Commentary

Quantum mechanics: Fixing the shifty split

Quantum mechanics is the most useful and powerful theory physicists have ever devised. Yet today, nearly 90 years after its formulation, disagreement about the meaning of the theory is stronger than ever. New interpretations appear every year. None ever disappear.

Probability theory is considerably older than quantum mechanics and has also been plagued from the beginning by questions about its meaning. And quantum mechanics is inherently and famously probabilistic.

For the past decade, Carl Caves, Chris Fuchs, and Ruediger Schack have been arguing that the confusion at the foundations of quantum mechanics arises out of a confusion, prevalent among physicists, about the nature of probability.[1] They maintain that if probability is properly understood, the notorious quantum paradoxes either vanish or assume less vexing forms.

Most physicists have a frequentist view of probability: Probabilities describe objective properties of ensembles of "identically prepared" systems. Caves, Fuchs, and Schack take a personalist Bayesian view: An agent assigns a probability p to a single event as a measure of her belief that the event will take place.[2]

Such an agent is willing to pay less than $\$p$ for a coupon that will pay her \$1 if the event happens, and she is willing to underwrite and sell such a coupon for more than $\$p$. Surprisingly, the standard rules for probability follow from the requirement that an agent should never face certain loss in a single event. (For example, if p exceeded 1, she would pay more than \$1 for a coupon that returned at most \$1; if p were negative, she would pay somebody to take a coupon from her that might cost her another \$1.) Avoiding certain loss is the only constraint on an agent's probability assignments.

The probability of an event is not inherent in that event. Different agents, with different beliefs, will in general assign different probabilities to the same event.

The personalist Bayesian view of probability is widely held,[3] though not by many physicists. It has profound implications for the meaning of quantum mechanics, which Fuchs and Schack call quantum Bayesianism—QBism for short. Since quantum states determine probabilities, if probabilities are indeed assigned by an agent to express her degree of belief, then the quantum state of a physical system is not inherent in that system but assigned by an agent to encapsulate her beliefs about it. State assignments, like probabilities, are relative to an agent.

QBism immediately disposes of the paradox of "Wigner's friend." The friend makes a measurement in a closed laboratory, notes the outcome, and assigns a state corresponding to that outcome. Wigner, outside the door, doesn't know the outcome and assigns the friend, the apparatus, and the system an entangled state that superposes all possible outcomes. Who is right?

For the QBist, both are right: The friend assigns a state incorporating her experience; Wigner assigns a state incorporating his. Quantum state assignments, like probability assignments, are relative to the agent who makes them.

QBism also eliminates the notorious "measurement problem." Classical probability theory has no measurement problem: An agent unproblematically changes her probability assignments discontinuously when new experiences lead her to change her beliefs. It is just the same for her quantum state assignments. The change, in either case, is not in the physical system the agent is considering. Rather, it is in the probability or quantum state the agent chooses to encapsulate her expectations.

From the beginning, Werner Heisenberg and then Rudolf Peierls maintained that quantum states were not objective features of the world, but expressions of our knowledge. John Bell tellingly asked, "Whose knowledge? Knowledge about what?" The QBist makes a small but profound correction: Replace "knowledge" with "belief." Whose belief? The belief of the agent who makes the state assignment, informed by her past experience. Belief about what? About the content of her subsequent experience.

Bell also deplored a "shifty split" that haunts quantum mechanics. The shiftiness applies both to the nature of the split and to where it resides. The split can be between the quantum and the classical, the microscopic and the macroscopic, the reversible and the irreversible, the unspeakable (which requires the quantum formalism for its expression) and the speakable (which can be said in ordinary language). In all cases the boundary is moveable in either direction, up to an ill-defined point. Regardless of what is split from what, all versions of the shifty split are vague and ambiguous.

For the QBist, there is also a split. It is between the world in which an agent lives and her experience of that world. Shiftiness, vagueness, and ambiguity all arise from a failure to realize that like probabilities, like quantum states, like experience itself, the split belongs to an agent. All of them have their own split. What is macroscopic (classical, irreversible, speakable) for Alice can be microscopic (quantum, reversible, unspeakable) for Bob, whenever it is part of her experience but not his. Each split is between an object (the world) and a subject (an agent's irreducible awareness of her or his own experience). Setting aside dreams or hallucinations, I, as agent, have no trouble making such a distinction, and I assume that you don't either. Vagueness and ambiguity only arise if one fails to acknowledge that the splits reside not in the objective world, but at the boundaries between that world and the experiences of the various agents who use quantum mechanics.

