Marcus Berg, 31 augusti 2018

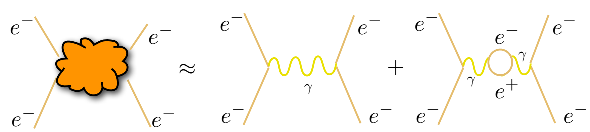
The quantum measurement "problem"

1 Two-page review of quantum measurement

Here are a few key points that are hopefully familiar to you:
• Quantum mechanics applies to everything, even big objects. (My undergraduate textbook had an example of a person walking through a door: compute the interference pattern.[1])
• $i\hbar\,\partial\psi/\partial t = H\psi$.
• The wavefunction ψ is itself not physical, in particular it is complex. "The Born rule" (Max Born) says that expanding in a basis $\psi = \sum c_i\psi_i$ where each state i corresponds to a well-defined value of a specific observable (like "spin along z is up"), the probability for measuring state i is $P_i = |c_i|^2$.
• In wave mechanics (Schrödinger) we describe a measurement by the strange but convenient instantaneous collapse of the wavefunction ψ to ψ_i, as shown for example on PhET. But neither ψ nor ψ_i is physical, so this collapse must be some idealization of an actual measurement process.

Perhaps less familiar, but also basic, are:
• *Decoherence* is when disturbance from an external system make parts of a wave that were initially in phase become unrelated from each other. This happens in wave physics in general, the only quantum thing is that particles are waves. For example, an interference pattern on water can be "fragile".
• *Entanglement* is a specific example of superposition of states when you have several particles. This has no classical counterpart, when there is no superposition of states. (I have notes on this too.)

The above concepts are all taught in *nonrelativistic* quantum mechanics."Quantum-ness" is not emphasized in basic courses and textbooks in relativistic quantum field theory. In fact when physicists talk about the grand principles of the 20th century, they mention quantum mechanics and relativity, not "quantum field theory and relativity".[2] In quantum field theory, the nontrivial quantum stuff happens in the interaction region (cloud in the figure below):

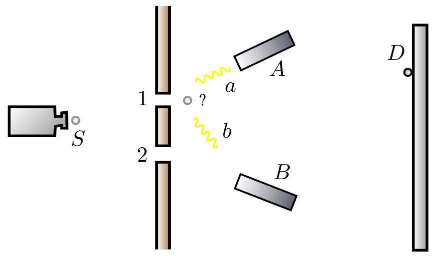


The two Feynman diagrams represent two distinct processes. The corresponding amplitudes are *added* before computing the cross section, so alternatives can interfere. Do they both happen, then?

In fact, Feynman originally developed his formalism in nonrelativistic quantum mechanics. So a simpler example is double slit: not even interaction, just interference of noninteracting paths. (See the notes on functional integrals, Appendix A Polchinski, or Feyman's popular book on QED [9]!). Here's an example where we use additional detectors A and B to detect the path, with a probability amplitude b to mix up the photon from the area around hole 1 with a photon from the area around hole 2. (We used this example in our high school physics text *Gymnasiefysik Fysik 3*):

[1] I still remember thinking about this at the time and finding it fascinating but unreasonable-sounding. There should be interactions with the environment. In fact this is relevant to the measurement discussion. See also the section on "dirt".

[2] In reality there were many challenges to make explicit relativistic aspects of quantum mechanics (like the Unruh effect from the 1970s), and there are still some aspects of textbook quantum field theory that have a nonrelativistic flavor, like using equal-time commutators when they could have used the Peierls bracket. In any case, I think it is fair to say that the aspects of quantum mechanics that are usally regarded as most difficult to grasp, like about measurement, one does not understand any better (or worse) after having a taken a typical course in quantum field theory.



so when $|b|$ is sufficiently small compared to $|a|$, we obtain "which-way" information, and the interference pattern is destroyed. More specifically, in the standard formalism,

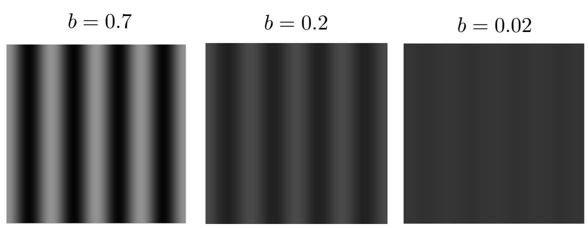
$$\langle \text{electron at } D, \text{photon at } A|S\rangle = \langle D, A|S\rangle = \langle D|2\rangle\, b\, \langle 2|S\rangle + \langle D|1\rangle\, a\, \langle 1|S\rangle \tag{1}$$

$$\langle D, B|S\rangle = \langle D|2\rangle\, a\, \langle 2|S\rangle + \langle D|1\rangle\, b\, \langle 1|S\rangle \tag{2}$$

then with $\langle D|1\rangle = A_1 e^{i\phi_1}$, $\langle D|2\rangle = A_2 e^{i\phi_2}$ we have the (unnormalized) probability

