac{1}{\sqrt{2}}\left(|\psi_{\text{dead}}\rangle - |\psi_{\text{alive}}\rangle\right)$$

or

$$|Y\rangle = \frac{1}{\sqrt{2}}\left(|\psi_{\text{dead}}\rangle + i\,|\psi_{\text{alive}}\rangle\right)$$

which thus prevents measurements along any other qubit axes than the Z axis. This suggests that the Schrödinger cat "state" is not really a quantum state at all. If one cannot manipulate it as a true qubit, then, by what abstraction are we allowed to pretend that its an actual qubit?

Phenomenology: The nature of a measurement There seems to be a lot of confusion about the nature of a measurement. The quantum eraser seems to highlight that confusion most clearly. The point is this: taking a quantum state, and sending it to two different physical locations, based on projections of its state, is not a "measurement"; it is merely the creation of entangled pairs with spatial separation. So: sending a photon through a birefringent crystal does not constitute a "measurement"; rather, its a just a variation of a two-slit experiment; phase coherence is not destroyed, and the two arms can be treated as the arms of an interferometer; the beams recombined. Similar remarks apply to the Stern-Gerlach magnets: in principle, if the magnetic fields and beam direction were precisely controlled, the phases would not be randomized, and the spin-1/2 beams in a Stern-Gerlach magnet could be recombined in a coherent fashion, forming an interferometer. These remarks apply, in particular, to the quarter-wave plates and polarizers of the quantum eraser: they are marking, not measuring.

The measurement occurs when the particle interacts with the detector; it is at this point that phase coherence is destroyed, and the situation turns into a Schrödinger-cat situation: there are no devices, no quantum-gates that can rotate a detection+non-detection state into some other orthogonal direction.

Phenomenology: most wave-function collapses do not matter Consider a photon striking a photographic plate. Suppose that photon came from a single narrow slit, so that its QM description would be the typical QM wave illuminating the photographic plate. The wave function collapse here is when some-or-another grain of silver iodide absorbs the photon. The classical description of this event is to say that the photographic plate is now in a superposition of many states: any one of millions of silver-iodide crystals might have absorbed that photon, and so that plate is in a (macroscopic) superposition of millions of states.

Consider now the future time evolution of that superposition. The plate might be shattered before it is developed, or maybe after. It might be discarded into a trash-heap before the experimenter looks at it. It might be discarded into a trash-heap after the experimenter looks at it, but considers the results so unimportant and uninteresting that they are promptly forgotten. Possibly the plate records something very important; the experimenter publishes a paper on what that plate recored. Did it matter if the important discovery in the plate was in the center, or shifted over a few centimeters one way or another?

The point here is that, in almost all photographic-plate superpositions, there are almost no long-time-period effects that are not thermalized. If the plate is broken before being developed, it is essentially thermalized immediately: all future timelines of the superposed plate are essentially equivalent. There is no "for want of a nail, the shoe was lost" effect. Thus, in this sense, (almost) all superpositions of the plate belong to the same equivalence class. One can pick a single representative from the equivalence class (*viz*, the photon explicitly hit this one crystal over here), and that single representative is sufficient to represent the forward time-like evolution of the universe. A different representative (*viz*, the photon explicitly hit this other crystal over there) gives the same result.

This implies a certain duality: either a wave-function collapse did occur, but it thermalized and did not matter, or a wave-function collapse did not occur, and there is a thermalized superposition of shards of film-plate moving forward in time. Either ontological beleif (the collapse did occur; it did not occur) appears to describe essentially the same outcome.

How long must one wait until thermalization? If the universe was described by infinite-precision real numbers, one might imagine after-shocks that continue a very long time. Insofar as the universe halts at the planck scale, that's where equivalence classes of superposed states become indistinguishable from any one member of that equivalence class.

The upshot: things thermalize in finite time, not infinite time. After a finite time, which specific, actual wave-function collapse occured no longer matters; any other collapse has the same effect on the future universe. All the different many-world possibilities are erased, and consolidated into one.

Objection: the ultimate thermalizer would be an event horizon. We know that classical bits continue to live on the event horizon; where did the qubits go, when they crossed? Are the classical bits on an event horizon exacly what is neeed to quantum-teleport qubits fom the other side? *Viz*, one needs N^2 classical bits to quantum-teleport N qubits; the mass of the black hole is proportional to the N qubits; the area of the event horizon is propotional to the mass-squared, and thus is equal to the number of classical bits needed to teleport those qubits to the other side. Can we conclude: event horizons do not destroy qubits? Equivalently, mass and qubits are the same thing.