Albert Einstein famously asked whether a wavefunction could be collapsed by the observations of a mouse. Bell expanded on that, asking whether the wavefunction of the world awaited the appearance of a physicist with a

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PhD before collapsing. The QBist answers both questions with "no." A mouse lacks the mental facility to use quantum mechanics to update its state assignments on the basis of its subsequent experience, but these days even an undergraduate can easily learn enough quantum mechanics to do just that.

QBism explains the persistence of the disreputable notion that "consciousness collapses the wavepacket." That is true, but in a banal way. The conscious experience of an agent guides her actions in any number of familiar ways. If she has at least an undergraduate degree in physics, these may include revising, on the basis of new experience, her expectations of future experience embodied in her prior quantum state assignments.

There are glimmerings of QBism in the writings of some of the founders of quantum mechanics. Niels Bohr wrote, "In our description of nature the purpose is not to disclose the real essence of the phenomena but only to track down, so far as it is possible, relations between the manifold aspects of our experience."[4] (Once I thought the crucial word here was "relations"; now I realize it is "experience.") Erwin Schrödinger, often philosophically at odds with Bohr, noted, "The scientist subconsciously, almost inadvertently simplifies his problem of understanding Nature by disregarding or cutting out of the picture to be constructed, himself, his own personality, the subject of cognizance."[5] (Here the crucial word is "subject.")

I find QBism by far the most interesting game in town. It has not, however, been enthusiastically received by the contemporary quantum-foundations community. Fuchs, in his role as QBism's most fervent advocate, is admired as a provocateur, his more technical work is highly regarded, and he was elected to the leadership of the American Physical Society's topical group on quantum information. But I would say that, with some important exceptions, the general response to QBism has been to shrug it off. I attribute that, in my uncharitable moments, to people having too much fun working on the puzzles that QBism has eliminated.

I write this Commentary not to persuade such experts, but to bring QBism to the attention of the much larger community of physicists who have no professional interest in quantum foundations. The message from QBism is this: You needn't feel guilty about never getting nervous about this stuff. You were right not to be bothered. But for the sake

of intellectual coherence, you had better reexamine what you wrongly may have thought you understood perfectly well about the nature of probability.

References

1. See, for example, C. A. Fuchs, http://arxiv.org/abs/1003.5209, secs. 1–3.
2. For a short, readable introduction, see R. Jeffrey, *Subjective Probability: The Real Thing*, Cambridge U. Press, New York (2004).
3. J. M. Bernardo, A. F. M. Smith, *Bayesian Theory*, Wiley, New York (1994), and the 65 pages of references therein.
4. N. Bohr, *Collected Works*, vol. 6, J. Kalkar, ed., North-Holland, Amsterdam (1985), p. 296.
5. E. Schrödinger, *Nature and the Greeks, and Science and Humanism*, Cambridge U. Press, New York (1996), p. 92.

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Letters

Measured energy in Japan quake

The article by Thorne Lay and Hiroo Kanamori titled "Insights from the great 2011 Japan earthquake" (PHYSICS TODAY, December 2011, page 33) is an interesting one. As a seismologist who worked in the field of underground nuclear explosions, I was caught by the following statement in the first paragraph: "Total strain energy equivalent to a 100-megaton explosion was released during the sliding." Some familiarity with this subject led me to think this is not right. If the authors would carefully review their calculations using the energy equivalent in TNT, the relationship between seismic moment and magnitude, and the relationship between strain energy and seismic moment, they would find that the seismic energy equivalent of the 2011 Japan earthquake is roughly 2×10^{18} J, while that of a 100-megaton nuclear bomb is roughly 4×10^{17} J. Thus the 2011 Japan subduction event released approximately five times as much energy as a 100-megaton device, which is approximately twice the largest nuclear detonation ever—a 50-megaton atmospheric explosion by the former Soviet Union in October 1961.

The 1964 Chilean earthquake had still more energy by a factor of about 3, or 15 times that of a 100-megaton nuclear device. I believe the authors used the relation for seismic energy release rather than total strain energy release. The seismic energy underestimates the total strain energy release by a variable that depends on friction on the fault plane. Accounting for total strain energy release would increase the earthquake energy number by orders of magnitude.

