



Libraries Automation Service WWW Server

Subject: Measurement in quantum mechanics FAQ

Newsgroups: [sci.physics](#) , [alt.sci.physics.new-theories](#) , [sci.answers](#) , [alt.answers](#) , [news.answers](#)

Posted-By: auto-faq 3.1.1.2

Archive-name: physics-faq/measurement-in-qm

Last-modified: Mon Jul 31 21:58:55 1995

This is a FAQ on physical and metaphysical approaches to the measurement problem in QM. Please help make it more complete. See 'What is needed' for details.

The existing sci.physics FAQ does a good job of dealing with technical questions in most areas of physics. However it has no material on interpretations of QM which are among the most frequently discussed topics in sci.physics. Perhaps in part because of that void a rogue 'FAQ' on the many worlds interpretation has appeared. That document is more a position paper that advocates the many worlds interpretation than an attempt at objectively answering questions about the interpretation. It contains serious factual errors. Thus there is a need for a document that provides a broader and more objective description of interpretations of QM and other approaches to the measurement problem.

This document is probably out of date if you are reading it more than 30 days after the date which appears in the header. The easiest way for most people to get a copy of any FAQ is by anonymous FTP or via e-mail server from rtfm.mit.edu. By FTP, look for the file

`/pub/usenet/news.answers/physics-faq/measurement-in-qm`

By e-mail send a message with a blank subject line and the words:

`send usenet/news.answers/physics-faq/measurement-in-qm`

The main sci.physics FAQ is in this same directory with file names part1 through part4 and can be retrieved in the same way. You can put multiple send lines in a single e-mail request.

This document, as a collection, is Copyright 1995 by Paul P. Budnik (paul@mtmath.com). The individual articles are Copyright 1995 by the individual authors listed. All rights are reserved. Permission to use, copy and distribute this unmodified document by any means and for any purpose EXCEPT PROFIT PURPOSES is hereby granted, provided that both the above Copyright notice and this permission notice appear in all copies of the FAQ itself. Reproducing this FAQ by any means, included, but not limited to, printing, copying existing prints, publishing by electronic or other means, implies full agreement to the above non-profit-use clause, unless upon explicit prior written permission of the authors.

This FAQ is provided by the authors "as is," with all its faults. Any express or implied warranties, including, but not limited to, any implied warranties of merchantability, accuracy, or fitness for any particular purpose, are disclaimed. If you use the information in this document, in any way, you do so at your own risk.

Contents

1. The measurement problem
2. Schrodinger's cat
3. The Copenhagen interpretation
4. The relative state (many worlds) interpretation
5. Is QM a complete theory?
6. The shut up and calculate interpretation
7. Bohm's theory
8. The Transactional Interpretation of Quantum Mechanics
9. Complex probabilities
10. Quantum logic
11. Consistent Histories
12. What is needed?
13. Is this a real FAQ?
14. Errors in the Many-worlds `FAQ'

 Item 1.

The measurement problem

 Paul Budnik (paul@mtnmath.com)

The formulation of QM describes the deterministic unitary evolution of a wave function. This wave function is never observed experimentally. The wave function allows us to compute the probability that certain macroscopic events will be observed. There are no events and no mechanism for creating events in the mathematical model. It is this dichotomy between the wave function model and observed macroscopic events that is the source of the interpretation issue in QM. In classical physics the mathematical model talks about the things we observe. In QM the mathematical model by itself never produces observations. We must **interpret** the wave function in order to relate it to experimental observations.

It is important to understand that this is not simply a philosophical question or a rhetorical debate. In QM one often must model systems as the superposition of two or more possible outcomes. Superpositions can produce interference effects and thus are experimentally distinguishable from mixed states. How does a superposition of different possibilities resolve itself into some particular observation? This question (also known as the measurement problem) affects how we analyze some experiments such as tests of Bell's inequality and may raise the question of interpretations from a philosophical debate to an experimentally testable question. So far there is no evidence that it makes any difference. The wave function evolves in such a way that there are no observable effects from macroscopic superpositions. It is only superposition of different possibilities at the microscopic level that leads to experimentally detectable interference effects.