To what extent can this be taken literally? Classical dynamical system evolve "on mass shell", conserving energy. Semi-classical methods expand in orders of $\hbar$ around the mass shell. Hmmm...

Phenomenology: classical approximations of the qubit The use of entangled particles in a quantum eraser setup allows one to at least partly "fake it": given enough measurements, one can use the record of the measurements, recorded as classical bits, to reconstruct interference patterns, or not, by selectively ignoring certain measurements (i.e. by ignoring certain bit-streams, those bit-streams of classical bits that recorded certain detections corresponding to certain placements of polarizers and quarter-wave places). However, this reconstruction and erasure of interference patterns from classical bits can only be done at certain phase angles: the collected data is not enough to alter the phase, post-facto, to any arbitrary value. That is, the quantum eraser setup (with an entangled pair) is enough to classically emulate $|0\rangle + |1\rangle$ but is not enough to emulate $|0\rangle + e^{i\phi}|1\rangle$ for any arbitrary phase ϕ, post-facto (after the measurement).

This does suggest that a tripartite entanglement, e.g. a GHZ state, used in a quantum-eraser setup, might allow one to build a better approximation of a qubit by multiple classical measurements, e.g. at phases at multiples of $2\pi/3$. Continuing in this direction, an entangled state of N particles can be used to approximate a (single) qubit at phase angles of $2\pi/N$.

Continuing in this train of thought, the large-N limit of entanglement perhaps resembles a very highly mixed black hole. This suggests that a series of quantum-eraser-style measurements against such a black hole allows one to model the full Bloch sphere (or a single qubit) with the classical bits recording the results of measurements: in a sense, a single black hole, being the large-N limit, could be taken to represent (or "be") a single qubit, thus supporting the ER=EPR conjecture.

Speculative framework: fibration Taking the idea that a wave function is a "particle in multiple places at once", consider then a fibration where points on the fibers are the different space-time locations for the particle. The base space consists of particles, themselves. Specifically, the base-space is some very high-dimensional representation of a Lie algebra, and the "particles" are the decomposition of that rep into various irreducible reps. Obvious stumbling blocks: in what sense can a rep be a "space"?

For example, the base space of the fibration could be the spin group. If the spin group is decomposed into spinors, then the fiber above the spinor records the possible spatial locations that spinor could have. A distribution on the fiber then corresponds to the wave function of that particular spinor. Since the decomposition of the spin group into Weyl spinors depends on the choice of basis, then picking a different basis corresponds to (among other things) the exchange of identical particles. In that sense, we get the Pauli exclusion principle "for free"; it is deduced from the starting point.

Hmm. Fibration is the wrong concept here. Sheaves, sections, etales, topi are more appropriate. Its not so much that a "particle is in multiple places at once"; rather, a particle is in multiple sections of space-time at once. The particle is fixed, it is being blasted into different parts of space-time by the Yoneda lemma. Space-time isn't single-valued, kind-of-like in the sense of a Grothendieck topology: there are multiple parts to it, they are joined together in a sheaf-like way.

Speculation: Cartan geometry Instead of asking "how can a particle be in two places at once", we ask "how can there be two spaces at once for a single particle?" An insight into this is provided by Urs Shreiber in his "because Deligne" blog post. Start by observing that the Poincaré group (call it G) is the isometry group of Minkowski space (it acts transitively). The Lorentz group (call it H) is the subgroup that fixes points (its the stabilizer). Thus, we can take the quotient G/H to be Minkowski space itself: So G/H is an example of a Klein geometry. The tangent space is H, and if we promote it to a local symmetry i.e. use it to define the vielbeins or frame fields, then one gets Cartan spaces (or General Relativity in the case of Poincaré=Iso(3,1))

The next step is to associate a massive particle with every possible connection consistent with their being a massive particle. That is, start with some fixed background geometry, satisfying the classical Einstein equations. Next, consider all possible perturbations to the background geometry, consistent with the presence of a fixed small mass. Call this space P. For each such perturbation, there is a perturbation to the vierbeins (by abuse of notation, call this P also). Natural questions include: when are two such perturbations smoothly homotopic? Can we define a "particle" to be the class of all homotopic perturbations?

The problem with this vision is that we don't yet have a measure for the space P (although we know that it needs to be the Feynman path integral). So: consider two "nearby" perturbations (two points in P). The task at hand is to derive a scalar action (Klein-Gordon Lagrangian) relating these two points, given a starting point of requiring only that the perturbations are consistent with the Einstein eqns. How might one do this?