Despite the catastrophic damage potential of nuclear bombs, the forces of nature occasionally unleash much larger energy releases. Although the nuclear bombs are under our control, earthquakes, volcanic eruptions, and extreme weather events are not. However, by judicious preparation and avoidance measures, humans can significantly diminish the damage of natural events.

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Lay and Kanamori reply: Our article states that the total radiated energy release estimated for the Tohoku event, as directly measured by integration of seismic-wave ground-velocity recordings and the source time function, is 4.2×10^{17} J. That number compares with David von Seggern's energy value for a 100-megaton explosion of "roughly 4×10^{17} J." Thus we seem to agree that our estimate of the seismic wave energy release from the earthquake corresponds to total energy from a 100-MT explosion.

The wording in the first paragraph of our article, however, should have been "total radiated energy" rather than "total strain energy." Some strain energy goes into heating the fault zone and other dissipative processes, so total strain energy will always exceed seismically radiated energy by an amount that cannot be measured by seismology. Von Seggern computes a number for "seismic energy" using a formula (apparently the Gutenberg–Richter relation) for radiated energy as a function of seismic magnitude; that is quite different from estimating radiated seismic energy directly as we did. His estimate of seismic energy is about a factor of five larger than our directly measured radiated energy estimate. Scaling relations between seismic magnitude and energy have very large spread, so we prefer direct measures of radiated energy from seismic waves.

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Private versus public energy solutions

Former Department of Energy official Steven Koonin expressed unwarranted confidence (PHYSICS TODAY, January 2012, page 19) that "energy needs to happen through the private sector. It owns, builds, operates essentially all the energy infrastructure in the country, and I don't think we have any intention of changing that."

I offer the following example to illustrate why I take issue with Koonin: During the night of 30 November–1 December 2011, residents of the West San Gabriel Valley, about 15 miles northeast of Los Angeles, experienced a severe Santa Ana windstorm that produced hurricane-force gusts. Thousands of trees were blown down, and power outages were widespread. The area is served by two utilities: Community-owned, not-for-profit Pasadena Water and Power (PWP), which provides electricity for the homes and businesses in Pasadena; and privately owned, for-profit Southern California Edison (SCE), which powers the surrounding communities.

Pasadena itself was probably the hardest hit, with about 1200 downed trees and nearly $30 million in damages. The wind speeds there during the event were at least as high as, and perhaps higher than, those in the surrounding communities. Nevertheless, only 10% of PWP customers lost power during the windstorm.

Meanwhile, Altadena, Arcadia, La Cañada Flintridge, and San Marino experienced total blackouts. In other nearby communities, such as Sierra Madre, South Pasadena, and Monrovia, at least 80% of homes and businesses lost power. In a front-page story in the *Pasadena Star-News* on 13 January 2012, SCE admitted that 75% of its customers in the area affected by the windstorm lost power.

In addition, while nearly all PWP customers had their power restored within 48 hours, many SCE customers had to wait much longer, some as long as a week.

The performance of SCE during and after the windstorm was so bad that it is now being investigated by the California Public Utilities Commission. Simply put, private-sector, for-profit SCE put in a dismal performance compared with the not-for-profit, community-owned PWP.

Perhaps Koonin needs to reconsider his belief that the private sector, with its focus on profits and stock dividends,

Measured responses to quantum Bayesianism

David Mermin, in his commentary in the July 2012 issue of PHYSICS TODAY (page 8), put forth what he calls the QBist (quantum Bayesian) approach to quantum foundations. He claims that replacing a frequentist approach to quantum probabilities with a Bayesian approach solves the quantum measurement problem and fixes the "shifty split" between classical and quantum that John Bell complained about.[1] I disagree. Mermin has not addressed the real issue that besets quantum probabilities, he has not solved the measurement problem, and he has put the shifty split in the wrong place.

The quantum measurement problem, understanding an actual physical measurement in fully quantum terms, has two parts. First, unitary time development (Schrödinger's equation) often results in a quantum superposition of different outcomes—different positions of the apparatus pointer, in the quaint language of quantum foundations. Various equivocations proposed to get rid of that Schrödinger's cat were among the main targets of Bell's critique. When this first part is solved the second task is to link the pointer position to the microscopic property the apparatus was designed to measure, at a time *before* the measurement took place. Experimental physicists talk about detecting a gamma ray emitted by a nucleus, or a neutrino emitted in a supernova explosion, using apparatus that either destroys or violently alters the object under study. They are not fools, though one might think so from reading textbook discussions of measurement that only consider properties of a microscopic system at a time after interacting with the apparatus—best referred to as a preparation, not a measurement.