Thus it would seem that there is no criteria for objective events and perhaps no need for such a criteria. However there is at least one small fly in the ointment. In analyzing a test of Bell's inequality one must make some determination as to when an observation was complete, i. e. could not be reversed. These experiments depend on the timing of macroscopic events. The natural assumption is to use classical thermodynamics to compute the probability that a macroscopic event can be reversed. This however implies that there is some **objective** process that produces the **particular** observation. Since no such objective process exists in current models this suggests that QM is an incomplete theory. This might be thought of as the Einstein interpretation of QM, i. e., that there are objective physical processes that create observations and we do not yet understand these processes. This is the view of the compiler of this document.

For more information:

Ed. J. Wheeler, W. Zurek, Quantum theory and measurement,
Princeton University Press, 1983.

J. S. Bell, Speakable and unspeakable in quantum mechanics, Cambridge
University Press, 1987.

R.I.G. Hughes, The Structure and Interpretation of Quantum
Mechanics, Harvard University Press, 1989.

Item 2.

Schrodinger's cat

Paul Budnik (paul@mtnmath.com)

In 1935 Schrodinger published an essay describing the conceptual
problems in QM[1]. A brief paragraph in this essay described the
cat paradox.

One can even set up quite ridiculous cases. A cat
is penned up in a steel chamber, along with the following
diabolical device (which must be secured against direct
interference by the cat): in a Geiger counter there is a
tiny bit of radioactive substance, so small that *perhaps*
in the course of one hour one of the atoms decays, but also,
with equal probability, perhaps none; if it happens, the
counter tube discharges and through a relay releases a hammer
which shatters a small flask of hydrocyanic acid. If one has
left this entire system to itself for an hour, one would
say that the cat still lives *if* meanwhile no atom has decayed.
The first atomic decay would have poisoned it. The Psi function
for the entire system would express this by having in it the
living and the dead cat (pardon the expression) mixed or
smeared out in equal parts.

It is typical of these cases that an indeterminacy originally
restricted to the atomic domain becomes transformed into
macroscopic indeterminacy, which can then be *resolved* by
direct observation. That prevents us from so naively accepting
as valid a "blurred model" for representing reality. In itself
it would not embody anything unclear or contradictory. There is
a difference between a shaky or out-of-focus photograph and a
snapshot of clouds and fog banks.

We know that superposition of possible outcomes must exist simultaneously
at a microscopic level because we can observe interference effects from these.
We know (at least most of us know) that the cat in the box is dead, alive or
dying and not in a smeared out state between the alternatives. When and how
does the model of many microscopic possibilities resolve itself into a
particular macroscopic state? When and how does the fog bank of microscopic
possibilities transform itself to the blurred picture we have of a definite
macroscopic state. That is the measurement problem and Schrodinger's
cat is a simple and elegant explanations of that problem.

References:

[1] E. Schrodinger, "Die gegenwartige Situation in der Quantenmechanik,"
Naturwissenschaften. 23 : pp. 807-812; 823-823, 8440849. (1935).
English translation: John D. Trimmer, Proceedings of the American
Philosophical Society, 124, 323-38 (1980),
Reprinted in Quantum Theory and Measurement, p 152 (1983).

Item 3.

The Copenhagen interpretation

Paul Budnik (paul@mtmath.com)

This is the oldest of the interpretations. It is based on Bohr's notion of 'complementarity'. Bohr felt that the classical and quantum mechanical models were two complementary ways of dealing with physics *both* of which were necessary. Bohr felt that an experimental observation collapsed or ruptured (his term) the wave function to make its future evolution consistent with what we observe experimentally. Bohr understood that there was no precise way to define the exact point at which collapse occurred. Any attempt to do so would yield a different theory rather than an interpretation of the existing theory. Nonetheless he felt it was connected to conscious observation as this was the ultimate criteria by which we know a specific observation has occurred.

References:

N. Bohr, The quantum postulate and recent the recent development of atomic theory, Nature, 121, 580-89 (1928),
Reprinted in Quantum Theory and Measurement, p 87, (1983).

