

LETTERS (continued from page 14)

tion protection purposes, that the risk of stochastic effects is proportional to dose without threshold throughout the range of the dose and dose rates of importance in routine radiation protection . . . has led some to believe that even the lowest exposures are unduly hazardous . . . Undue concern, as well as carelessness with regard to radiation hazards, is considered to be detrimental to the public interest. Additionally, the specification in the ALARA principle that economic and social factors be considered has at times been overlooked, resulting in excessive costs with little benefit."⁷ I refrain from checking the monetary value of human life.

In response to Richard Garwin: A sensor for metal toxicity is the sense of taste. Sometimes it warns against acute toxicity, as with some copper compounds; sometimes not, when the stuff is sweet, such as lead acetate. But acute intoxication with metals, before man started to produce them a few thousand years ago, might occur in the terrestrial biosphere only exceptionally, for example at metallic ore outcrops. Much like ionizing radiation, the metals are ubiquitous in the environment at low concentrations. Indeed, mercury and lead at such low levels are hormetic; in other words, stimulatory at sub-inhibitory concentrations.⁸ Therefore, in the past 100 million years there was no need for organisms to develop sensors for metals. However, the safety margin for ionizing radiation is much larger than for many other potentially dangerous agents in the environment. For example, a toxic level for lead in the blood is only 3 times higher than its "normal" level.¹⁰ A lethal one-hour dose of ionizing radiation is about 10 million times higher than the average natural dose received in the same time. So, developing a sensor for ionizing radiation was even less needed than for metals.

Costs of reactor safety in the US of \$2.5 billion per life saved were estimated by Cohen (my original article, ref. 18). These costs were implemented between 1975 and 1985, raising the cost of one nuclear power plant by more than \$2 billion. Cohen stated that, ironically, this spending forced utilities to build, instead of nuclear plants, coal-burning power plants, each of which during its lifetime kills about 1000 people with polluted air. These esti-

mates of life-saving costs were based on LNT and a risk factor calculated from data on Hiroshima and Nagasaki A-bomb survivors exposed to high doses (about 2500 mSv) at extremely high dose rates (such as 2500 mSv per second). Therefore they are not relevant for estimating cancer risk in populations in which the most exposed individual receives a radiation dose ranging from 0.0001 mSv to 0.040 mSv per year.¹⁰ Between 1970 and 1984 the normalized radiation exposure from the non-military nuclear fuel cycle decreased by a factor of 3.¹⁰ This was the only measurable effect of spending (according to Cohen) a total of some \$100 billion in the US alone. In fact this money was wasted fighting a phantom risk. Garwin's calculations of life-saving costs are made on the same principle.

Spontaneous oxidative double strand DNA breaks (DSBs), which are more difficult to repair than single-strand breaks (SSBs), occur at a rate of 40 per cell per year. Natural background of low linear energy transfer radiation of 1 mSv per year produces 0.04 DSB per cell per year. The spontaneous DSBs, though 1000 times greater than those from natural low background radiation of 1 mSv per year,¹¹ still result in a negligible number of mutations compared to those resulting from about 400 million other spontaneous oxidative damages per year. Low doses of ionizing radiation decrease the spontaneous mutation rates caused by DSBs and SSBs, and thus decrease the rate of spontaneous cancers.