If one has just a single perturbation of this sort, and asks about its time-evolution in the background geometry, one should get the classical eqns for a (point) particle in a gravitational field.

Note: another way of thinking about "because Deligne" is the speculation that gluing of pre-sheafs into sheaves happens along the edges of the past-lightcone. Very crudely, Deligne happens where past light-cones don't intersect.

Speculation: Taming the Feynman integral

The primary driver towards the many-worlds interpretation is the Feynman integral

$$Z = \int [\mathscr{D}\psi] \exp i\hbar \int d^4x \mathscr{L}$$

where the integration takes place over all possible worlds. One can regain a single macroscopic reality while retaining all quantum predictions by limiting the integration to the "microscopic" domain, and specifically, only below the scale of the Penrose Planck mass. But how might one go about doing this?

Well, lets look at a typical interference experiment. Here, one has wave-fuctions of the form $|a\rangle + e^{i\phi}|b\rangle$ for two states $|a\rangle$ and $|b\rangle$ on the different arms of the interferometer. The many-worlds integration to be performed is over the pahse angle ϕ – and, to be precise, it really needs to be performed over the qubit Bloch sphere S_2 and it really needs to be performed over a mixed state, viz. given a probabilistic weight for each pure state. This kind of integral is doable and possible because its essentially a two-particle integral, whereas the QFT path integral Z is over infinite particle states.

The way to get from a two-particle state to an infinite-particle state is to consider the tensor algebra, and specifically, the exterior algebra. Thus, the Feynman integral looks like an integral over the symmetric algebra (for bosons) and the exerior algebra (for fermions). Except that, for fermions, we kind-of know that it has to be over the Clifford algebra, instead. As one's thoughts plotz down this corridor, its kind-of impossible to bump into all the concepts that enrich string theory; so this speculation is more of an "OK go that way, but go sideways"... the goal is to formalize the integral, so that its computable, and not to re-invent string theory. Again: to be able to write the integral so it has a planck-mass cutoff in the $\mathscr{D}\psi$ integration...

3 Other stuff

Unsorted ideas that don't fit anywhere yet:

Mass:

Mass is an emergent property as well....

Bose-Einstein statistics

Is there any relationship between the form of Bose-Einstein statistics and the Baker-Campbell-Hausdorff formula, which has the generating function for the Bernoulli numbers in it: viz $x/(1-e^{-x})$? It seems like there should be: Bose-Einstein applies to a Fock space of identical particles, which can be described by the tensor algebra (is the same thing as the tensor algebra) - However, because of identical particles, etc. question posed here: Physics Overflow -Baker-Campbell-Hausdorf - Jordan Algebra – this work should be completed.

Possibly-useful references for this work: Alice Rogers, Supermanifolds

Work on Baker-Campbell-Hausdorff formula:

V.A. Kostelecky, M.M. Nieto, D.R. Truax, Baker-Campbell-Hausdorff relations for the supergroups, J. Math. Phys. 27 (5) (May 1986) 1419-1429.

V.A. Kostelecky, D.R. Truax, Baker-Campbell-Hausdorff relations for the super-Poincare group, J. Math. Phys. 28 (10) (October 1987) 2480-2487.

See also the PhD thesis:

Cook, James Steven. Foundations of Supermathematics with Applications to N=1 Supersymmetric Field Theory, circa page 232.

The flip side of this coin is: Can one state the analogous Poincaré–Birkhoff–Witt theorem for exterior algebras instead of tensor algebras? That is, by working with universal enveloping algebra and the (umm I guess its called the Gerstenhaber algebra) does one a BCH-like formula, but with a generating function of $1/(1+e^{-x})$ - that is, the Fermi-Dirac statistics? It seems plausible that this should work out.

Second Law of Thermodynamics

The Second Law of Thermodynamics says that entropy is increasing, and Boltzmann's constant says entropy is mass but this is "upside down" from hawking radiation/entropy. So, there seem to be conflicting ideas: the mass of thermodynamic systems goes up in proportion to the heat energy (see Planck, Max (1907), "Zur Dynamik bewegter Systeme", Sitzungsberichte der Königlich-Preussischen Akademie der Wissenschaften, Berlin, Erster Halbband (29): 542–570 English Wikisource translation: On the Dynamics of Moving Systems) ... so if you melt ice, the mass of the melt-water is greater than that of the original ice. Relatedly, there's a mass loss from binding energy. However, the relation of binding energy to entropy is opaque: when a system is bound, did the number of accessible states decrease? It would seem so: the ionized state is not accessible to the system. However, one does not normally associate an entropy with a QM system, but the goal is to argue that the entropy of atomic hydrogen is less than that of ionized hydrogen.