Item 4.

The relative state (many worlds) interpretation

Paul Budnik (paul@mtmath.com)

The idea of the wave function collapsing has always been troubling to some physicists. Everett made the assumption that no collapse occurs and worked out the mathematical implications of this assumption. It is important to understand that the Everett interpretation does not explain macroscopic observations or solve the measurement problem. It assumes they exist and also assumes that the wave function does not collapse. These two assumptions lead to the relative state model. Instead of the existence of a single unique set of macroscopic observations there is an ill defined multiplicity of relative states that are also referred to as many worlds.

There is no relativistic version of Everett's interpretation. Since the relativistic version of quantum mechanics, quantum field theory. has no wave function it is not clear how a relativistic version could be developed.

References:

H. Everett. "Relative State" Formulation of Quantum Mechanics, Reviews of Modern Physics, 29 454-462, (1957)
Reprinted in Quantum Theory and Measurement, p. 315, (1987).

Item 5.

Is QM a complete theory?

Paul Budnik (paul@mtmath.com)

Einstein did not believe that God plays dice and thought a more complete theory would predict the actual outcome of experiments. He argued[1] that quantities that are conserved absolutely (such as momentum or energy) must correspond to some objective element of physical reality. Because QM does not model this he felt it must be incomplete.

It is possible that events are the result of objective physical processes that we do not yet understand. These processes may determine the actual outcome of experiments and not just their probabilities. Certainly that is the natural assumption to make. Any one who does not understand QM and many who have only a superficial understanding naturally think that observations come about from some objective physical process even if they think we can only predict probabilities.

There have been numerous attempts to develop such alternatives. These are often referred to as 'hidden variables' theories. Bell proved that such theories cannot deal with quantum entanglement without introducing explicitly nonlocal mechanisms[2]. Quantum entanglement refers to the way observations of two particles are correlated after the particles interact. It comes about because the conservation laws are exact but most observations are probabilistic. Nonlocal operations in hidden variables theories might not seem such a drawback since QM itself must use explicit nonlocal mechanism to deal with entanglement. However in QM the non-locality is in a wave function which most do not consider to be a physical entity. This makes the non-locality less offensive or at least easier to rationalize away.

It might seem that the tables have been turned on Einstein. The very argument he used in EPR to show QM must be incomplete requires that hidden variables models have explicit nonlocal operations. However it is experiments and not theoretical arguments that now must decide the issue. Although all experiments to date have produced results consistent with the predictions of QM, there is general agreement that the existing experiments are inconclusive[3]. There is no conclusive experimental confirmation of the nonlocal predictions of QM. If these experiments eventually confirm locality and not QM Einstein will be largely vindicated for exactly the reasons he gave in EPR. Final vindication will depend on the development of a more complete theory.

Most physicists (including Bell before his untimely death) believe QM is correct in predicting locality is violated. Why do they have so much more faith in the strange formalism of QM than in basic principles like locality or the notion that observations are produced by objective processes? I think the reason may be that they are viewing these problems in the wrong conceptual framework. The term 'hidden variables' suggests a theory of classical like particles with additional hidden variables. However quantum entanglement and the behavior of multi-particle systems strongly suggests that whatever underlies quantum effects it is nothing like classical particles. If that is so then any attempt to develop a more complete theory in this framework can only lead to frustration and failure. The fault may not be in classical principles like locality or determinism. Their failure may only be in the imagination of those who are convinced that no more complete theory is possible.

One alternative to classical particles is to think of observations as focal points in state space of nonlinear transformations of the wave function. Attractors in Chaos theory provide one model of processes like this. Perhaps there is an objective physical wave function and QM only models the average or statistical behavior of this wave function. Perhaps the structure of this physical wave function determines the probability that the wave function will transform nonlinearly at a particular location. If this is so then probability in QM combines two very different kinds of probabilities. The first is the probability associated with our state of ignorance about the detailed behavior of the physical wave function. The second is the probability that the physical wave function will transform with a particular focal point.