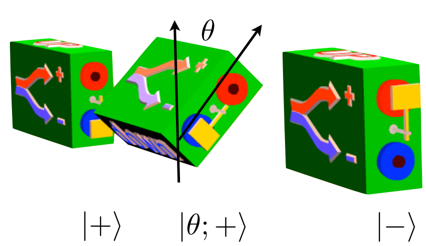
$$P = |\langle D, A|S\rangle|^2 + |\langle D, B|S\rangle|^2 = (a^2 + b^2)(A_1^2 + A_2^2) + 4abA_1A_2\cos(\phi_1 - \phi_2) \tag{3}$$

which we can plot for decreasing b, meaning more and more efficient which-way detectors A and B:[3]



Feynman says this double slit experiment is *"the heart of quantum mechanics. In reality, it contains the only mystery."* The mathematics above is like in ordinary wave physics except when when $b \to 0$, it becomes indistinguishable from classical particles bouncing through two slits in classical mechanics: no interference. As Coleman argues in his lecture "Quantum Mechanics In Your Face", we should say that classical mechanics is *defined* as the $b \to 0$ limit. It is only because we are so familiar with classical mechanics that we secretly think of it as a separately existing theory. By the way, in 2013 there was a nice experimental implementation where they move a cover over the slits [3].

Once Feynman starts to calculate he uses the Stern-Gerlach as the basic example instead of double-slit, mainly because the key Stern-Gerlach information is discrete (so does Sakurai, Schwinger, etc):



This picture is from the PhET Stern-Gerlach app, that is quite instructive to play around with: can you predict what percentage of spins coming in from the left make it through all the way to emerge from the uncovered blue hole on the right, as a function of axis rotation angle θ? (More sample problems in *Gymnasiefysik Fysik 3*.) Schwinger in his quantum mechanics course at Harvard used polarized light to illustrate this feature of Stern-Gerlach [8]. Then the mathematics is identical to classical optics as covered in a course on wave physics[4] but the key change in the physics is that particles pass through the experiment one by one as opposed to the many-particle waves of classical optics.

[3]In Mathematica, with $a = 1$, $A_1 = 1$, $A_2 = 1$, $\phi_1 = 0$: `DensityPlot[P/10 /. b -> 0.7, {ϕ2, -4 π, 4 π}, {x, 0, 1}, ColorFunction -> GrayLevel, ColorFunctionScaling -> False, PlotPoints -> 100]`

[4]like Schwartz's wave course, the Schwartz that has a quantum field theory textbook

2 Is there a measurement "problem"?

Feynman wrote the following haiku: "*I'm an old enough man that I haven't got to the point that this stuff is obvious to me. ... every new idea, it takes a generation or two until it becomes obvious that there's no real problem ... I cannot define the real problem, therefore I suspect there's no real problem, but I'm not sure there's no real problem.*"

I don't think the measurement "problem" is really a problem in the sense of something that needs to be solved. Many people (e.g. Jürgen Fuchs!) have the same feeling, but there are some physicists who disagree and believe there is a problem to solve, like 't Hooft, Gell-Mann[5] and Weinberg. The latter writes: *"My own conclusion (not universally shared) is that today there is no interpretation of quantum mechanics that does not have serious flaws, and that we ought to take seriously the possibility of finding some more satisfactory other theory, to which quantum mechanics is merely a good approximation."* [5]

These people mostly express such views when they get older (which of course does not rule out they already held these views earlier). One example of someone who expressed the views when still young is Brian Greene [4]. He expressed it in his popular science books, not his research papers.[6] To me, the interpretation of quantum mechanics is a little like the cosmological constant problem: most physicists with some interest in cosmology seem to think about it at various points, but most of the time one makes no progress, so there is nothing to write.[7]

If I don't think there is a problem to solve, why write this text? Put simply, if people like Weinberg say there is a problem with one of our main physical theories, it behooves us all to try to understand what the problem might be, and then decide for ourselves. Like other texts of this kind, its main purpose is not to "teach" details of the measurement problem, but to provide a relatively quick summary that helps you pick one or more of the readable references I give here, and read it. (And discuss it with me, if you want!)