Hauptvermutung

The Hauptvermutung of geometric topology hypothesizes that any two triangulations of space are equivalent. It only holds in 2,3 dimensions, but is false in higher dimensions. A necessary condition for it's failing is that the Reidmeister torsion be non-zero for H^3.

The primary idea is that there are some "wild spheres" – fractal, hyperbolic spheres which have boundary-handles that are tangled into knots about each-other. (See the Edwards notes for a construction, see notes by Steve Ferry, chapter 24 for another construction)

If the nature of QCD confinement is that, to the quarks, space looks hyperbolic, so that the surface of the proton appears to be infinitely far away, then the question is, what is the geometry of that surface? Is it some wild sphere? If wave-function collapse appears to be similar to confinement, then the same question applies: are collapsed regions topologically wild?

4 Textbook Quantum Mechanics

The sections below review connections between various more-or-less textbook topics. Some of these are a bit speculative in how they are worded; some merely seem speculative, but can be found in standard textbooks. Anyway, the material is a bit of a stretch from what would be acceptable in a standard presentation, but not so far out that it should give professionals in the field conniptions.

4.1 What is spin? - Stern-Gerlach

The Ohanian paper "What is spin?" Ohanian-What is Spin? and several updates Gsponer - What is spin? and Extraordinary momentum and spin in evanescent waves suggest (flat-out state) that spin is a purely classical (but non-local) phenomenon of a wave. This begs for a re-analysis of the Stern-Gerlach experiment in this light: Stern-Gerlach is a textbook example of why quantization is needed, suggesting there is no other explanation – whereas the Ohanian paper suggests that, with the appropriate treatment, a classical wave packet of the Dirac field will feel a classical force acting on it, consistent with experimental results from Stern-Gerlach.

The Gsponer article suggests that the Dirac field is more easily described, intuited if written in a certain quaternion algebra. Maybe worth follow-up.

What is Ohanian really saying?

What he is saying is that spin is angular momentum. This feels like a tautology, so its worth clarifying the difference. Spin is described by representations of the Lorentz group. That is, a "classical" field can be understood as a (square-integrable) function from Minkowski space to a given, fixed representation of the Lorentz group. The representation defines the "spin" of the field. One might say that a classical field is a section of an associated fiber bundle over the base space of Minkowski space, with the principal bundle having fibers that are the Lorentz group. Thus, the associated bundle has fibers that transform under the Lorentz group. i.e. transform as representations of the Lorentz group. The label on the representation is the spin.

However, the definition of angular momentum requires the Poincaré group, because momentum is defined as the generator of translational symmetry, and angular momentum is defined as the generator of rotational symmetry of the base space. That is, the Poincaré group acts on both the fiber and on the base space. To have things actually be consistent, one wants to be able to identify angular momentum with spin, so that eigenstates of the angular momentum operator (which is defined only in the Poincaré algebra) correspond to eigenstates of the spin operator in the Lorentz algebra.

There are two ways to do this. One way is to consider a single point in space-time, fix it, and then consider the tangent space to that point: as a tangent space, it contains derivatives of the field (derivatives in the space-time directions), and Lie derivatives in the "vertical" space, along the fiber, i.e. valued in the Lorentz algebra. For a fixed spin representation, this is exactly the same thing as having the space-time derivatives be labeled by the Lorentz-algebra spin labels – this is why fields are always algebra-valued. At this point, one notes that the momentum and angular momentum operators of the Poincaré algebra act on that tangent space. Turing the crank, one discovers that spin can be interpreted as angular momentum. This is how most textbooks demonstrate the equality.

Another way to do this is to take a specific section of the fiber bundle, a specific field configuration, and ask what happens there. That is, instead of restricting to the algebra at a fixed point in space-time, one wants to ask about the structure of the function space, the structure of the Banach space of all functions defined on the base space. To answer this, one needs to do what Ohanian does. So, instead of working in the tangent space of a single point in space-time, he chooses to work in the space of all (square-integrable) functions. Yes, one expects to get the same answer, either way, but it is not entirely a foregone conclusion that this would be the case: now that the distinction being made above is explicit, one could certainly create insane configurations where the spin is not equal to the angular momentum. (this is kind-of what is happening with anyons!??)