A model of this type might be able to explain existing experimental

results and still never violate locality. I have advocated a class of models of this type based on using a discretized finite difference equation rather than a continuous differential equation to model the wave function[4]. The nonlinearity that must be introduced to discretize the difference equation is a source of chaotic like behavior. In this model the enforcement of the conservation laws comes about through a process of converging to a stable state. Information that enforces these laws is stored holographic like over a wide region.

Most would agree that the best solution to the measurement problem would be a more complete theory. Where people part company is in their belief in whether such a thing is possible. All attempts to prove it impossible (starting with von Neumann[5]) have been shown to be flawed[6]. It is in part Bell's analysis of these proofs that led to his proof about locality in QM. Bell has transformed a significant part of this issue to one experimenters can address. If nature violates locality in the way QM predicts then a local deterministic theory of the kind Einstein was searching for is not possible. If QM is incorrect in making these predictions then a more accurate and more complete theory is a necessity. Such a theory is quite likely to account for events by an objective physical process.

References:

[1] A. Einstein, B. Podolsky and N. Rosen, Can quantum-mechanical descriptions of physical reality be considered complete?, *Physical Review*, 47, 777 (1935).
Reprinted in *Quantum Theory and Measurement*, p. 139, (1987).
[2] J. S. Bell, On the Einstein Podolosky Rosen Paradox, *Physics*, 1, 195-200 (1964).
Reprinted in *Quantum Theory and Measurement*, p. 403, (1987).
[3] P. G. Kwiat, P. H. Eberhard, A. M. Steinberg, and R. Y. Chiao, Proposal for a loophole-free Bell inequality experiment, *Physical Reviews A*, 49, 3209 (1994).
[4] P. Budnik, Developing a local deterministic theory to account for quantum mechanical effects, hep-th/9410153, (1995).
[5] J. von Neumann, *The Mathematical Foundations of Quantum Mechanics*, Princeton University Press, N. J., (1955).
[6] J. S. Bell, On the the problem of hidden variables in quantum mechanics, *Reviews of Modern Physics*, 38, 447-452, (1966).
Reprinted in *Quantum Theory and Measurement*, p. 397, (1987).

Item 6.

The shut up and calculate interpretation

Paul Budnik (paul@mtnmath.com)

This is the most popular of interpretations. It recognizes that the important content of QM is the mathematical models and the ability to apply those models to real experiments. As long as we understand the models and their application we do not need an interpretation.

Advocates of this position like to argue that the existing framework allows us to solve all real problems and that is all that is important. Franson's analysis of Aspect's experiment[1] shows this is not entirely true. Because there is no objective criteria in QM for determining when a measurement is complete (and hence irreversible) there is no objective criteria for measuring the delays in a test of Bell's inequality. If the demise of Schrodinger's cat may not be determined until someone looks in the box (see item 2) how are we to know when a measurement in tests of Bells inequality is irreversible and thus measure the critical timing in these experiments?

References:

[1] J. D. Franson, Bell's Theorem and delayed determinism, Physical Review D, 31, 2529-2532, (1985).

Item 7.

Bohm's theory

Paul Budnik (paul@mtmath.com)

Bohm's interpretation is an explicitly nonlocal mechanistic model. Just as Bohr saw the philosophical principle of complementarity as having broader implications than quantum mechanics Bohm saw a deep relationship between locality violation and the wholeness or unity of all that exists. Bohm was perhaps the first to truly understand the nonlocal nature of quantum mechanics. Bell acknowledged the importance of Bohm's work in helping develop Bell's ideas about locality in QM.

References:

D. Bohm, A suggested interpretation of quantum theory in terms of "hidden" variables I and II, Physical Review, 85, 155-93 (1952). Reprinted in Quantum Theory and Measurement, p. 369, (1987).

D. Bohm & B.J. Hiley, The Undivided Universe: an ontological interpretation of quantum theory (Routledge: London & New York, 1993).

Item 8.

