The Origins of Quantum Mechanics

Linas Vepstas

updated 10 March 2018 updated 3 Nov 2020

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 and principles that seem like they are true or should be true. These are interspresed with speculations: some fairly wild ideas that seem to embody the postulates in some way.

The postulates attempt to be as truthful, accurate and physical as possible, matching reality as we know it. The speculations are unbound, other than that they should be vaugely plausible. The speculations seemed like coherent ideas when I wrote them, but this is not guaranteee. Even if they make sense, they may be wrong or just bad. Note also: I change my mind as I go along; the later me might disavow the bad ideas the earlier me wrote. 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; this is an exploration of uncharted territory.

"*How can you be in two places at once when you're nowhere at all?*" – Firesign Theatre

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:

Speculation: Mass, sigma models, harmonic functions and ergodic orbits

Consider a map $f : M \to N$ between Riemannian manifolds (M, γ) and (N, g). In physics, this is just the generalized sigma model. The energy of the map is

$$E(f) = \frac{1}{2} g_{ij} \gamma^{\alpha\beta} \partial_\alpha f^i \partial_\beta f^j$$

This is just a generalization of the Dirichlet energy, appropriate for sigma models. The "tension field" $\tau(f)$ is defined as

$$\tau(f) = \text{trace} \nabla df$$

and the Euler-Lagrange equations for the energy E are $\tau(f) = 0$. A map is called "harmonic" if it satisfies the Euler-Lagrange equations.

There are two simple examples of such harmonic maps. First, if $N = \mathbb{R}$ then the Euler-Lagrange eqns reduce to $\Delta f = 0$ where Δ is the Laplace-Beltrami operator on M. Such an f is a harmonic function, and this is why, for general N, such maps deserve to be called "harmonic". Another example sets $M = S^1$ the circle. In this case, the Euler-Lagrange equations reduce to the equations for closed geodesics on N. All of the above is just standard textbook stuff.

Now we make the speclative leap: harmonic functions correspond (more or less) to massless fields, and closed geodesics correspond to periodic (non-ergodic) orbits. Periodic orbits are countable (in general), and are a set of measure zero. Perhaps mass corresponds to ergodic orbits?

How could this work out? We would need to find some M and N with a countable collection of harmonic maps. We would need to look at the nearby functions f with non-vanishing tension. To be interpreted as mass, we want find linear variations, that is, those for which $\tau(f) = \lambda f$ which allows λ to be interpreted as the field mass. What's the spectrum? Is it continuous? discrete? What does this all look like if M and/or N are spin manifolds?

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.

Translation surfaces

These are flat surfaces with non-trivial genus. They look like Riemann surfaces, flattened out, so that all of the curvature has been forced into a finite number of singularities.

I am assuming that a similar construction is possible for dimension greater than two.

Can these singularities be interpreted as physical particles in flat space?

What would the "physics" of such a space be? Obviously, it suggests that the geodesics would be wildly out of control, so that seems to imply its surely unphysical...

Metaplectic group, Metaplectic structure

The metaplectic group is a double-cover of the symplectic group. Via the Heisenberg group... For each and every $\hbar$ there is a distinct representation of the Heisenberg group acts on the Hilbert space of square-integrable funs. The action is to shift the position and the momentum, plus an addtional phase ... It's $\psi(x) \mapsto e^{i\hbar c} e^{ib\cdot x} \psi(x + \hbar a)$ where $b = \overrightarrow{b}$ and $a = \overrightarrow{a}$ vectors. This is the Stone-von Neumann theorem.

I'm confused... if I follow a geodesic on a riemannian manifold, then is it not the case that, if I stay on the same coordinate chart, then the the two points, in the cotangent bundle, are given by a symplectic matrix? That seems true, cause how could it not be?

Symplectic manifolds are special cases of Poisson manifolds... does the metaplectic structure also carry over to Poisson manifolds?

Shift and Clock matrices

See wikipedia https://en.wikipedia.org/wiki/Generalizations_of_Pauli_matrices which describes a construction from Weyl: it builds Σ_1 as a shift operator, and Σ_3 as a clock operator and then the Sylvester matrix, as a basis for $\mathfrak{gl}(n, \mathbb{C})$ and then the $n \to \infty$ limit as the algebra of Poission brackets (presumably a quantum group).