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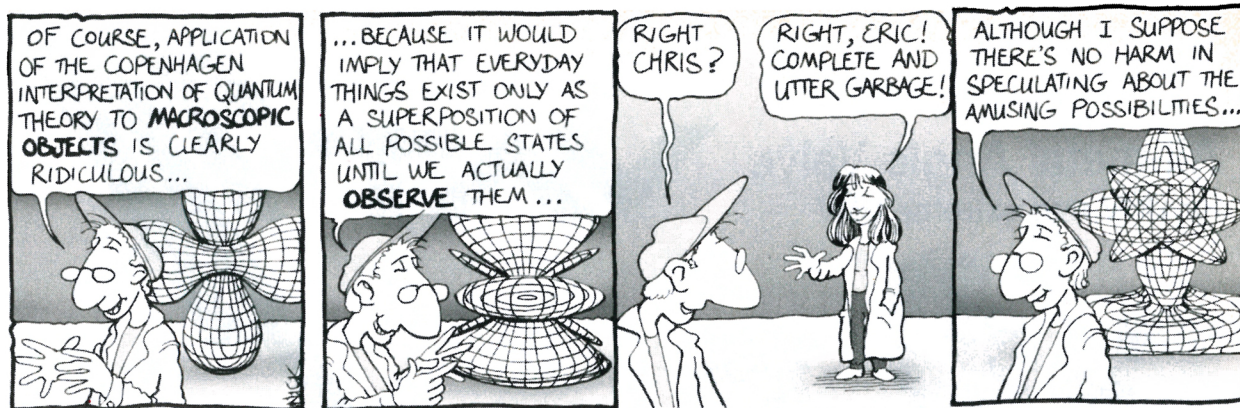
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Quantum Histories, Mysteries, and Measurements

Quantum mechanics is an extremely successful theory with numerous applications, and it is used daily in the laboratory. Yet, from the very beginning it was plagued with debates about its interpretation. There are two camps: those who feel that "interpretation" cannot mean anything other than merely the way in which quantum mechanics is used to obtain results, and those who adhere to the idea that there must be a "real" world hiding behind the mathematical formalism. The debaters keep talking about the difficulties—and even the "mysteries"—of quantum mechanics, rather than trying to resolve them. That may be an innocuous pastime, but when two respectable physicists are also caught in this quagmire (Robert Griffiths and Roland Omnès, *PHYSICS TODAY*, August 1999, page 26), it becomes necessary to point out a few basic truths.

The following essential ingredient is missing in almost all such debates. Although the whole world is ruled by quantum mechanics, macroscopic systems, having many degrees of freedom, can be described to a great approximation by classical theory. The reason is that any single macroscopic state is actually a superposition of an enormous number of eigenstates.¹ Unlike an atom, Schrödinger's cat is not in an eigenstate corresponding either to life or to death, but always in one or the other enormous subspace of its Hilbert space. There are so many interference terms between these subspaces that they average out to zero. As a result the cat is either alive or dead, with probabilities given by the components of the



wavefunction in the two subspaces. When a system is observed, it is true that its Hilbert vector is affected by the act of observing, but in a macroscopic system, the vector merely roams around in one and the same macroscopic subspace. The position of a pointer on a dial is not affected by looking at it, because it is not in a quantum mechanical eigenstate but in a subspace of 10^{50} eigenstates.² The cat is dead or alive whether or not I (or Wigner's friend) look at it. Measurements consist of leaving a macroscopic mark, which is objective and not influenced by further observation. A microscopic object like an electron is observed by allowing it to interact with a macroscopic system prepared in a metastable state, such as a Wilson camera, a Geiger counter, or a photographic plate. The Schrödinger equation serves to compute the behavior of the object, and the measuring apparatus serves to register the result.

Another essential ingredient is often ignored in the debates. It is the realization that the act of measuring requires an interaction between the object and the apparatus, and can therefore be described only by the wavefunction of both together, in their combined Hilbert space. After the interaction, this wavefunction does not factor into an object wavefunction and a wavefunction of the measuring apparatus. Therefore, one cannot speak about the state of the object after the measurement, since it remains entangled with the wavefunction of the apparatus. However, if one takes into account that the apparatus is macroscopic, the correlation terms are so numerous that they average out; one is left with the projection of the combined wavefunction into the subspace of the object. This is John von Neumann's "collapse" of the wavefunction, which now turns out to be a consequence of

the Schrödinger evolution of the combined system. It provides the probabilities for the several possible macroscopic outcomes of the measurement, as the average of the interference terms.

The account given above is not just one more theory of the interpretation of quantum mechanics. It is a description of what actually happens when quantum mechanics is applied to the observable world. It has been demonstrated explicitly by an example.³ In this light, let us consider some of the questions raised by Griffiths and Omnès.