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Mermin seems to think that the measurement problem—presumably the first problem—is solved by using the quantum wavefunction to calculate a probability, in a manner easily taught to undergraduates. However, the main conceptual difficulty with quantum probabilities is not in calculating them but in identifying their referents, what it is they are about. When the weatherman assigns a high probability to a severe thunderstorm on Thursday afternoon, both frequentists and Bayesians will want to seek shelter. They will agree that the probability refers to thunderstorms rather than stock-market prices. The first task in constructing a probabilistic model, for the weather or games of chance or radioactive decay, is to identify a sample space of mutually exclusive possibilities, one and only one of which can be correct or occur in a particular experiment or on a particular occasion. Only when a sample space has been defined is it possible to assign probabilities to suitable subsets, averages to random variables, and so forth.

A classical phase space is easily turned into a probabilistic sample space: The different points represent distinct physical states of affairs. But a quantum Hilbert space is very different if, following John von Neumann's thinking, one associates physical properties with (closed) subspaces and associates their negations with orthogonal complements of the subspaces. That classical–quantum difference was the origin of quantum logic,[2] which, despite early hopes and much hard work, has not resolved the conceptual difficulties of quantum mechanics.[3] But it does point to important issues that need to be considered when discussing quantum probabilities. Mermin and his fellow QBists should pay attention.

Quantum orthodoxy has no solution for the fundamental problem of defining a quantum sample space. Instead, it covers with a black box the mysterious whatever-it-is that the quantum wavefunction might have something to do with. Talking about what is under the box is more or less strictly forbidden, for it is well known that physicists who attempt to do so will fall into the quantum swamp, to be eaten by the Great Smoky Dragon or driven insane by the Paradoxes. The black box is my term for Bell's split between the macroscopic and microscopic, a split forever shifting as experimentalists manage to entangle larger and larger quantum systems. According to Mermin, the QBists place the split "between the world in which an agent lives and her experience of that world." That is no improvement: The box would then cover the physicist rather than the quantum mystery. Bell would not have been pleased.

Bell was unaware of the consistent or decoherent histories approach to quantum mechanics,[4] which, unlike QBism, solves both measurement problems in a way fully consistent with the Hilbert space structure of quantum mechanics and consistent with special relativity. It drives the shifty split off to infinity where it belongs: Quantum physics applies at all scales, from the quarks to the quasars. And it gets rid of the spooky nonlocal influences that Einstein found so distasteful. It seems odd that Mermin has thrown his lot in with the QBists rather than paying serious attention to the histories approach which, unlike QBism, clearly satisfies two desiderata for a good quantum interpretation that he himself put forward 15 years ago:[5] "The theory should describe an objective reality independent of observers and their knowledge" and "objectively real internal properties of an isolated individual system should not change when something is done to another non-interacting system."

Mermin says even undergraduates can be taught enough quantum mechanics to update a quantum state assignment. I remember when my undergraduate quantum mechanics teacher, Robert Dicke of cosmic-background fame, was visibly uncomfortable as he introduced us to wavefunction collapse. For those who share this discomfort, I recommend that rather than telling students to "shut up and calculate," we

- Introduce them to the rudiments of probability theory, including the conditional probabilities used by both frequentists and Bayesians; one need not take sides.
- Explain how to construct quantum sample spaces and assign probabilities in a way that does not generate paradoxes.
- Explain wavefunction collapse as a tool for calculating certain conditional probabilities that can also be obtained using other methods; it is not a physical process.

Readers who want more details can contact me or consult the works cited in reference 4. I think it is high time we abolished antiquated approaches to teaching quantum theory along with the shifty split that confuses both students and their instructors.