So of course, the shift opeators are that thing you applied to a Bernoulli process i.e. to anything with a product topology that you want to shift. By Ornstein siomorphism, everything is isomorphic to some Bernoulli scheme. This is what we get via ergodic theory.

So, what happens if we take a measure-preserving dynamical system, via a totally standard defintion, and add a clock matrix to it? What does that do? Yikes!

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.

Lounesto spinor field classification

If I am understanding this correctly, the Ohanian paper, in saying that the Dirac field is carrying angular momentum, is actually saying that $\overline{\psi}\gamma_u\gamma_v\psi$ is carrying angular momentum. Apparently, this quantity vanishes for a Weyl spinor field; we conclude that Weyl spinors on thier own cannot carry angular momentum! Huh! That certainly isn't obvious!

Mass

If we square the Atiyah-Singer-Dirac operator on a spin manifold, we get the Laplacian plus the scalar curvature. So should we interpret mass as the square of the scalar curvature? But mass is also the quadratic Casimir invariant of the Lorentz group. Soooo...????

Then, the Yamabe problem ...

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

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

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

Other assorted ideas about wave-function collapse.

4.4.1 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.

4.4.2 Wave function collapse is entropy

Arbitrarily small deformations of certain compact 3D manifolds can preserve the volume but increase the metric entropy and the topological entropy. (metric entropy w.r.t. the Liouville measure). Livio Flamino, Local entropy rigidity for hyperbolic manifolds Comm. Anal. Geom., 3 (1995), 555–596.

So, gravity at the plank-mass level, "flea's egg" level, tweaks the spacetime manifold in such a way that entropy increases, geodesic flows separate or dettach, leading to wave-function collapse.

4.5 Many-worlds, Measurement, Wave-fuction Collapse

Some basic formulations and framework. Abandoned construction. What is described below is interesting, but was ultimately founded on a hidden misconception. The intitial idea was that somehow, an entangled state could be decomposed into a pair of spin-1/2 states in some non-local entanglement. Then with enough geometric sophistication, ... everything would ... get resolved. This fails due to a key misperception. The maximally-entangled Bell states require not one, but two pairs of entangled spinors. For a while it feels like this can be avoideed, evaded, but not so. So this attempt at a geometric attack fails, because the entangled system cannot be described by a single dynamical, geometrical point. Well, it can, but they are glued together in such a way that a measurement singles out one of the two; this is the collapse, and the below fails to elucidate how that singling-out-one-of-two prodcess works. Still, its a ... curious tour of geometry.

Let M be the spacetime manifold (a Riemannian manifold) and also Φ, the field space. Consider a map $\phi : M \to \Phi$. Setting $\Phi = \mathbb{C}$ one gets the interpretation that $\phi = \psi(x)$ is a QM wave-function .

Take M to be flat, and let ∇^2 be the conventional notation for Laplacian in flat space, the grand-total kinetic energy of a wave function is

$$\mathscr{H}(\psi) = \frac{1}{2}\int_M \psi^\dagger(x)\nabla^2\psi(x)\,dx^m = \left\langle \psi \left| \nabla^2 \right| \psi \right\rangle$$

This is obviously the total kinetic energy of the wave-function ψ, up to a factor of $\hbar/m$.

This is also the Lagrangian for the sigma model! (See wikipedia article, which I wrote.) It can be re-written as

$$\mathscr{H}(\psi) = \frac{1}{2}\int_M \left(\nabla\psi^\dagger(x)\right)\left(\nabla\psi(x)\right)dx^m = \frac{1}{2}\int_M d\phi\wedge *d\phi$$

and the $\nabla\psi(x) = d\phi$ is both a vielbein and also a solder form; it solders together the cotangent space T^*M to $T^*\Phi$, keeping in mind that the cotangent spaces are where the momenta live that define the geodesic flows on the manifolds. I mention this because it seems significant...