▷ What is so special about the measuring process? The answer is that, in a measurement, a microscopic object leaves a mark in a macroscopic apparatus, which is no longer subject to the quandaries of the quantum world. Whether someone looks at the object is irrelevant; all that matters is that the interaction occurs. Thus the measurement is not, as Griffiths and Omnès declare "one of the most intractable difficulties standing in the way of understanding quantum mechanics."

▷ Is not the entire universe quantum mechanical? In principle, yes, but we live on a macroscopic level; the only features we can observe are macroscopic and therefore objectively verifiable, as in classical physics. It has been claimed that the universe is in a quantum state that collapses into an observable world only after one looks at it.⁴ It may well be asked what this assertion has to do with the actual observations of astronomers.

▷ Does the macroscopic-superposition principle (that is, the interference between a live and dead cat) require hidden variables, modifications of the Schrödinger equation, or "consistent histories"? No, all that is required is to realize that one cannot treat many-body systems in

the same way as single atoms. The fact that they do have individual eigenvalues and eigenstates is as irrelevant as it is for a billiard player to know that the balls consist of molecules.

▷ Which of the two sets of consistent histories labeled (3) and (4) in Griffiths and Omnès's article is the correct description of the beam splitting experiment? History (3) is the exact microscopic evolution, while history (4) takes into account that detectors C and D are many-body systems; hence, (4) is the essential approximation on which our macroscopic world picture is based. There is no need to talk about consistent histories: Everything one wants to know is contained in the combined Schrödinger equation of object-plus-detectors. As for the question raised by Griffiths and Omnès regarding whether the history (3) occurs in place of, or at the same time as, the histories in (4), it makes as much sense as the question whether a billiard ball is described by the Schrödinger equation of all its molecules or by the equations of classical mechanics. Further, the analogy with noncommuting spin operators is inappropriate as it ignores the many-body aspect, and random torques are extraneous to the issue.

In the final section of their article, the authors see a glimmer of light when they consider the classical limit. They are right in rejecting the argument based on that childish theorem of Paul Ehrenfest's, and instead they appeal to coarse graining based on appropriate subspaces in Hilbert space. That is correct, but rather than being an afterthought, it is the very starting point of understanding the measurement process, as I have tried to make clear. This interplay between the microscopic quantum world with probability amplitudes on the one hand, and the

world of macroscopic observations, probabilities, and classical physics on the other hand, is the solution of what is called the mystery of quantum mechanics. The classical limit is a generalization of Niels Bohr's "correspondence principle." It also provides the irreversibility needed to arrive at permanent, objectively recorded measurement results; they are the material of science (rather than the "internal mental state of the individual" referred to by von Neumann and Wigner). An act of measurement is completed when the result is recorded macroscopically.

To conclude, quantum measurements can be fully understood by applying standard quantum theory to object-plus-apparatus, while taking into account that the apparatus is macroscopic. The difficulties are created by talking about microscopic phenomena in classical terms, and by talking about macroscopic objects (like cats) as if they were as small as atoms. There is no need for "coherent histories." Details are given in reference 3.

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Without wishing in any way to diminish the importance of the ideas developed by Robert Griffiths and Roland Omnès and by Murray Gell-Mann and James Hartle, I must protest the suggestion by the former pair that "the measurement problem" in quantum mechanics—or what they call "the macroscopic-superposition problem"—is inherently difficult and can be resolved only by modifying quantum mechanics or by viewing the problem from the vantage point of the "consistent histories" approach.

This problem was solved by Hugh Everett¹ without modifying the standard quantum formalism one bit. To make no reference to him not only perpetuates an unjust neglect, but also shows an *a priori* prejudice.