3 Interpretations of quantum mechanics

There is a list of "interpretations of quantum mechanics" on Wikipedia. But unlike most Wikipedia lists, its main usefulness lies in illustrating that it is not very useful: neither the rows nor the columns are very well-defined. In general, Wikipedia pages about foundations of quantum mechanics seem to be written mostly by non-experts, for example reporting details about *votes* at some conference about which interpretation people prefer. The original texts cited here seem much preferable.

3.1 Copenhagen

This is the standard interpretation: the wavefunction or state vector $|\psi\rangle$ is not a real, measurable object, but measurements probe some aspect of the wavefunction, such as an eigenvalue c_i for some eigenstate $|i\rangle$, and probabilities for measuring this property is given as $P = |c_i|^2$, the "Born rule". Some people say Copenhagen is the *absence* of an interpretation and needs to be "completed", but in the next subsection I summarize some arguments that it is not. The idea that standard quantum mechanics is incomplete in this sense probably comes from three sources:

a) Textbooks of quantum mechanics tend to focus on formalism and not reproduce discussions of interpretations actually given by original workers on quantum mechanics (Bohr, Heisenberg, etc.).

[5] Many people have suggested that perhaps the famous Gell-Mann vs. Feynman rivalry might have made it seem like more of a difference of opinions that it is.

[6] The observations in this paragraph (old established people talk about it, young people only write about it in non-professional communication) has led some people to believe there is some kind of conspiracy in physics, that young researchers are not allowed to talk about the foundations of quantum mechanics. If the physics community has rules about that, I was never informed!

[7] Incidentally, my feeling is that more physicists consider the cosmological constant problem a real problem than there are who consider the quantum measurement problem a real problem, but I have no numbers to back up this feeling. In both cases there certainly exist people who do not consider the problems to be problems.

b) Original work was not expressed very clearly by today's standards, in particular Niels Bohr was a great physicist but not a model of clarity. But when he had to be clear he was ultimately clear.
c) Some of the issues that lead to the current discussions (see below) had not arisen yet in the lifetime of Bohr and Heisenberg, so it is up to the generations after them to apply their lessons to these new situations. I wouldn't call that "new interpretation", more of a "new application".

An example of a concise original summary with some perspective is Max Born's nobel lecture, where he cites Bohr. An example of a Bohr paper is Bohr-Rosenfeld 1933 [6], about how to generalize fixed-time field values to spacetime (see e.g. Hartle's discussion of it [7]).

3.2 Many-Worlds Interpretation (MWI)

The many-worlds interpretation of quantum mechanics was supposedly invented by Everett in 1957 and developed by Bryce DeWitt in the 1960s and 1970s [18]. The most compact summetry is given at Lubos's blog:
"*What they mean when they credit Everett are the following principles:*
1. Quantum mechanics applies to all objects in Nature, including the large ones
(such as apparatuses and human beings)
2. The measurement is preceded by interactions that produce quantum entanglement between the observer and the observed system
3. The collapse associated with the measurement isn't an objectively real process
(let alone one that could influence other parts of Nature superluminally)
And I haven't even mentioned a more complex idea, Bohr's complementarity, which we will look at later. Now, it's ironic that Everett ever gets any credit for those points because all of them were invented by the "Copenhagen school"."

Bryce DeWitt wrote in 1970 that "*the mathematical formalism of the quantum theory is capable of yielding its own interpretation* " [18].[8] To me, that amounts to saying that many-worlds is logically already contained in the original formulation of quantum mechanics, just that the originators of quantum mechanics didn't spell out these things it in detail as much as Bryce tried to do (see below). If I am allowed this rewording, then ordinary quantum mechanics does not need any new construct to be complete, which in my mind would mean there is no measurement problem. This agrees with Lubos's statement above, but probably disagrees with most people talking about many-worlds today. (They are of course entitled to their interpretation of Bryce's statements, but I am entitled to not be too interested in their interpretations once I arrived at the feeling that there is no problem to solve.)