The Transactional Interpretation of Quantum Mechanics

Lawrence R. Mead (lrmead@whale.st.usm.edu)

The transactional interpretation of quantum mechanics (J.G. Cramer, Phys. Rev. D 22, 362 (1980)) has received little attention over the one and one half decades since its conception. It is to be emphasized that, like the Many-Worlds and other interpretations, the transactional interpretation (TI) makes no new physical predictions; it merely reinterprets the physical content of the very same mathematical formalism as used in the "standard" textbooks, or by all other interpretations.

The following summarizes the TI. Consider a two-body system (there are no additional complications arising in the many-body case); the quantum mechanical object located at space-time point (R₁,T₁) and another with which it will interact at (R₂,T₂). A quantum mechanical process governed by E=hν, conservation laws, etc., occurs between the two in the following way.

- 1) The "emitter" (E) at (R₁,T₁) emits a retarded "offer wave" (OW) Ψ . This wave (or state vector) is an actual physical wave and not (as in the Copenhagen interpretation) just a "probability" wave.
- 2) The "absorber" (A) at (R₂,T₂) receives the OW and is stimulated to emit an *advanced* "echo" or "confirmation wave" (CW) proportional to Ψ at R₂ backward in time; the proportionality factor is Ψ^* (R₂,T₂).
- 3) The advanced wave which arrives at 'E' is $\Psi \Psi^*$ and is presumed to be the probability, P, that the transaction is complete (ie., that an interaction has taken place).
- 4) The exchange of OW's and CW's continues until a net exchange of energy and other conserved quantities occurs dictated by the quantum boundary conditions of the system, at which point the "transaction" is complete. In effect, a

standing wave in *space-time* is set up between 'E' and 'A', consistent with conservation of energy and momentum (and angular momentum). The formation of this superposition of advanced and retarded waves is the equivalent to the Copenhagen "collapse of the state vector". An observer perceives only the completed transaction, however, which he would interpret as a single, retarded wave (photon, for example) traveling from 'E' to 'A'.

Q1. When does the "collapse" occur?

A1. This is no longer a meaningful question. The quantum measurement process happens "when" the transaction (OW sent - CW received - standing wave formed with probability $\Psi \Psi^*$) is finished - and this happens over a *space-time* interval; thus, one cannot point to a time of collapse, only to an interval of collapse (consistent with relativity).

Q2. Wait a moment. What you are describing is time reversal invariant. But for a massive particle you have to use the Schrodinger equation and if Ψ is a solution (OW), then Ψ^* is not a solution. What gives?

A2. Remember that the CW must be time reversed, and in general must be relativistically invariant; ie., a solution of the Dirac equation. Now (eg., see Bjorken and Drell, Relativistic QM), the nonrelativistic limit of that is not just the Schrodinger equation, but two Schrodinger equations: the time forward equation satisfied by Ψ , and the time reversed Schrodinger equation (which has $i \rightarrow -i$) for which Ψ^* is the correct solution. Thus, Ψ^* is the correct CW for Ψ as the OW.

Q3. What about other objects in other places?

A3. The whole process is three dimensional (space). The retarded OW is sent in all spatial directions. Other objects receiving the OW are sending back their own CW advanced waves to 'E' also. Suppose the receivers are labeled 1 and 2, with corresponding energy changes E_1 and E_2 . Then the state vector of the system could be written as a superposition of waves in the standard fashion. In particular, two possible transactions could form: exchange of energy E_1 with probability $P_1 = \Psi_1 \Psi_1^*$, or E_2 with probability $P_2 = \Psi_2 \Psi_2^*$. Here, the conjugated waves are the advanced waves evaluated at the position of R_1 or R_2 respectively according to rule 3 above.

Q4. Involving as it does an entire space-time interval, isn't this a nonlocal "theory"?

A4. Yes, indeed; it was explicitly designed that way. As you know from Bell's theorem, no "theory" can agree with quantum mechanics unless it is nonlocal in character. In effect, the TI is a hidden variables theory as it postulates a *real* waves traveling in space-time.

Q5. What happens to OW's that are not "absorbed" ?