What is quantum?

If first-quantized fields are "purely classical", in the above sense, then where does quantum come up, really? The olde-fashioned way is to say "quantum requires wave functions whose amplitude we square to get probability", but a more modern view is to say "fermions, which are grassman variables, are not observable alone, but do become classically visible as pairs." – that is we need to take squares of amplitudes because we are working with fermions, and not for other reasons. (Actually, fermions are not grassmann numbers; they belong to the spin space, which is constructed from real Clifford algebra times the complex numbers, which is a grassmann algebra only if one ignores the anti-fermions.) Looked at this way, the Schroedinger eqn for the hydrogen atom sure starts to feel like a "classical" wave equation problem. Its not really quantum, at all. The confusion came from the fact that someone kept thinking of the planetary model, which was wrong.

This suggests a question: so if the Schroedinger-hydrogen solution is really a "classical" result, then what is quantum? And we get back the old echo: "its the problem of wave-function collapse, you dummy", and specifically, the interpretation of the squared amplitude as a probability.

4.2 Complex numbers in QM

Why do complex numbers show up in QM? Why aren't wave functions purely real? Why can't they be purely real? Where does the complex phase come from? Various arguments are given below.

There is a completely different, but related question: Why is the probability the square of a complex-valued wave function? The arguments below do not seem to shed any light on that.

A1: Spin algebra? A conservative argument says that it comes from fermions, and specifically, the spin algebra. The spin algebra is built from a real Clifford algebra V as

$$V \otimes \mathbb{C} = W \oplus \overline{W}$$

(see e.g. Jurgen Jost, "Riemannian Geometry and Geometry Analysis, page 69) where V is a real Clifford algebra (itself built from a tensor algebra!) and is generated from $2n$ components in order to describe n Weyl spinors and n conjugate spinors. One cannot get the required spin structure without using $\mathbb{C}$ in this construction. This argument makes sense because most "physical" things in nature are fermions.

Note that, in order for fermions to exist, a bundle must be a spin structure. i.e. is a lift of a frame bundle to a double-covering of SO(N) by Spin(N). The usual remarks apply. But this works only for neutral fermions. For charged fermions, the bundle must have a Spinc structure, where as per usual notation, its a twisted product

$$\mathrm{Spin}^{\mathbb{C}}(n) = \mathrm{Spin}(n) \times_{\mathbb{Z}_2} \mathrm{U}(1)$$

i.e. the U(1) carries the electromagnetism. This paragraph is non-controversial, its the orthodox interpretation. The next question asks: is there another way? Or is it just a very basic confusion about the proliferation of complex numbers all shot-through QM and Clifford/spin algebra?

A2: Electromagnetism? Wild guess: Perhaps the complex phase is actually a hidden side-effect of electromagnetism? There are two chains of though that lead to this: First, that the Aharonov-Bohn effect is most easily, most geometrically understood in a "pre-quantum" fashion, as a holonomy of the U(1) fiber bundle. Thus, even before we perform any quantization, and just stick to "pre-quantization", we already get a complex phase that arises naturally, but only due to the electromagnetic structure on the fibre bundle.

The classifying space for U(1) bundles is $\mathbb{CP}^\infty$ and the thing about $\mathbb{CP}^\infty$ is that it is exactly the same things as a Hilbert space on which QM is naturally formulated. It is complex, because wave-functions are complex-valued, its projective, because only normalized wave functions matter. We can identify the Hilbert space with the occupation states of the simple harmonic oscillator.

Cell structure Some minor insight might be gleaned by reviewing the structure of $\mathbb{CP}^\infty$. The cellular structure (cell complex) of $\mathbb{CP}^\infty$consists of all even dimensional cells (See "Algebraic Topology", Allen Hatcher, chapter 0 page 7) (See S.P. Novikov, "Topology I: General Survey" eqn 3.15 page 62). That is, one writes

$$\mathbb{CP}^\infty = \kappa^0 \cup \kappa^2 \cup \kappa^4 \cup \cdots$$

with

$$\kappa^{2n} = \mathbb{CP}^n \backslash \mathbb{CP}^{n-1} = \left\{ (z_0 : \cdots : z_n) | : z_j \neq 0 \, \forall j \right\} \simeq D^{2n}$$

where by abuse of notation, the above makes the mistake of writing a cell, which is actually a map of a disk, with the disk itself. Oh well. This is just a mnemonic reminder, that's all.