What was the detail that Bryce DeWitt tried to spell out in his interpretation? People discussing this did not seem to have read what he actually says about it in his quantum field theory book from 2003 [17], where he writes *"[The many-worlds interpretation] has been adopted by the author out of practical necessity because he knows of no other. At least he knows of no other that imposes no artificial limitations or fuzzy metaphysics while remaining able to serve the varied needs of quantum cosmology, mesoscopic quantum physics, and the looming discipline of quantum computation."*. In the book, nothing seems too surprising: the measurement process is represented by an apparatus just like for Bohr and Heisenberg, and each apparatus is only aware of one world at a time, and the "splitting into two worlds" happens smoothly during an interval of coupling between the apparatus and the measured system. This makes the "splitting of worlds" seem down-to-earth, and indeed Bryce emphasises that people get too worked up by the choice of words: *"if the words offend then choose others. Better still, let the formalism speak for itself"*.[9] There is also a useful discussion of imperfect measurements (p.159). So in my mind, it

[8] He gave a collquium in Austin specifically on this, so I know he didn't change his mind in later years, although he might have refined it as we see in the book quotes below.

[9] Somewhat unfortunately, given the above discussion, Bryce also says that although he uses the measurement theory of Bohr and Rosenfeld from 1933, he believes that those authors would have "strongly repudiated" his use of it. But if Bohr had been confronted with the "varied needs" Bryce is trying to adapt the formalism to, all of which essentially developed much later, perhaps Bohr would have been more supportive? Now all those people have passed away including Bryce himself, so we will never know. Incidentally, because of the "strongly repudiated" remark it might seem disingenous of me to claim that Bryce's interpretation is essentially the Copenhagen interpretation, but to me that is just the "MWI follows from the

is acceptable to take the measurement theory in this book and view it as a modern version of the Copenhagen interpretation, where (just like the Copenhagen people said) wavefunction collapse is not a real, physical process. About the Born rule, DeWitt writes, "*Although the terminology of probability theory is now being used, the worlds themselves have no probabilistic antecedents. They are defined neither in terms of an a priori metaphysics nor in terms of the mathematical properties of the state vector, but in terms of factual physical properties of the system in the state that the state vector represents. However, once terminology of probability theory has been introduced there need be no hesitation in using it in exactly the same way as it is used in the standard probability calculus.*"

3.3 Other interpretations: it doesn't get any easier!

"Against philosophy"...is the title of a chapter in Weinberg's book "Dreams of a final theory". It argues that focusing too literally on things that can be measured can be misleading: the wavefunction is not "observable" in ordinary quantum mechanics, but it is used to compute observable quantities, which is a good enough argument for physicists to use it. In particular Heisenberg seems to have liked positivism but later gave up its literal interpretation.[10]

One example of the many interpretations (other than Copenhagen and many-worlds) is the von Neumann-Wigner interpretation that "consciousness causes collapse", a very fuzzy-sounding idea that the otherwise very precise Wigner seems to have disavowed in later years[11]. I would tend to agree with Bryce that "fuzzy metaphysics" is to be avoided in physics whenever we can.

One final comment about "improving" quantum mechanics: it doesn't get any easier! Some people (like Weinberg, in the quote above) express hope that a new theory replacing quantum mechanics would be "simpler" conceptually, in particular it should be more like classical physics. But don't get your hopes up: Weinberg is not saying you shouldn't learn the usual formalism of quantum mechanics (wave functions, the Dirac bra-ket formalism, etc.). After all, the Weinberg quote is from his book about the usual formalism! The usual formalism describes all existing measurements in microscopic physics, so if it is ever subsumed by a better theory, that theory should have quantum mechanics as a special case. The hypothetical new theory might have some new conceptual simplicity, but it will presumably be *more* difficult calculationally, not easier. Einstein's mechanics in special relativity has Newtonian mechanics as a special case. Einstein's theory has very satisfying *conceptual* simplicity, but $E = \sqrt{m^2c^4 + p^2c^2}$ is a more complicated formula than $E = \frac{1}{2}mv^2$, so trying to solve questions in slow-moving mechanics using relativity usually makes it unnecessarily difficult, which is why we usually don't. In the same spirit, hoping that a new theory would avoid the Dirac bra-ket formalism or wave functions entirely seems naive; at most, the interpretations could become more direct somehow, but adherents would still need to be able to use the usual formalism. I like the usual formalism!