What Everett did not do was explain why it is so easy to construct apparatus with variances in macroscopic variables sufficiently narrow

to allow us to carry out good quantum measurements in the first place. This is where the consistent histories approach comes in. With appropriate coarse graining, it not only allows us to understand the emergence of the classical world (through the action of the environment), but also allows us to understand those cases in which macroscopic apparatuses are in the process of reacting to quantum observables having discrete spectra.

The traditional concept of measurement is not the whole story, but Griffiths and Omnès are wrong to suggest that it no longer provides a good gateway to quantum mechanics—that it "plays no fundamental role." It needs to be supplemented—by including the environment and choosing a good coarse graining—to explain why we don't see superpositions of live and dead cats; that is all. It is a synthesis that Gell-Mann and Hartle call "post-Everett."

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Robert Griffiths and Roland Omnès write that with their approach to quantum mechanics "there is no need to invoke mysterious long-range influences and similar ghostly effects that are sometimes claimed to be present in the quantum world" (page 26). Readers who are familiar with the work of John Bell on quantum nonlocality—which shows that nonlocality is an inevitable consequence of the predictions of quantum theory, regardless of how that theory is interpreted, be it in terms of hidden variables or according to the standard Copenhagen interpretation—may well ask how Griffiths and Omnès have managed to accomplish this feat.

The answer is that they have not. Their consistent-histories formulation of quantum theory is, in fact, inconsistent, and it suffers from the very contradictions that Bell showed must arise whenever one attempts to give an entirely local account of quantum correlations.

Of course, Griffiths and Omnès do not agree with this conclusion. They maintain that inconsistencies cannot arise within their approach without violating their prohibitions—related to quantum incompatibility—against simultaneously considering the sorts of collections of propositions that, in

fact, are inconsistent, according to Bell and the no-hidden-variables theorems. But an inconsistent collection of propositions cannot be converted to a consistent one merely by prohibiting certain kinds of reasoning.

Logic and reasoning are so fundamental in our thought processes that the very notion of proposing significantly different alternatives to them is profoundly problematical. Consider, for example, the problem of learning to reason correctly according to a proposed alternative. Any such proposal would be expressed in a collection of propositions whose implications would have to be carefully deduced. To do that, we would presumably already have to know how to correctly reason according to the alternative.

Be that as it may, one might reasonably expect a clear description of the rules of correct "quantum reasoning" to be a bit elusive. Griffiths and Omnès demonstrate quite nicely that it is. They write that, for a spin $1/2$ particle, "the quantum beables of this system . . . are of the form $S_w = 1/2$, where w is a unit vector . . . and S_w is the component of spin angular momentum in that direction" (page 28). Using this notion of quantum events and beables, they then stress the distinction between a statement that is meaningful but false and one that is, in fact, meaningless. As an example of a meaningful but false statement, they give $S_z = 1/2$ and $S_z = -1/2$, which amounts to the same thing as $S_z = 1/2$ and $S_z = 1/2$. As an example of a meaningless statement, they give $S_x = 1/2$ and $S_z = 1/2$, declaring it to be a meaningless expression because it cannot be associated with any genuine quantum beable . . . there seems to be no sensible way to identify the assertion " $S_x = 1/2$ and $S_z = 1/2$," with $S_w = 1/2$ for some particular w (page 28). However, this argument cannot be correct, since it applies to $S_z = 1/2$ and $S_z = 1/2$ just as well as to $S_x = 1/2$ and $S_z = 1/2$.

Griffiths and Omnès also argue that the statement $S_x = 1/2$ or $S_z = 1/2$ is meaningless. But suppose that after measuring a spin component and finding the value $1/2$, I forget whether it was the x -component or the z -component that I had measured. Would it not then be appropriate to say that $S_x = 1/2$ or $S_z = 1/2$? And if that is so, what could be meant by the assertion that the statement is meaningless?

If both " $S_x = 1/2$ " and " $S_z = 1/2$ " are meaningful—regardless of what

the meanings might be—while “ $S_x = 1/2$ and $S_z = 1/2$ ” and “ $S_x = 1/2$ or $S_z = 1/2$ ” are meaningless, that can be only because the words “and” and “or” are problematical. But if we really don’t understand “and” or “or,” we can hardly be expected to understand much of anything else—certainly not the foundations of quantum mechanics.