Cool, but incomplete. Several problems. First, $\psi : M \to \mathbb{C}$ has no geometry, it is a scalar field. By convention, we set $\langle\psi|\psi\rangle = 1$ and so $|\psi\rangle \in U(1)$. This is acceptable, but is kind-of misleading. First, there are no true scalars in nature. Second, it isn't obvious that if there were, that there woulldn't be anything that is preventing the time evolution $|\psi\rangle \to 0$ (*i.e.* to have evolution of $\langle x|\psi\rangle = \psi(x) \to 0$ everywhere, *i.e.* for all x). There's no reason for a scalar wave function not to be homotopic to zero, because $\mathbb{C}$ is contractible and homotopic to a point, but $U(1)$ is not.

How, exactly, did we sneak in something non-contractable into something that is contractable? For scalar fields, this seems to be a fiat, *ad hoc* assumption. Can we do better for spinors? We know that lepton number is conserved. What I don't have is any description of a spinor as a topological soliton. I believe that such a description exists; I've never seen one in a textbook. One of the goals here is to provide this description explicitly. If it is a topological solition, then there is an argument that one can have time evoltion that $\psi(x) \to 0$ locally, for some x but not for all x.

For a spinor, we can set $\Phi = \mathbb{C}\times\mathbb{CP}^1$ so that $\mathbb{CP}^1$ encodes the spin direction, and $\mathbb{C}$ encodes the overall magnitude, and the phase-slop for the spin pointing in the z direction. What is $|\psi\rangle$? Should we write $|\psi\rangle \in \mathbb{CP}^1$ or should we write $|\psi\rangle \in U(1)\times\mathbb{CP}^1$? This is again a bit subtle. The $\mathbb{CP}^1$ is enough to account for the spin direction, but we still need to have an extra phase if we are going to write a normal two-component Weyl spinor. This extra phase "doesn't matter" in the sense that $\langle\psi|\psi\rangle = 1$ so it is easy to just "throw it away". But the Aharonov-Bohm effect, and also the neutron interferometer in gravity both require the holonomy of the circle bundle for the proper explanation, so we can't just throw the phase away.

Neither $U(1)$ nor $\mathbb{CP}^1$ are homotopically contractible to a point. If the spinor is a soliton, is it a solition in just $\mathbb{CP}^1$ or do we need it to be a solition in $U(1)\times\mathbb{CP}^1$? Which BTW, has the same number of dimensions as $S^3 \cong SU(2)$ so maybe that means something...

XXX TODO. We need an appendix that provides explict maps between all the various different notations e.g. how to write a two-component spinor from a coordinate chart on $\mathbb{CP}^1$ and so on.

What is the formal mathematical formalism for saying this? I guess that if we do this corectly, then we have some homology-like statement. We need two, actually, one that says that $H_{something}\left(\psi : M \to \mathbb{C}\times\mathbb{CP}^1\right) = U(1)\times\mathbb{CP}^1$ because somehow, the fiber of the fiber bundle gets the origin removed, and another that says $H_{other}\left(\psi : M \to \mathbb{C}\times\mathbb{CP}^1\right) = \mathbb{Z}$ because its the topological winding number (the lepton number, where the generator of $\mathbb{Z}$ is a single spinor). That's what we need to get after integrating $\psi : M \to \mathbb{C}$ over the manifold M. Despite reading about differential topology and algebraic topology, I have not spotted this, yet... am I being dumb?

In other words, the wave function on spacetime is actually a section of a fiber bundle with a non-trivial topology, and $U(1)\times\mathbb{CP}^1$ is the invariant of that bundle. (For scalar fields, we drop the $\mathbb{CP}^1$.) A spinor is then some manifold invariant of the global bundle. What is this invariant? How to articulate this formally? Do we need to use spin manifolds to do this?

What about scalars? What is the correct way to get $\langle\psi|\psi\rangle = 1$ automatically, instead of declaring it to be the case, by fiat? I mean, is it not the case that any fiber bundle $E \to M$ with fiber $\mathbb{C}$ is always contractible to a point? Or is that not the case? If it's contractible, then how do we get $\langle\psi|\psi\rangle = 1$? If it's not contractible, then why isn't this in introductory textbooks, e.g. in Bott&Tu? Some key ingredient is missing...

For the n-particle system, it is not enough to set $\Phi = \mathbb{C}^n$ as that results in all particles having the same location. The n-particle model requires using $\phi : M\times\cdots\times M \to \mathbb{C}^n$ in order to get $\psi(x_1, x_2, \cdots, x_n)$ as the wave-function. Can the multiple M's be interpreted as the "many worlds"? Meh. ...