4 The DeWitt metric

Bryce DeWitt was a very serious physicist who wrote very little popular science. But some of his research does have some of the flair of popular science. In his way of talking about things, even a concept as technical as the space of field histories can stimulate deep thoughts: every point of this space represents a possible history for all of spacetime, including the metric field, and all fields in it (hence also all particles that are excitations of those fields) from beginning to end. By the way, this also includes spacetimes that may be infinite in either direction, like presumably our universe that as far as we know will have no end in finite time. DeWitt was perhaps the first to discuss how "far away" one entire such history is from another entire history, i.e. introduce a *metric on the space of field*

formalism" statement.

[10]Heisenberg vs. positivism is analogous to when Einstein was so inspired by Mach early on in the formulation of relativity, but then realized that general relativity does not obey Mach's principle. Naturally, he got rid of Mach, not general relativity.

[11]You might argue, well von Neumann was a mathematician, surely he would be precise? But actually I find that some mathematicians, even ones who clearly know a lot of physics like von Neumann, tend to be very precise about their mathematics, but speculate more freely about the laws of physics than physicists, to the point where it gets too vague for my taste.

histories. The metric specifically on the space of spacetime metrics (that was actually introduced in the now-defunct Wheeler-DeWitt context mentioned below) is called the DeWitt metric [11].[12] The simplest instance is for small metric fluctuations, i.e. the usual graviton:

$$G_{g_{\mu\nu}(x)g_{\rho\sigma}(x')} = \sqrt{|\det g|}\,(g^{\mu\rho}g^{\nu\sigma} + g^{\mu\sigma}g^{\nu\rho} - g^{\mu\nu}g^{\rho\sigma})\delta(x,x') \quad (4)$$

with which distances in the *space of metric fields* are given by

$$ds^2 = \int d^4x\, d^4x' G_{g_{\mu\nu}(x)g_{\rho\sigma}(x')} dg_{\mu\nu}(x) dg_{\rho\sigma}(x')\ . \quad (5)$$

With this tool, one can try to give a number to questions like "how much are the usual laws of physics violated in one given universe", and then claim to compute that the more they are violated, the less likely that universe is to interact with ours. Personally I don't find this question very interesting in general, since it seems hard to imagine any way to ever test whether there are other universes with different laws of physics, especially if they are less likely to come into contact with us the more distinguishable they are from us (whatever that might mean!).

To prevent confusion: in standard cosmology, observers on planets in faraway galaxies see a different observable universe than we do. But this particular distinction between observable (to them) and unobservable (to us) is purely in the context of classical physics, because of the finiteness of the speed of light, and in standard cosmology there is no sense in which these distant regions of the universe are in a "superposition" with ours in any quantum-mechanical sense. In particular, in standard cosmology, the laws of physics are the *same* in those regions of the universe as in our observable universe.[13] The simplest instance of this is if light from stars formed in their region of the universe hasn't reached us. Of course we could just try to wait until that starlight reaches us, but since the expansion of the universe accelerates, some of that starlight may *never* reach us, providing an example of something that is presumably never observable to us but still not very exotic.[14] So let us keep issues of classical physics apart from the measurement problem.

4.1 Many-worlds vs. moduli space

More restrictive versions of the discussion of the DeWitt metric do seem interesting to me, and calculable. A more recent relative (offspring?) of the DeWitt metric is the moduli space metric of string compactifications. This means we make some symmetry assumptions to simplify things, for example use supersymmetry to reduce an infinite number of parameters (e.g. arbitrary metrics) to a finite number of parameters (e.g. moduli of Calabi-Yau metrics), use those symmetries to find a classical metric on moduli space, then compute quantum corrections to the metric. One could then study dynamics on that quantum-corrected moduli space. Metastable points of that dynamics are semiclassical solutions of string theory, which are now collectively referred to as the "string landscape". So far, I have not said anything about quantum measurement; there is a theory, and it has many solutions.

(For reference, it is not surprising that a comprehensive theory should have many solutions: we can easily write a theory of particle physics beyond the Standard Model with an *infinite* number of physically inequivalent versions. Take $SU(N)$ gauge theory for any N. Having a *finite* number of solutions is what is surprising, even if it is only for one corner of string theory landscape. In the original discussions the restriction that leads to a finite number of solutions is the tadpole condition, that is more restrictive in string theory than in typical field theory. In the simplest example, N of $SU(N)$ should be 32 or smaller by the tadpole condition. I tend to think of in string theory as having

[12] This reference is usually given, e.g. in [20], but it is somewhat confusing since that paper (Part I) is about the 3+1 theory, and this is covariant. In the 2003 book, it is given as the Jacobi field operator.