The point is that a single qubit is $\mathbb{CP}^1$, i.e. is the Bloch sphere, i.e. is the 2-sphere i.e. is the 2-disk with the edge of the disk identified to a point. Then n qubits are $\mathbb{CP}^n$, etc. The reason that qubits are projective is because only the relative phases of wave-functions matter, and the amplitude of a wave function is always normalized to one. Specifically, a single qubit is

$$\mathbb{CP}^1 = \left\{ z_0 \left|\uparrow\right\rangle + z_1 \left|\downarrow\right\rangle \middle| |z_0|^2 + |z_1|^2 = 1, \; z_0/z_1 \text{ real} \right\} \simeq S^2$$

and the gluing together of these things requires only the even-dimensional cells...

To summarize – is it possible that the complex-valued wave function of textbook QM secretly a side-effect of electromagnetism and U(1) bundles? Or is this a red herring?

A3: Spherical harmonics counter-argument We can view 3D space as a homogeneous space of the rotation group. Recall that homogeneous spaces are defined as quotients of Lie groups. In this case, the Y_{lm} spherical wave functions needed to represent the spherical harmonics are naturally complex-valued, and this natural-complex-valued-ness has nothing at all to do with electromagnetism. (Unless, of course, the 3-dimensionality of space was a side-effect of electromagnetism...)

A4: Counter-counter-argument Now that we've invoked the rotation group, the inevitable is in store. Namely, vectors belong to the adjoint rep of the rotation group. Maxwell equations are exactly the same thing as a statement about the transformation properties of these vectors under the action of the Poincaré group. That is, the electric and magnetic fields are vectors in the Lorentz algebra, i.e. live in the "vertical" fibers, the fibers being in the associated bundle of the Lorentz group. The actual transformation properties of these vectors, under "horizontal" translations in Minkowski space, are given by the Poincaré algebra. These are exactly equal to the Maxwell equations.

The fact that the electric and magnetic fields satisfy the wave equation is due to the fact that the wave equation is just the quadratic Casimir operator of the Poincaré algebra. (A worthy exercise is to write down the details to support these claims).

Opaque in the above construction is how U(1) arises. The Poincaré algebra gave us a differential structure. To get the A field from the E,B fields requires the notion of differential topology, i.e. the notion of closed forms for the E-field, and exact forms for the B-field. These last two statements follows from the fact that electric charges exist, magnetic ones don't (except in the form of a holonomy). So why should electric charges exist, but magnetic ones not? This is partly answered by looking at the representation theory of the Poincaré group: it has massive particles in it. These can have an electric charge, because "mass" and "electric charge" both point in the same direction: i.e. in the time-like direction. By contrast, magnetic monopoles could only be consistent with imaginary mass, i.e. tachyonic degrees of freedom. So the Poincaré algebra is really a kind of no-go for magnetic monopoles. This last sentence is highly textbook non-standard, and so needs to be fully fleshed out in detail, as it is non-obvious.

Once one deduces that the Poincaré algebra effectively prohibits magnetic monopoles (except in the form of a holonomy) then the standard machinery of differential topology allows one to deduce U(1) from the A field.

Side note: as soon as one invokes the rotation group, it necessarily follows that one must discuss the fundamental rep, not just the adjoint rep. The fundamental rep leads invariably to the spin algebra, mentioned up top.

In sum, this is a rather long argument. It raises the question: why is space 3+1 dimensional? Because, as soon as we know its 3+1 dimensional, the above purports to show that (a) electromagnetism is inevitable, as a rep of the Poincaré algebra (b) The U(1) structure is inevitable, (c) spin structures are inevitable (d) complex-valued wave-functions are inevitable.

What it does not yet show is why the square of the wave-function can be interpreted as a probability, and why wave-functions must collapse.

4.3 Probability and wave functions

Why does the square of the wave function yield a probability? Where do the complex numbers come in, and why?

A1: Maybe its entropy One suggestive argument comes from Mikhail Gromov, in "In a Search for a Structure, Part 1: On Entropy." Relevant aspects are summarized in the wikipedia article Fisher information metric. To repeat them here: consider the finite-dimensional simplex of probabilities summing to one: $\sum_i p_i = 1$. Consider the change of variable $p_i = y_i^2$ so that the simplex is mapped to a quadrant of a sphere: $\sum_i y_i^2 = 1$. Consider the the flat metric in the Euclidean space $y = (y_1, \cdots, y_n)$ This flat metric induces a metric on the sphere. Converting coordinates back to the p_i one finds that the flat Euclidean metric has become the Fisher information metric! That is, if we want to consider how close two different probability distributions are, we use the Fisher information metric; alternately, we can take the square root of the probability, and use the Euclidean metric.