This is confusing. The confusion can be heightened.

Set $n = 2$ and prepare ψ to be a singlet state. Locally, (local section) this has the form of $\psi : M\times M \to \mathbb{C}^2$ with $(x_1, x_2) \mapsto \psi(x_1, x_2)$. Globally, its a point in $\mathbb{CP}^2$ ... and we can use the four maxinmal Bell states as the basis for $\mathbb{CP}^2$. XXX TODO this all needs to go into an appendix. This will help clarify reasoning.

Consider a pair of projections $\pi_1\otimes\pi_2 : \mathbb{CP}^2 \to \mathbb{CP}^1\otimes\mathbb{CP}^1$ that split up the entangled state $\phi = \psi(x_1, x_2)$ into a pair of states $\psi_1(x_1)$ and $\psi_2(x_2)$. This projection is meant to obey all the usual algebra for an entangled vector state decomposed into two half-spin doublets (The Bell basis, Wikipedia [[Measurement in quantum mechanics]]). Of course, the projections π_1 and π_2 depend on the local qubit (Bloch sphere) coordinates chosen by experimentors at locations x_1 and x_2. That is, in conventional QM notation, $\pi_i = |\uparrow iL\rangle\langle\uparrow iL| \oplus |\downarrow iL\rangle\langle\downarrow iL|$ where each component $|\uparrow iL\rangle\langle\uparrow iL|$ is with regard to some local coordinate frame L on the Bloch sphere at the point x_i for experimenter i.

What we seem to be doing here is that we are not using a truly flat, locally trivial basis for the charts of the atlas, but rather, we are using charts with some kind of torsion in them. The states $|\uparrow iL\rangle$ are either torsion forms of some kind or they are vielbeins of some kind, or maybe both...

The projection π_i is neither a measurement, nor is it a measuring device...

A single measurement is the application of the projection operator in such a way that only *one* eigenstate results, and not a linear combination thereof. That is, one gets either $|\uparrow iL\rangle$ or one gets $|\downarrow iL\rangle$ but never both; one never gets the phases; the phases are erased. This is the "collapse". Put another way: if we take $\phi(x)$ to be a single point in the fiber $\mathbb{CP}^2$, the measurement does *not* map it to a single point, but to a random choice of one of two points.

That is, the projection is not a map of points. It can only be a map of densities.

From the point of view of the total bundle $\phi : M \times M \to \mathbb{CP}^2$, the ϕ is still entangled; there was no "wave function collapse". From the point of view of experimenter in one of the two spaces M, a collapse did happen. That is, the experimenter in M_1 applied π_1 to get $\pi_1(\phi)$ which is "collapsed" from his point of view.

Hypothesis: The measurement drives the "entangled" point ϕ to a specific location consistent with the measurement outcome. This is not a violation of Bell's theorem, since it is a non-local (not-so-)"hidden" variable. The perceived randomness is entirely due to the ergodicity of orbits on the manifold.

Additional point of confusion: The sphere S^2 viewed as a Bloch sphere, has a 3D Killing vector algebra. I guess this is just a fancy way of saying $S^2 \cong S^3/U(1)$ the Hopf fibration. So if a point in S^2 is a spin direction, then what is the $U(1)$? Is this the wave-function phase? Yes, that is the natural innterpretation when writing spinor coordinates on the Bloch sphere. It doesn't make sense for this to be "some other $U(1)$". Is this the same $U(1)$ as electrognetism? It seems that this is necessary, since Aharonov-Bohm shows that the phase is real, and its the holonomy, so we seem forced in this situation to conclude that this extra phase is also electromagnetism. The Hopf fibration is non-trivial ... This is begging for a revisit to the sigma model.

Interpreting the $U(1)$ as electromagnetism is problematic for neutrinos. The crazy idea would be to double-down on the bet – namely, while gauging $U(1)$ we get a vector field ... can we interpret it as being a vector in some symmetric space, with an algebra of Killing vectors greater than 4, so that some of the isometries belong to a center, and that center is what "interacts" with the neutrino, while the remaining 4 isometries are the standard electromagnetic potential? That would allow two spinors, both unified, one feeling a guage force i.e. electric charge, and one neutral... Hmmm So .. algebraically, what are the candidates for this symmetric space?