A5. Inasmuch as they do not stimulate a responsive CW, they just continue to travel onward until they do. This does not present any problems since in that case no energy or momentum or any other physical observable is transferred.

Q6. How about all of the standard measurement thought experimnts like the EPR, Schrodinger's cat, Wigner's friend, and Renninger's negative-result experiment?

A6. The interpretational difficulties with the latter three are due to the necessity of deciding when the Copenhagen state reduction occurs. As we saw above, in the TI there is no specific time when the transaction is complete. The EPR is a completeness argument requiring objective reality. The TI supplies this as well; the OW and CW are real waves, not waves of probability.

Q7. I am curious about more technical details. Can you give a further reference?

A7. If you understand the theory of "advanced" and "retarded" waves (out of electromagnetism and optics), many of the details of TI

calculations can be found in: Reviews of Modern Physics, Vol. 58, July 1986, pp. 647-687.

Item 9.

Complex probabilities

References;

Saul Youssef _Quantum Mechanics as Complex Probability Theory_ hep-th 9307019.
S. Youssef, Mod.Phys.Lett.A 28(1994)2571.

Item 10.

Quantum logic

References:

R.I.G. Hughes, _The Structure and Interpretation of Quantum Mechanics_, pp. 178-217, Harvard University Press, 1989.

Item 11.

Consistent Histories

Reference:

R. B. Griffiths, "Consistent Histories and the Interpretation of Quantum Mechanics", Journal of statistical Physics, 36(1/2):219-272(1984)

Item 12.

What is needed?

All comments suggested and contributions are welcome. We currently have nothing on Complex Probabilities and Quantum Logic. The entries on the following topics are minimal and should be replaced by complete articles.

- Copenhagen interpretation
- Relative State (Everett)
- Shut up and calculate
- Bohm's theory

Alternative views on any of the topics and suggestions for additional topics are welcome.

Item 13.

Is this a real FAQ?

Paul Budnik (paul@mtmath.com)

A FAQ is generally understood to be a reasonably objective set of answers to frequently asked questions in a news group. In cases where an issue is controversial the FAQ should include all credible opinions and/or the consensus view of the news group.

Establishing factual accuracy is not easy. No consensus is possible on interpretations of QM because many aspects of interpretations involve metaphysical questions. My intention is that this be an objective accurate FAQ that allows for the expression of all credible relevant

opinions. I did not call it a FAQ until I had significant feedback from the `sci.physics' group. I have responded to all criticism and have made some corrections. Nonetheless there have been a couple of complaints about this not being a real FAQ and there is one issue that has not been resolved.

If anyone thinks there are technical errors in the FAQ please say what you think the errors are. I will either fix the problem or try to reach on a consensus with the help of the `sci.physics' group about what is factually accurate. I do not feel this FAQ should be limited to noncontroversial issues. A FAQ on measurement in quantum mechanics should highlight and underscore the conceptual issues and problems in the theory.

The one area that has been discussed and not resolved is the status of locality in Everett's interpretation. Here is what I believe the facts are.

Eberhard proved that any theory that reproduces the predictions of QM is nonlocal[1]. This proof assumes contrafactual definiteness (CFD) or that one could have done a different experiment and have gotten a definite result. This assumption is widely used in statistical arguments. Here is what Eberhard means by nonlocal:

Let us consider two measuring apparatus located in two different places A and B. There is a knob *a* on apparatus A and a knob *b* on apparatus B. Since A and B are separated in space, it is natural to think what will happen at A is independent of the setting of knob *b* and *vice versa*. The principles of relativity seem to impose this point of view if the time at which the knobs are set and the time of the measurements are so close that, in the time laps, no light signal can travel from A to B and *vice versa*. Then, no signal can inform a measurement apparatus of what the knob setting on the other is. However, there are cases in which the predictions of quantum theory make that independence assumption impossible. If quantum theory is true, there are cases in which the results of the measurements A will depend on the setting of the knob *b* and/or the results of the measurements in B will depend on the setting of the knob *a*.[1]

It is logically possible to deny CFD and thus to avoid Eberhard's proof. This assumption can be made in Everett's interpretation. Everett's interpretation does not imply CFD is false and CFD can be assumed false in other interpretations. I do not think it is reasonable to deny CFD in some experiments and not others but that is a judgment call on which intelligent people can differ.