[13] Here I exclude "eternal inflation" that some might consider part of "standard cosmology".

[14] Strominger has an interesting comment about this in [10], that the Green's function for a scalar field in de Sitter space has two solution, one of which has a singularity at a spacetime point itself, and another that connects two antipodal points that are separated from each other by a particle horizon. The comment is (p.13) " as we try to understand the quantum theory of de Sitter space [these Green's functions] will surely turn out to have some purpose in life"

to do with quantum gravity, in the sense that they can be translated to solving equations in the closed-string or gravitational-like sector, that one usually ignores in quantum field theory in flat space.)

Anyway, to me the basic idea of moduli space dynamics is reasonably down-to-earth and does not seem to require new development of the foundations of quantum mechanics: I can imagine staying in a fixed universe, or at most a brief semiclassical motion between neighboring such spaces, i.e. ascribe some level of realism to each step of evolution of the universe, much like the standard observationally-supported ΛCDM cosmology is a solution of Einstein's equation with quantum fields and particles in it. These fixed hypothetical "universes" would each have different laws of physics (analogously to different values of the parameters of a ΛCDM-like cosmology), which should ultimately lead to distinct predictions for experiments (like the parameters of ΛCDM cosmology have been narrowed down to some intervals by experiment and observation), so they cannot all describe our particular universe. In other words, the other "universes" don't really deserve to be called universes: they are called "false vacua" in the above link. They were just theoretical hypotheses that I discard as the theory develops to make clearer predictions, and as new kinds of experimental data becomes available. I am happy to talk about the string landscape in this restricted sense, and there seem to be many questions one can address without necessarily having to confront any measurement problem.

5 Many-worlds vs. multiverse

The "string multiverse" is then the idea that in some sense, the points in the landscape (that represent different laws of physics) would all actually coexist in some grand quantum-mechanical superposition. This does sound somewhat like the many-worlds interpretation, although to me they seem to be distinct ideas. In particular the extravagant postulate that all universes somehow coexist does not seem to in itself address any measurement problem. (Confusingly, the string landscape in the above sense is sometimes conflated with the string multiverse in this sense. People who do so give reasons for this conflation, but let me keep them distinct here: in the string landscape I am allowed to focus on one solution at a time, whereas in the string multiverse people imagine a superposition of solutions.)

Incidentally, there have been some ideas about testing "multiverse" theories by for example looking for "bubble collisions" in the cosmic microwave background, where some other expanding universe (bubble) leaves some trace of "colliding" with ours [16]. As far as I know, noone has found any such traces and people thinking about that seem to agree that even if there were such bubbles, it is not clear or likely they should have left any observable trace. I guess if you want to think about that, you would have to decide something about whether those other "worlds" should be thought of in the many-worlds sense as some superpositions with our universe and that they might have different laws of physics, or just in the classical cosmology sense, that they are just distant regions of our universe that we can no longer access, just as future astronomers on Earth in an accelerating universe will no longer be able to see some distant galaxies that we see today. In the absence of any evidence of bubble collisions, I am not sure this is urgent to resolve. (If bubble-people retort that my questions aren't urgent to solve either, well, we can't agree on everything.)

6 Quantum cosmology

One of the first attempts at a quantum theory of the entire universe was the Wheeler-DeWitt equation, first published by Bryce in 1963 under the name of Einstein-Schrödinger equation [11]. When I was in Austin, Bryce referred to it as "that damned equation". I think it was because he felt the whole discussion at the time to be horribly out of date, and led the discussion in the wrong direction. In particular he became more focused on Lagrangian formalisms, partly to address time more on the same footing as space, unlike in Hamiltonian formalisms where time is distinguished.

In any case, quantum cosmology is useful to think about even if you are not terribly interested in cosmology, since it forces you to think about the question of what happens in quantum mechanics if no humans are around to make measurements, a question that must have an answer, and one that was a reason Bryce was led to the many-worlds interpretation. One lucid author in this field is Jim Hartle, who wrote a nice review of it that he recently made available [7]. Another is Ashtekar, as in Ashtekar variables. Our own Claes Uggla worked with Ashtekar on quantization methods in cosmology [19].

7 Quantum mechanics is local

People talk of quantum nonlocality as proven by the Bell experiments. But "quantum nonlocality" is a confusing combination of words: here it means that if a *classical* theory were to represent what is measured in a Bell experiment, that classical theory would have to be nonlocal. Quantum mechanics in the usual sense is *local*: the Schrödinger equation is a local differential equation, i.e. only refers to a specific point x, and quantum field theories in the Standard Model only refer to a specific spacetime point x.