After this "measurement" in the M_1 world, the wave function needs to continue to evolve in its "collapsed" form $\pi_1(\phi)$ even though in Φ nothing has changed at all. This is a hypothesis (unproved theorem): we need to demonstrate that the dynamics of ϕ is consistent with the dynamics of $\pi_1(\phi)$ ... that is we need to prove the theorem that

$$\begin{array}{ccc} \phi & \overset{time}{\longrightarrow} & \phi' \\ \downarrow & & \downarrow \\ \pi_i(\phi) & \overset{time}{\longrightarrow} & \pi_i(\phi') = (\pi_i(\phi))' \end{array}$$

is actually a commuting diagram; it is not obvious that it is, however much I would hope it is... If this can be proven, then there "is no wave-function collapse." (A proof of this suggest the Myers-Steenrod theorem: every isometry of connected riemann manifolds is in fact smooth)

Lets stick to the sigma model, so time evolution is just the kinetic term, so non-interacting. Lets interpret time as geodesic flow. The geodesic flow is fiber-by-fiber, but since each fiber is identical to all the others, they all evolve "the same way". What is different is that the selected points are different. So, for the top row, we have $\phi(x_1, x_2)$ is different for each different $x1, x2$ and so is travelling along a different geodesic. So we need .. what? Jacobi fields for neighboring fibers. How does this work? We need a connection, a horizontal bundle on each fiber.

The only problem with this picture is that we don't have a tangent for ϕ anywhere... we don't know which geodesic ϕ is supposed to travel on.

The problem is, of course, there is also a diffferent world M_2 where the π_2 measurement was made.

Status: big fail. See second appendix. Above uses flawed perception in thinking that the the two particle-state can be decomposed into a pair of particle states – it cannot. The Bell states cannot be decomposed in this way; they require two pairs of two-particle states to be written correctly. And measurement isolates one of these two pairs. This is the fundamental stumbling block ever since the first days of quantum. The fancy language above does not impact it.

TODO: write down, explicitly, the extended π_i as a projection of $M \times M$

TODO: Introduce time. Is time the geodesic flow on M?

TODO: Introduce spinors i.e. make M into a spin manifold.

Note $\Phi = \mathbb{CP}^2$ is the complex projective plane, its non-trivial, (unlike $\mathbb{CP}^1 \cong S^2$) and there's a K2-surface in there, etc.

TODO: I don't believe in many-worlds. So then, how do we get back to just one M?

This is kind of a "replica trick". The replica trick was originally used in spin glass theory, but was eventually replaced by something else... how does that work?

4.6 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)$\times$SU(2)$\times$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.

4.7 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?

4.8 Planck scale

Quote: "from the Planck-scale induced gravitational modifications of the wave particle duality to the low energy realm of quantum mechanics where the de Broglie's wave particle duality holds to a great accuracy (Hackermueller et al., 2003). For C 60 F 48 molecule, the experiment finds fringe visibility lower than expected. It may be indicative that the modification to the de Broglie wave particle duality may become significant at a much lower energy." where Hackermueller et al., 2003 is ...???

4.9 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 TODO - Other work & References

A to-do list for ruther reading and thought.

5.1 FQXi

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.

5.2 Aharonov

The latest & greatest from Aharonov:

Yakir Aharonov and Daniel Rohrlich What is nonlocal in counterfactual quantum communication?, Physical Review Letters, arxiv.org/abs/2011.11667 has a concept of a "modular angular momentum" carried by a particle, but that it dettaches from the particle. Its non-local. Shades of de Broglie–Bohm theory. Whoa. So that is another plausible idea.

A Appendix: Notation Conventions

Some of the speculations above seem to actually require more precise notational conventions. Write these down, so as to avoid screwups.