It is mathematically impossible to have a unitary relativistic wave function from which one can compute probabilities that will violate Bell's inequality. A unitary wave function does satisfy CFD and thus is subject to Eberhard's proof. This is a problem for some advocates of Everett who insist that only the wave function exists. There is no wave function consistent with both quantum mechanics and relativity and it is mathematically impossible to construct such a function. Quantum field theory requires a nonlocal and thus nonrelativistic state model. The predications of quantum field theory are the same in any frame of reference but the mechanisms that generate nonlocal effects must operate in an absolute frame of reference. Quantum uncertainty makes this seemingly paradoxical situation possible. There is a nonlocal effect but we cannot tell if the effect went from A to B or B to A because of quantum uncertainty. As a result the predictions are the same in any frame of reference but any mechanism that produces these predictions must be tied to an absolute frame of reference.

There is a certain Alice in Wonderland quality to arguments on these

issues. Many physicists claim that classical mathematics does not apply to some aspects of quantum mechanics, yet there is no other mathematics. The wave function model is a classical causal deterministic model. The computation of probabilities from that model is as well. The aspect of quantum mechanics that one can claim lies outside of classical mathematics is the interpretation of those probabilities. Most physicists believe these probabilities are irreducible, i. e., do not come from a more fundamental deterministic process the way probabilities do in classical physics. Because there is no mathematical theory of irreducible probabilities one can invent new metaphysics to interpret these probabilities and here is where the problems and confusion rest. Some physicists claim there is new metaphysics and within this metaphysics quantum mechanics is local.

References:

P. H. Eberhard, "Bell's Theorem without Hidden Variables",
Il Nuovo Cimento, V38 B 1, p 75, Mar 1977.

Item 14.

Errors in the Many-worlds `FAQ'

Paul Budnik (paul@mtmath.com)

The following refers to the periodic posting archived at `rtfm.mit.edu' as '/pub/usenet/news.answers/many-worlds-faq'. This is a list of the most egregious errors I found in reading parts of this document and scanning the whole mess. It is by no means an exhaustive list of the errors.

Q1 Who believes in many-worlds?

The poll by "Political scientist" L David Raub quoted under this topic suggests that 58% of "leading cosmologists and other quantum field theorists" agree "Yes, I think MWI is true".

By no means does this suggest that 58% of physicists (or leading physicists) believe this. It is easy to construct a poll to get the answers you want or to misinterpret poll results. Among the supporters listed is Richard Feynman. Since he wrote extensively on physics and there are no corroborating reports that he accepted many worlds this casts doubt on the entire survey. It is true that many physicists believe that the existing model fully describes the evolution of the wave function and thus are lead to some form of relative state interpretation. Others have a wide range of differing opinions. Probably the most commonly held opinion is some form of `shut up and calculate'.

Q2 What is many-worlds

[...] The histories form a branching tree which encompasses all the possible outcomes of each interaction.

This sentence suggests that interactions are some how derivable from the wave function and that the possible outcomes of these interactions are also derivable. The wave function never generates interactions it only allows us to compute the probability of a specific interactions we define. There is no canonical set of possible outcomes from an interaction.

This is a critical point and it is why Bohr was led to evolve the notion of complementary ways of describing physics. Events do not exist in the wave function model. Only a wave function that allows us to compute probabilities for events exist. You cannot escape this. You can assume events happen and work out the consequences of that

assumption and the assumption that the wave function does not change or collapse as a result of these events. However doing this provides no explanation for how these events come about nor does it allow us to derive a canonical set of all possible outcomes.

Some consequences are:

[...]

That the conventional statistical Born interpretation of the amplitudes in quantum theory is **derived** from within the theory rather than having to be **assumed** as an additional axiom. (See "How do probabilities emerge within many-worlds?")