In Polchinski's book, he expresses the view that it is good that string theory has at least a little bit of nonlocality, because it might help resolve the black hole information paradox. (Note that this would have been difficult to understand if you had the impression that quantum mechanics is non-local to begin with.) This is because the theory on the string worldsheet is local on the worldsheet, but because the string is extended in spacetime, interactions are not strictly local in spacetime, although this effect becomes vanishingly small if strings are vanishingly small. (In fact many popular accounts of string theory credit the fact that interactions are not localized at a point with the result that some string interactions are "automatically" free of short-distance divergences.) The small nonlocality of textbook string theory should not be confused with the huge nonlocality a classical theory would need to have to reproduce Bell experiments.

8 Entropy

The Wikipedia page for wavefunction collapse says (August 2018) *"collapse is merely a black box for thermodynamically irreversible interaction with a classical environment"*. To the extent that this statement makes sense, the density matrix is a useful concept in quantum statistical mechanics.[15]

It seems clear that coarse-graining increases entropy — less is known about a system after averaging procedures. And some interpretations of quantum mechanics, like consistent histories, talk about coarse-graining. But again, in my mind they add additional layers of interpretation that are not needed in quantum mechanics. It is true that the measurement process is in a sense irreversible. Then one should perhaps say it increases entropy. But there are some papers on quantum *pasts*, by Hartle for example [12].

9 Chaos

Feynman in his lectures points out the analogy between the uncertainty of quantum mechanics and chaos in the sense that specification of initial conditions with less-than-infinite precision (as is in principle always done, although we often don't explicitly incorporate this in our equations) leads to unpredictable behavior is some systems but not others. In particular there is no chaos in the harmonic oscillator (linear ODE), but there is chaos in the oscillator that is not precisely harmonic (e.g. has additional quadratic term in ODE, even if coefficient is very small). But this leads to an interesting apparent clash: quantum mechanics is inherently linear and chaos is inherently nonlinear. Quantum chaos is an active area of research, with many serious researchers like Srednicki.[16]

[15] As discussed in my text on functional integrals, it can be confusing to mix the words "quantum" and "statistical", but here I have something simple and specific in mind, the theory of density matrices.

[16] Bryce gave a course on quantum chaos in Austin, but I didn't attend (perhaps from exhaustion after his course on quantum field theory in curved spacetime).

10 Mesoscopic "dirt"

One time at Cornell I told Henry Tye I don't understand why people say that flash memory (like a memory stick) is quantum-mechanical. I thought of quantum physics in pristine, clean laboratories, often with cryogenic equipment to reduce thermal fluctuations, whereas a memory stick seems like a dirty, cheap mesoscopic system at room temperature, that should experience strong thermal and decoherence effects, so it should be essentially classical. Henry explained to me that some aspects of mesoscopic quantum physics are robust against being dirty. I later heard similar sentiments from cond-mat people like Robert Laughlin, who emphasized the extreme precision in quantum Hall measurements:

$$\frac{h}{e^2} = 25812.807557(18)\ \Omega$$

with 11 significant figures (the parenthesis are the experimentally uncertain digits), despite the environment sometimes being very complicated and very different across different experimental setups. Laughlin writes [15]: "*emergent exactness, growing out of the uncertain, probabilistic statistical natural of particles, may be the most important emergence of all*". This is supposed to illustrate that if you really want to understand a quantum system, you should be explicit about what measurement you want to make (hence the title of this text). In particular, a system "being quantum" is not a well-defined concept. Part of "being quantum" should certainly be that measured energies are quantized (the origin of the word "quantum"), and this quantumness is visible already in the undergraduate experiment of line spectra from heated gases, at room temperature. This measurement does not depend on detailed phase information in some wavefunction.

To be clear, "dirt" is not specific to mesoscopic physics, for example the line spectra can be contaminated by admixtures of other gases. In general, if you are trying to study system A, having another system B nearby might affect system A in some way. Only if system B is either "small" (has negligible effects on A in general) or "weakly coupled" to system A (e.g. would have big effects if close, but is sufficiently far away) can we ignore its effects on system A. So particle physics is not "immune to dirt" either, but particle experimentalists work hard to clean things up: there are sufficiently few other particles present in the interaction region in the detectors at CERN (it is cooled down[17], and the vaccuum is among the best on Earth) that we can neglect them completely.