References

1. J. S. Bell, *Speakable and Unspeakable in Quantum Mechanics*, 2nd ed., Cambridge U. Press, New York (2004), chap. 23.
2. G. Birkhoff, J. von Neumann, *Ann. Math.* **37**, 823 (1936).
3. G. Bacciagaluppi, in *Handbook of Quantum Logic and Quantum Structures: Quantum Logic*, K. Engesser, D. M. Gabbay, D. Lehmann, eds., North-Holland, Amsterdam (2009), p. 49.
4. P. C. Hohenberg, *Rev. Mod. Phys.* **82**, 2835 (2010); R. B. Griffiths, *Consistent Quantum Theory*, Cambridge U. Press, New York (2002); R. B. Griffiths, *Found. Phys.* **41**, 705 (2011).
5. N. D. Mermin, *Pramana* **51**, 549 (1998).

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■ **David Mermin's exposition** on QBism was excellent. However, it left the reconciliation between subjective and objective probability up in the air. There must be circumstances in which the subjective probabilities of different scientifically trained agents coincide, and hence the subjective and objective approaches also coincide. Quantum mechanics reminds us, if such a reminder is needed, that the human experiences that produce coinciding subjective probabilities, and hence interesting science, form a small subset of all human experiences.

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■ **Some time ago,** John Bell and I wrote a tongue-in-cheek article on the interpretation of quantum mechanics;[1] in it we remarked that only a minority of physicists had any interest in the topic and that the typical physicist thought he would understand it "if ever he can spare twenty minutes to think about it." According to David Mermin, however, that situation has changed dramatically, and "new interpretations [of quantum mechanics] appear every year."

Actually, most textbooks on the subject continue to be based on the well-established Born interpretation of quantum mechanics, which postulates that the absolute square of the Schrödinger wavefunction, projected onto a particular quantum state, gives the

probability for the occurrence of that state after an appropriate measurement has been made. Mermin indicated that the problem with the Born interpretation involves a confusion, "prevalent among physicists," regarding the nature of probability. He presented his preference for a "personalist Bayesian view," which leads separate "agents," like Eugene Wigner and his friend, to make different probability assignments based on their beliefs. But as Richard Feynman remarked, "Nature does not know what you are looking at, and she behaves the way she is going to behave whether you bother to take down the data or not."[2]

Indeed, as applied to experiments, probability simply corresponds to the relative frequency for the occurrence of a given final state after identical states are prepared initially and measurements are made by appropriate detectors—devices that, through an irreversible macroscopic process, provide a permanent record of the outcome. Hence, the results of an experiment are independent of any observers, whose only role is to set up appropriate measurement devices, calculate the probabilities from quantum theory, and then write papers based on the recorded results.

As Max Born also emphasized, in classical mechanics the situation is analogous, when one takes into account that in practice initial conditions in an experiment are known only imprecisely.[3] Hence the observed evolution of the system is also determined by a probability function that satisfies the Liouville equation. But in that case, after a measurement is performed, no one talks about the "collapse" of this classical function, whereas in quantum mechanics it is common to use that controversial expression for the Schrödinger wavefunction.

Mermin discussed a "shifty split" between the classical and the quantum domain, a split that supposedly "haunts quantum mechanics." He claimed that "regardless of what is split from what, all versions of the shifty split are vague and ambiguous." However, that is not always the case, as a simple example shows. Consider an initial quantum wavepacket for a particle under an attractive central force that varies inversely with the square of the radial distance—the classic problem of planetary motion from the viewpoint of quantum mechanics, for example, or an excited electron in a Rydberg atom. For a finite time, the wavepacket will follow a classical Keplerian elliptic orbit, but like the Liouville distribution for the analogous classical problem, from the outset it will disperse due to the spread in initial position and momentum.

For the classical distribution to be in close correspondence with the quantum wavepacket, its initial spread is assumed to satisfy the uncertainty relation. Eventually, however, the head of the wavepacket, like in the classical distribution, will catch up with its tail. Afterwards, further spreading of the wavepacket leads to interference effects between the two components, and the classical–quantum correspondence ceases to be valid. The time when such interference effects first appear is of order $n\tau/3$, where n is the mean principal quantum number of the wavepacket and τ is the Kepler period.[4] Experimentally, that transition has been observed to agree with theory in Rydberg atoms[5] for $n = 100$, $\tau = 152$ picoseconds, and nothing shifty about this classical–quantum correspondence is found. For planetary orbits, however, n is of order 10^{70}, and therefore the answer to Albert Einstein's often repeated remark "Is the moon there when nobody looks?" (see David Mermin, PHYSICS TODAY, April 1985, page 38) is yes.