A.1 The two sphere

Recurring theme $S^2 \cong \mathbb{CP}^1$ is the Bloch sphere, consisting of linear combinations of $|\uparrow\rangle = |0\rangle$ and $|\downarrow\rangle = |1\rangle$ and so $|\psi\rangle = \alpha|\uparrow\rangle + \beta|\downarrow\rangle$ for $\alpha, \beta \in \mathbb{C}$, so that is a total of four real numbers. Next $\langle\psi|\psi\rangle = 1$ so $\alpha^*\alpha + \beta^*\beta = 1$ leaves three real numbers. Phase ambiguity implies only two real numbers to get a point on the Bloch sphere. The third number, the phase, is physically real, in that it shows up in Bohm-Aharonov, and cannot be swept away; it also implies that it must be the $U(1)$ of electromagnetism.

The goal here is to interpret $|\psi\rangle$ as a pure state, and specifically, a "point" on a single fiber of some fiber bundle. That is, it is just marking a spin direction for one point in spacetime. Given a spacetime point x, the fiber above x is $\psi(x) = \langle x|\psi\rangle$ which has a real magnitude $p(x) = \|\psi(x)\|^2 = |\psi^*(x)\,\psi(x)| < 1$.

The word "point" here is ambiguous. There are several possible interpretations. The classical interpretation is to associate $|\psi\rangle$ as a single point on the Bloch sphere. (I think this is the mental model most physicists use). Another interpretation is to associate $|\psi\rangle$ with Killing fields on the Bloch sphere. We'll be using the second interpretation; this is required to overcome technical problems relating to Clebsh–Gordan coeficients for the two-particle state.

At this time, no physical assumptions about mixed states are being made; in particular, the density matrix is NOT assumed! This is because I want to leave open the door to a more general concept of mixed states as arbitrary probability distributions on the surface of the Bloch sphere, rather than as points in it's interior, as standard QM density matrix formalism requires.

Per Wikipedia, a single point on S^2 can be paramterized by Hopf coordinates:

$$\alpha = e^{i\varphi} \cos \eta \quad \text{and} \quad \beta = e^{i\varphi} e^{i\xi} \sin \eta$$

with $0 \leq \eta \leq \pi/2$: attention, this is NOT the usual sphere coord, its off by a factor of two! So $\eta = 0$ corresponds to $|\uparrow\rangle$ and $\eta = \pi/2$ corresponds to $|\downarrow\rangle$. The longitude is $0 \leq \xi \leq 2\pi$. The phase is $e^{i\varphi}$.

The two-sphere has three Killing vector fields. So, basicaly, treat S^2 as a symmetric space. The Killing field algebra $\mathfrak{g} \cong \mathfrak{su}(2)$ splits into

$$\mathfrak{g} = \mathfrak{m} \oplus \mathfrak{h}$$

where $\mathfrak{m} \cong TS^2$ and $\mathfrak{h} \cong \mathfrak{u}(1)$ is the stabilizer subalgebra.

I assume that this is equivalent to the Hopf fibration, but this needs to be double-checked, with explicit expressions given.

Per the "hairy ball theorem", vector fields on S^2 cannot be combed, which is why the stabilizer $\mathfrak{h}$ is not vacant. Picking a single point on S^2 corresponds to picking a Killing field that is vanishing at that point. The other two are not, they are tangent to T_pS^2 where $p = |\psi\rangle$ is the point on the Bloch sphere.

The biggest problem with using Killing fields to represent a point is that there will also be a second spot somewhere on the sphere where a given Killing field must vanish. This problem can be avoided by noting that a given fixed Killing field is just one representative out of an infinite set of such fields. Infinite set because, at each point on the spehre, we are free to choose any coordinate frame we like; the Killing fields can be any wild, crazy combing of hair, as long as we recognize thier algebra is the algebra of the isometries, i.e. $\mathfrak{su}(2)$. That is, let $p = |\psi\rangle$ correspond to the coset of all Killing fields that vanish at p.

A pure state is meant to be represented by a single point on the Bloch sphere. The manipulation of a pure state, e.g. by rotating it via some device, move the point on the Bloch sphere. The phase is interpreted as residing in the $\mathfrak{h}$ part, That is, $\varphi \in \mathfrak{h}$ where φ is the φ from above, so that $e^{i\varphi} \in U(1) \cong S^1$ is the overall phase. Thus, one expects holonomy effects to arise if the spin is moved about a closed path. TODO: provide explcit expressions for the holonomy.