The above "just works", Gromov adds a category-theoretic argument, that the manipulations pass through for the limit $n \to \infty$, provided the various measures are square integrable; basically, it's just fine if the point y is taken to belong to a Hilbert space.

This conception of the metric passes over to the Fubini-Study metric, as mentioned in that article, when one takes the y to be complex-valued: What this says is that basically, the Fubini-Study metric on $\mathbb{CP}^n$ is essentially the same thing as the flat Euclidean metric. That is, to compute the distance between two complex-valued wave functions, one simply takes their difference.

Put another way: the difference in the information between two wave functions is just their difference, but constrained to the fact that they live in complex projective space, and not in Euclidean space. Precisely, this is the Burres metric, with lots of details glossed over. The projective nature of it all makes the formulas look complicated, obscuring the underlying phenomenon.

4.4 Wave function collapse is comultiplication

The spin algebra that describes Weyl spinors is constructed from a Clifford algebra, as mentioned above. The Clifford algebra is in turn constructed from the tensor algebra. The part that is of interest here is that the tensor algebra posses a Hopf algebra structure, and I believe (should double-check) that this Hopf structure survives all the way to to the spin algebra.

One may then ask the question: what corresponds to comultiplication in the Hopf algebra? One seemingly plausible answer is that it corresponds to wave-function collapse. That is, if one has a single fermion (single Weyl spinor) v, under comultiplication, it passes over to

$$\Delta(v) = 1 \boxtimes v + v \boxtimes 1$$

where I used $\boxtimes$ to perversely note the tensor product of two tensor algebras, so as not to confuse it with the tensor product that is internal to the tensor algebra itself (which is a fatal mistake). (I steal this idea from the Wikipedia article on the topic, which is

OK, since I wrote that Wikipedia article! Catch-22) That is $\Delta(c) \in TV \boxtimes TV$. What does this mean? How can one interpret this? Well, if TV, or more precisely, the spin algebra constructed from TV describes the configuration of some number of number of spinors in the universe, (including the fact that they obey the Grassmann algebra, i.e. Pauli exclusion), then one must conclude that $TV \boxtimes TV$ describes two universes, i.e. two worlds of the many-worlds hypothesis. The comultiplication above is stating that these two universes are in superposition (the box $\boxtimes$ is still a tensor product; its bilinear; so the superposition is captured by this expression). However, it is also stating that the fermion that is NOT at a particular location in one universe, is in that location in a different universe. i.e. the collapse proceeded so that, in each universe, the fermion ends up in a different position (and that all of these collapsed states are still coherent).

Hmm. This needs more work. We want to say that the fermion collapsed to different locations/momenta in different universes, and the above does not make that clear. I think this is a notational deficiency... Hmm. Needs more work.

Follows from the Hopf algebra structure on tensor algebras.

4.5 Laplacians and Energy

Where do Laplacians come from? Why is there a quadratic differential operator everywhere? There are two clues I want to assemble here: the Berezin integral, and Kirchoff's theorem for a graph. The story goes as follows: Start with a Clifford algebra, and build the spin group for even N for it. Factor the spin group into Weyl spinors. These naturally anti-commute, by construction. Throw away the spin aspects of this construction, but keep the anti-commuting aspects. This leads to the Berezin integral:

$$\det A = \int \exp\left[-\theta A \eta\right] d\eta d\theta$$

for Grassmann variables η and θ and an ordinary matrix A. This is abridged standard textbook stuff.

Next, Kirchoff's theorem for a graph states, more or less, that, when one has a graph, and one constructs the Laplacian matrix of the graph (by taking a difference of its adjacency matrix, and a diagonal degree matrix), then the determinant of that matrix is given by a sum over all spanning trees of the graph. Inverting this argument: if you have a determinant, then, by change of basis, try to turn it into the Laplacian of some graph.

This seems to do two things: It tells you that, to first order, the interaction of N particles really is a tree, has to be a tree, or rather, a sum of trees. Next, it tells you that, from the graph-theoretic point of view, it has to be the Laplacian that accomplishes this. But, from physics, we know that Laplacians are associated with kinetic energy.

Thus, it seems that, by merely assuming the Clifford algebra, (or assuming the spin algebra), we are forced to conclude that the Laplacian, i.e. kinetic energy, describes the situation. This, to me is really remarkable. I've never heard of this before. To deduce the necessity of Laplacians from the existence of Clifford algebras? Wow.