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Griffiths and Omnès insist that assertions such as “ $S_z = 1/2$ and $S_x = 1/2$ ” are not simply wrong but meaningless.

In order to see why their proposal is not a way out of the problem, think, for example, of the French and the American revolutions. They are different “histories” about which true statements can be made, and it would be nonsensical to declare that a combination of a true statement about one history and a true statement about the other is “meaningless.” One may reply that the quantum world is strange, microscopic, nonclassical, and so forth. But remember that the issue here is to get a better understanding of that strange world and not, for example, to make new predictions. If, in order to do that, we have to give up the ordinary use of the word “and” combining two true propositions—a usage that is essential to the way we think—then surely no improvement of our understanding is achieved. In fact, suppose we do give up the ordinary use of “and,” but still allow people to write two statements on the same page. How can one think that each statement, taken separately, is true, but that the text written on that page is “meaningless”? In fact, if one would be allowed to make such moves (declare that the conjunction of two true propositions is meaningless) in other fields, then it is difficult to imagine any conceptual problem that could not, in that way, be “solved.”

Nor can one claim that such rules of logic are “imposed by experiments”; indeed, nothing in the experiment tells us that the retrodiction that Griffiths and Omnès attempt is true. To reply that this retrodiction is what the measurement shows is to fall into the trap of naive realism: attributing an objective reality to whatever concept the physical theory introduces and declares to be “meas-

urable.” What one needs, instead of a naive realist approach, is a description of those specific physical processes called “measurements,” which accounts for their observable results—without appealing to a *deus ex machina* such as “the observer,” and without abandoning the fundamental modes of thinking that are essential in our attempts to understand the world. Remarkably, this was done more than forty years ago by David Bohm.

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GRIFFITHS AND OMNÈS REPLY: Nico van Kampen’s letter contains a recapitulation of a number of items from his 1988 article in *Physica*. We agree with him on several of the points that he makes: macroscopic systems can be described to a good approximation by classical theory, a measurement must be discussed using the full Hilbert space of the apparatus along with the measured system, and macroscopic effects play an important role in actual laboratory measurement processes.

Recent progress in interpretation relies, in our opinion, on three basic ideas: decoherence, consistent histories, and modern derivations of classical physics from quantum mechanics. Constraints on the length of our article prevented us from going into details or explaining various connections between these topics; in particular, the importance of decoherence (as in the “decohering histories” of Gell-Mann and Hartle) in making the history of a macroscopic measurement consistent. While we think that van Kampen has an excellent intuition for what is involved in the quantum measuring process, his treatment is not, in our opinion, completely systematic in a way that avoids criticisms of the sort discussed by Mittelstaedt.¹ We believe that the analysis using consistent histories confirms the basic correctness of van Kampen’s strategy, while allowing it to be extended to discuss such things as the correlations between a pointer position and the earlier state of the measured system *before* it interacted with the apparatus—a matter of obvious importance for the interpretation of many laboratory experiments.

Bryce DeWitt asserts that the problem of macroscopic superpositions was solved by Hugh Everett, and criticizes us for making no refer-

ence to Everett’s work. While it is no doubt the case that his work was a source of inspiration for quite a few physicists, including some consistent historians, we agree with Barrett² that Everett’s ideas do not constitute a complete solution to the measurement problem. In addition, as was pointed out, there are substantial differences between the many worlds interpretation and the consistent histories approach.³ Nowadays it is generally acknowledged in the quantum foundations community that the two are quite distinct.