There has been much debate on this question in `sci.physics'. There is no way to derive the probabilistic interpretation from the theory without first making additional assumptions that imply this in the first place. How else could such a derivation be possible? The wave function model is just that: a mathematical model. How could you interpret that model as defining probabilities without additional assumptions?

Many-worlds is a re-formulation of quantum theory, published in 1957 by Dr Hugh Everett III.

Of course it is not a reformulation of QM. It is a derivation from the **existing** formulation and an interpretation of the existing formulation.

Everett discovered that the new, simpler theory - which he named the "relative state" formulation - predicts that interactions between two (or more) macro systems typically split the joint system into a superposition of products of relative states.

Everett's interpretation is not a new theory and is not simpler than standard QM. As a physical theory it is the **same** as standard QM. It is only the **metaphysical** assumptions that are different than other interpretations. The superpositions related to an interaction are a standard part of the QM model and they are experimentally detectable. Assumptions about relative states are metaphysical assumptions. They are not predictions because they have no testable consequences.

Q5 What is a measurement?

A measurement is an interaction, usually irreversible, between subsystems that correlates the value of a quantity in one subsystem with the value of a quantity in the other subsystem.

There are no irreversible processes in QM. The assumptions that such exist, if specific enough, would lead to a theory experimentally distinguishable from QM. This is not part of Everett's interpretation.

Q12 Is many-worlds a local theory?

The simplest way to see that the many-worlds meta theory is a local theory is to note that it requires that the wave function obey some relativistic wave equation, the exact form of which is currently unknown, but which is presumed to be locally Lorentz invariant at all times and everywhere.

This is impossible as Bell[1] and Eberhard[2,3] have proved. The argument made later about contrafactual definiteness is irrelevant because the wave function model satisfies CFD. It gives definite probabilities for any set of initial conditions. The only way to model that mathematically and obtain the standard predictions is with explicitly nonlocal operations.

The relativistic wave function does not exist because it is a logical impossibility. QM requires a configuration space model that is irreducibly nonlocal. Evolution of the wave function in configuration space is local in that space. However the projections from the configuration space wave function model to predictions in physical space is irreducibly nonlocal in physical space. In relativistic QM there are fields that evolve locally but there is also a non local state model. Relativistic QM is an oxymoron in the sense that no model that it is consistent with the predictions of QM can evolve locally in physical space. However, because of quantum uncertainty, the predictions are the same in all relativistic frames of reference. This is possible because the nonlocal effect is 'encrypted' with quantum uncertainty so that we cannot tell in what direction it operates in. However the effect must exist to reproduce these predictions and in any mathematical model it must operate in a particular direction and thus in a particular frame of reference.

According to many-worlds model worlds split with the operation of every thermodynamically irreversible process.

There is no such thing as a discrete thermodynamically irreversible process. There is a continuous process that over time becomes more unlikely to be reversed. The above statement makes no sense.

Q36 What unique predictions does many-worlds make?

A prediction occurs when a theory suggests new phenomena. Many-worlds makes at least three predictions, two of them unique: about linearity, (See "Is linearity exact?"), quantum gravity (See "Why *quantum* gravity?") and reversible quantum computers (See "Could we detect other Everett-worlds?").

This is dead wrong. Of course you can extend any interpretation to create a new theory and people to some degree are trying this but Everett's interpretation on its own makes no new predictions. The argument about reversible quantum computers actually makes the Copenhagen interpretation into a testable theory that differs from standard QM and then devises a test for this theory. The predictions related to linearity are standard part of the existing model. Similarly the claim that all forces (including gravity) must be quantized is a standard part of the existing theory.

References:

- [1] J. S. Bell, On the Einstein Podolsky Rosen Paradox, *_Physics_*, 1, 195-200 (1964).
- [2] P. H. Eberhard, *Il Nuovo Cimento*, "Bell's Theorem without Hidden Variables", 38 B 1, p 75, (1977).
- [3] P. H. Eberhard, Bell's Theorem and the Different Concepts of Locality", *Il Nuovo Cimento* 46 B, p 392, (1978).

[[LAS home page](#) | [UNIS](#) | [NewsPages](#)]