But many systems of interest in materials science are of interest because of their potential applications, so in particular in mesoscopic physics, system that can *handle* a little bit of "dirt", i.e. is "robust", may be of greater interest. For example, external effects like decoherence is one of the main challenges facing people trying to build bigger quantum computers, where phase correlations are often important. Topological quantum computation is an attempt to make them more robust.

Finally, "more dirt" when comparing mesoscopic physics to the microscopic world of particle physics; certainly our big macroscopic everyday world is much dirtier than the clean rooms of materials science. Getting lots of actual dirt in your memory stick is probably not a good idea.

11 Popular science

Sean Carroll thought a lot about the direction of time: From Eternity to Here maybe also his new book "The Big Picture". Carroll writes entertainingly but I don't find the arguments very deep.

On the other hand I only know of one top-level researcher who writes popular science well, and that's Steven Weinberg (nobel prize winner in Austin): Dreams of a Final Theory and the more recent Third Thoughts. Much of his writing is factual and down-to-earth, so occasionally avoids the deep issues.

Brian Greene is also fairly down to earth in Fabric of the Cosmos, but less so in Hidden Reality [4], that I haven't read all the way through for that reason.

[17] One of the reminisciences about Fermi's original Chicago cyclotron states that it was more fun when you could just turn on an accelerator and didn't have to wait a week for it to cool down. This was a long time ago!

Lubos blog might be the best current source for clear texts about the measurement problem[18], but the level varies quickly between popular and technical, like this blog entry. Another more popular-level blog is mentioned below.

12 Future theories: Holography

At some point people said holographic duality (e.g. AdS/CFT in string theory) would clarify quantum mechanics, since there is a limit where one side of the duality is classical, and the other is quantum, and they are equivalent according to the duality. But at infinite N (in $SU(N)$, the 't Hooft limit) the theory is perhaps too simple to be of direct conceptual interest, in particular interesting quantum corrections like β functions vanish in maximally supersymmetric Yang-Mills theory by symmetries (i.e. by assumption). Before we take $N \to \infty$, both sides of the duality receive corrections, so neither is completely classical. There is a lot of work on computing entanglement entropy, like the Ryu-Takayanagi proposal.

13 Future theories: ER=EPR or GR = QM

A related idea to holography helping with the measurement problem is that black holes and similar classical aspects of general relativity might help. Susskind had a conjecture with Maldacena called ER=EPR, conjecturing that quantum entanglement like in Einstein-Podolsky-Rosen can be described by classical connection by wormhole solutions like that of Einstein-Rosen, i.e. the conjecture is just to erase Podolsky [14]. Later Susskind went all the way and stated that in fact GR = QM [13]. Clearly he did this to be controversial, but if literally true, that should presumably resolve the measurement problem, since there isn't one in classical GR? Apparently now Aaronson is working with Susskind, as the latter refers to unpublished work in [1]. In the abstract to that paper, Susskind writes *"It is not known what the limitations are on using quantum computation to speed up classical computation. It is also not known what the limitations are on the duration of time over which classical general relativity can describe the interior geometry of black holes. What is known is that these two questions are closely connected: the longer GR can describe black holes, the more limited are quantum computers."*. This certainly sounds like fun.

14 Future theories: Quantum computing

You would think quantum-computing people would be at the forefront of these questions. Some of them are indeed working at the forefront, but I haven't noticed a lot of fruitful interaction with high energy theorists, until maybe recently [2]. One of the founders of quantum computing is Deutsch, who also co-wrote an eulogy of Bryce DeWitt. Deutsch has written some books that I haven't read. When I did read some of his arguments I didn't find them too compelling, but DeWitt gives Deutsch a lot of credit (Chapter 9 ends "This chapter depends heavily on the work of David Deutsch", but refers to mostly unpublished discussions). Aaronson writes: *Why does David Deutsch (one of the originators of QC) think that a scalable quantum computer would be a powerful demonstration of the truth of the many-worlds interpretation? What are the counterarguments to Deutsch's position?* Aaronson gives some nice simple arguments on his blog. But these arguments also show why a physicist might be put off by some quantum-computing discussions: he says "double blech" about physics-style arguments!

Referenser

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[18] If you read about foundations of quantum mechanics on Lubos's blog it may be useful to point out that in recent years, his opinion changed to be less supportive of consistent histories and more "pure Copenhagen".

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