Sheldon Goldstein’s letter raises several issues, and we begin with the topic of Bell’s inequality and nonlocality. Bell derived an inequality for correlations in two-spin (or two-photon) systems, and this inequality is clearly violated by the predictions of quantum mechanics. (Experiments are in good agreement with quantum theory, though whether they demonstrate a violation of Bell’s inequality is still the subject of some debate.) Consequently, quantum theory is in disagreement with one or more of the assumptions used in deriving the inequality. These include locality, but also the assumption, at least in Bell’s original work and some of the subsequent derivations, that it makes sense to ascribe simultaneous physical reality to two different components of the spin angular momentum of a spin-half particle. But as pointed out in our article, in the consistent histories approach this is meaningless, so from this perspective such derivations of Bell’s inequality fail because they employ an assumption that is incompatible with the structure of the quantum Hilbert space used to describe the spin.⁴ This has nothing to do with issues of locality, as it refers to properties of a single spin-half particle; distant measurements on the other particle are entirely irrelevant. There are, to be sure, other approaches to deriving Bell inequalities in which there is no direct reference to incompatible spin states. Thus, while the matter has not been definitively settled, there is at present no reason to accept without qualification Goldstein’s blanket assertion that “nonlocality is an inevitable consequence of the perditions of quantum theory.”

Goldstein also raises the issue of the consistency of the consistent histories rules for reasoning, and both he and Bricmont are concerned that



"You'll get all the answers to the grand unified theory later . . .
I'm just here to fit you for wings."

what we are proposing amounts, in the words of the latter, to "abandoning the fundamental modes of thinking that are essential in our attempts to understand the world." In responding to this, we note that most quantum physicists follow von Neumann⁵ in supposing that a Hilbert space is the proper mathematical structure for describing a quantum system, that physical properties correspond to subspaces of the Hilbert space, and that the negation of a property corresponds to the orthogonal complement of its subspace. It is regrettable that so few seem to be aware that these principles inevitably require some modification of the usual rules of propositional logic when dealing with quantum properties. (For an elementary discussion of this point, see Sec. IVA in ref. 6.) Such a modification was proposed in 1936 by von Neumann and Garrett Birkhoff,⁷ and we strongly urge our colleagues to read at least the introduction to this paper in order to convince themselves that it is possible to tinker with the rules of propositional logic without losing one's reason, bringing about the collapse of Western civilization, or joining the postmodernists. Having done so, they will be in a much better position to examine our proposal with an open mind, since it is (in our opinion) far less radical than the one proposed by two prominent 20th-century mathematicians. What we are recommending^{8,9} is a syntactical rule governing how logical expressions can be formed in a meaningful way, which prohibits

combining propositions from distinct, incompatible consistent families. Each consistent family, on the other hand, constitutes a logic in which the usual rules of reasoning apply. Hence, rather than demanding that physicists learn new rules of reasoning, we are doing precisely the opposite: showing how the standard rules of reasoning can be safely imported into the quantum domain without leading to any inconsistencies, paradoxes, or contradictions. Inconsistencies arise, new modes of reasoning have to be invented, and the meaning of the logical connectives AND and OR becomes problematical precisely when the rules we propose for meaningful statements are ignored, and attempts are made to combine with one another statements belonging to different, incompatible logics. When the rules are followed, the consistent histories approach is consistent, as conceded by one of its severest critics.¹⁰

In addition, Goldstein asks whether " $S_z = 1/2$ or $S_x = 1/2$ " makes sense if one has measured one component of spin, but forgets which one. Note that different apparatus settings are needed to measure different components of spin. These different settings correspond to macroscopically distinct quantum states with mutually orthogonal projectors, and these settings must be included as part of a consistent quantum description.

Jean Bricmont also raises the issue as to why true statements about the French and American revolutions can be combined, whereas

this is not possible in his example of two successive measurements of a spin-half particle. The answer is that decoherence is a sufficiently effective process that all ordinary macroscopic events, including those that constitute human history, can be embodied in a single consistent family, often referred to as a quasiclassical family.

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Correction

April 2000, page 83—Robert Rathbun Wilson was misquoted in his response to the question: Is there anything here that projects us in a position of being competitive with the Russians, with regard to this race? The last sentence of Wilson's reply should have read: "In that sense, it has nothing to do directly with defending our country, except to make it worth defending." ■