A.2 The two-spin system

The goal here is to represent an entangled state of two qubits. Naively, this is an element in $\mathbb{C}^4$, which we will decompose into spinors, and apply normalization constraints. This follows the usual algebra.

Here's the tricky part: this is not a fiber over a single spacetime; it has two spatial coordinates, one for each spinor. So if M is the spacetime manifold, the two-spin system is a fiber over the base $M \times M$. It has two spatial positions.

So the general two-particle state will be

$$|\psi_A\rangle \otimes |\psi_B\rangle = (\alpha_A |\uparrow_A\rangle + \beta_A |\downarrow_A\rangle) \otimes (\alpha_B |\uparrow_B\rangle + \beta_B |\downarrow_B\rangle)$$

Note the tensor product makes it bilinear. Expanding this gives

$$|\psi_A\rangle \otimes |\psi_B\rangle = \alpha_A \alpha_B |\uparrow_A \uparrow_B\rangle + \alpha_A \beta_B |\uparrow_A \downarrow_B\rangle + \beta_A \alpha_B |\downarrow_A \uparrow_B\rangle + \beta_A \beta_B |\downarrow_A \downarrow_B\rangle$$

The primary issue with the above expression is that a two-particle state cannot be determined by a pair of points on the Bloch sphere. For example, Bell states are the maximally entangled states. These are, in conventional notation:

$$\begin{aligned}
\left|\Phi^+\right\rangle &= \frac{1}{\sqrt{2}} (|\uparrow\uparrow\rangle + |\downarrow\downarrow\rangle) \\
\left|\Phi^-\right\rangle &= \frac{1}{\sqrt{2}} (|\uparrow\uparrow\rangle - |\downarrow\downarrow\rangle) \\
\left|\Psi^+\right\rangle &= \frac{1}{\sqrt{2}} (|\uparrow\downarrow\rangle + |\downarrow\uparrow\rangle) \\
\left|\Psi^-\right\rangle &= \frac{1}{\sqrt{2}} (|\uparrow\downarrow\rangle - |\downarrow\uparrow\rangle)
\end{aligned}$$

and there is no choice of α, β that can express the above just a single pair of spin-1/2 states. So for example, to get $|\Phi^+\rangle$ set $\alpha_A = \alpha_B = 1$ and $\beta_A = -\beta_B = i$ to get

$$\begin{aligned}
|\psi\, 1\, i\rangle &= (|\uparrow_A\rangle + i\,|\downarrow_A\rangle) \otimes (|\uparrow_B\rangle - i\,|\downarrow_B\rangle) \\
&= |\uparrow\uparrow\rangle - i\,|\uparrow\downarrow\rangle + i\,|\downarrow\uparrow\rangle + |\downarrow\downarrow\rangle
\end{aligned}$$

and also

$$\begin{aligned}
|\psi\, 1\, -i\rangle &= (|\uparrow_A\rangle - i\,|\downarrow_A\rangle) \otimes (|\uparrow_B\rangle + i\,|\downarrow_B\rangle) \\
&= |\uparrow\uparrow\rangle + i\,|\uparrow\downarrow\rangle - i\,|\downarrow\uparrow\rangle + |\downarrow\downarrow\rangle
\end{aligned}$$

in order to get

$$\left|\Phi^+\right\rangle = |\psi\, 1\, i\rangle + |\psi\, 1\, -i\rangle$$

up to normalization. Thus, at least two pairs of points on S^2 are needed! One pair is not enough! This is the root cause of "wave function collapse"; a measurement of a Bell state discards one of these two possibilities.

OK, well, that was interesting ... "the precise meaning of an old proverb."

That is, we need to work with states $|j\, m\rangle$ with $j = 1$ and $m = -1, 0, +1$ and thus a generic two-particle state is

$$|\psi_{AB}\rangle = \zeta\, |j = 1\; m = -1\rangle + \eta\, |j = 1\; m = 0\rangle + \theta\, |j = 1\; m = +1\rangle$$

requires three complex numbers ζ, η, θ in $\mathbb{C}^6$. (Requiring normaliztion knocks this down, etc.) The point here is that a generic $|\psi_{AB}\rangle$ cannot be written as a single pair of points on the Bloch sphere; it requires two pairs of points.

The end.