There is another way to get to Laplacians. One standard textbook approach is to assume the existence of a Riemannian manifold, build the tangent space, then point out that the exp function generates geodesics on the manifold (maps tangent vectors to geodesics), and then observe that geodesics can be obtained by solving for the extremum of either the length or the energy of smooth, differentiable paths on the manifold. The one and the other construction give a Lagrangian or a Hamiltonian point of view, and both end up requiring a second-order differential operator applied to the path. Thus, geodesics on Riemannian manifold necessarily involve a second-order differential operator.

We can try to apply this argument in reverse: Assume a Clifford algebra, deduce the existence of a Laplacian, and then deduce that a Riemannian manifold is the necessary home for the resulting dynamics. In other words, by assuming N indistinguishable fermions, we are forced to deduce Riemannian geometry.

If we start with the spinc group instead of the spin group, is there some way that this forces you to deduce pseudo-Riemannian geometry? The reason I think this is a plausible question is because the extra $U(1)$ degree of freedom in the spinc group might force one into using a (massless) gauge boson when one attempts to write the Berezin integral. (I mean, that degree of freedom has to go somewhere .. where else can it possibly go?) If that is the case, then it seems that the Maxwell equations are forced on us, whether we like it or not, and then the Maxwell equations necessarily imply special relativity, which necessarily imply a pseudo-Riemannian manifold.

The other things that this construction is doing is to force the use of the partition function. That is, the Berezin integral is forcing us to take a functional-integral description of the situation. And more strongly: not just any functional integral, but necessarily over something that involves a second-order differential operator ... one that is summed over. Namely, the action. And we already know that we can view quantum mechanics as the necessary outcome or by-product of employing functional integrals over the action.

A third interesting thing gets entangled here: why is space-time 3+1 dimensional? The hand-waving here is that spinors are necessarily objects that inhabit $SL(2,\mathbb{C})$ viz, the Lorentz group, and this leads to the adjoint rep being 3+1.

If this can be made rigorous, then this is a direct path between what are normally viewed as very distant topics. We start with a rather weak assumption – Clifford algebras, and get a rainbow of physics paraphernalia falling out, including **both** a pseudo-Riemannian geometry **and also** quantum mechanics! This is just too much, its ludicrous beyond belief!

The determinant: but why? The above started talking about the determinant out of thin air. Why?

But why Laplacian, really? The determinant could have been written as a product of eigenvalues, so why would one want to write the matrix perversely as a Laplacian? I presume that it is something about the spin factorization that is forcing this. More precisely, a graph Laplacian L can be factored as a pair of oriented edge incidence matrices:

$$L = EE^T$$

where E has a +1 at the head of an arrow (oriented edge) and a -1 at the tail. Presumably, it is something about the spinor factorization that causes us to talk about edges

(perhaps the edges connect the two components of the spinor?)

I mean, in a certain sense, it does not make sense to work with Laplacians if one does not have some ambient space-time; at the time we start talking about it, the ambient space-time does not yet exist.

What about the Standard Model? We know that in the end, we need SU(3)×SU(2)×U(1) bit trying to for this at this point seems unnatural. So lets not go there.

What about gravity? The goal was to discover something about gravity, but we've done no such thing in this section. ... on the other hand, until we work out the details in the above, we also have no clue whether the above forces a metric of any kind, i.e. if space-time become rigid, or if it remains completely floppy and metric-less.

4.6 Indeterminate particle number

In QFT, the number of particles is indeterminate – i.e. there are virtual particle pairs. Why? What does this have to do with the above discussions? The above construction started with a Clifford algebra over a fixed, finite-dimensional vector space. Does the limit $N \to \infty$ somehow make the particle count indeterminate? How?

4.7 Screech-halt

The core problem with the above arguments is that they take little tippy-toe steps into the territory of string theory, which is not where this was supposed to go, but seems to be edging towards, none-the-less. Its not at all the full-on orthodox string theory of 26-dimensional bosonic strings, and its not the even quantum deformations and affine Lie algebras. But its starting to wander willy-nilly in that general direction-ish. Ugh. Oh well.

FWIW, the above also starts sounding a little bit like a super-duper toy version of Urs Schreiber's superpoint. I have no clue if there's anything to that.

5 Other work & References

There is lots and lots of speculation of the above sort that is available here: https://fqxi.org/community – the FQXi community website. Most of it is more focused, more developed, and probably better argued. Maybe.