References

1. J. S. Bell, M. Nauenberg, in *Preludes in Theoretical Physics*, A. de Shalit, H. Feshbach, L. van Hove, eds., North Holland, Amsterdam (1966), p. 270; reprinted in J. S. Bell, *Speakable and Unspeakable in Quantum Mechanics*, Cambridge U. Press, New York (1987), p. 24.
2. R. P. Feynman, R. B. Leighton, M. Sands, *The Feynman Lectures on Physics*, vol. 3, Addison-Wesley, Redwood City, CA (1989), p. 3-7.
3. M. Born, *Dan. Mat. Fys. Medd.* **30**(2), 1 (1955).
4. M. Nauenberg, *Phys. Rev. A* **40**, 1133 (1989).
5. M. Nauenberg, C. Stroud, J. Yeazell, *Sci. Am.*, June 1994, p. 44.

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■ **Because it's important** that physicists discuss the problems of quantum foundations, I was delighted to see David Mermin's commentary on "fixing the shifty split." But I must disagree with my friend's view.

There are multiple ways of interpreting probabilities.[1] In quantum physics, the Bayesian "degree of belief" interpretation can be appropriate for mixed states, but only the frequentist or "ensemble" interpretation applies to the pure states that Mermin discusses.

A recent paper by Matthew Pusey and coauthors[2] concludes that if two different pure quantum states are assigned to a single physical situation, one of the states is objectively wrong. The proof's main assumption is that any quantum system has some set of real physical properties, labeled λ. Pusey and coauthors show that two different pure states cannot represent the same λ. So, although their paper implies but does not assume that quantum states are physically real, it does assume that each specific physical situation has behind it some kind of physical reality. Chris Fuchs, quoted favorably by Mermin, might dispute the notion that some kind of physical reality actually exists, but most physicists are probably sufficiently realist to grant that notion. Without such a notion, science lies somewhere between solipsism and superstition.

Whenever I hear subjective interpretations of quantum physics, I wonder about such questions as the one Mermin quotes from Albert Einstein: Can a mouse collapse a wavefunction? Mermin's Bayesian response, that a mouse cannot but a physics student can, doesn't reassure me. Were wavefunctions not collapsing before there were physicists?

The "Wigner's friend" paradox, as described in Mermin's commentary, doesn't need a subjective interpretation to find a solution. In fact, Eugene Wigner abandoned his interpretation around 1970 once he became aware of the ideas underlying quantum decoherence.[3] Suppose Wigner's friend makes a measurement, the outcome of which is known to the friend but not to Wigner. Regardless of whether the outcome is known, and in fact regardless of the presence of the friend, the measurement device must decohere the quantum system that's being measured, putting the system into a mixed state of the either/or form, with no interferences, just like a coin that's been flipped. A definite outcome has thus occurred, and Wigner and his friend will both agree on this. Neither the friend nor Wigner affects the outcome. She has simply "looked" and now knows the outcome, while Wigner doesn't know. That is not paradoxical and does not require a revision of our understanding of pure quantum states.

There's no reason to give up on a realist interpretation of quantum physics, much less a realist interpretation of nature itself.

References

1. A. Hobson, *Concepts in Statistical Mechanics*, Gordon and Breach, New York (1971), p. 27.
2. M. F. Pusey, J. Barrett, T. Rudolph, http://arxiv.org/abs/1111.3328v1.
3. M. Schlosshauer, *Decoherence and the Quantum-to-Classical Transition*, Springer, Berlin (2007), p. 364.

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■ **David Mermin's essay** is a welcome introduction to the quantum Bayesian, or QBist, approach to quantum theory. Unfortunately, it stops just as the story gets interesting. The QBist research program is illuminating not just because it makes old philosophic problems melt away but because it nourishes new ideas that take root in the solid ground of everyday physics.

For example, the field of quantum-state tomography is concerned with how to acquire information about unknown quantum states. But if one holds, as the QBists do, that quantum states are states of knowledge, information, or—most provocatively—belief, what can it mean for a state of knowledge to be unknown? A subjective Bayesian classical statistician is in the same quandary when confronted with

the notion of an unknown probability. That problem was solved by Bruno de Finetti's representation theorem, which establishes that "unknown probability" is sloppy shorthand for a rigorously defined experimental scenario. Informally speaking, the classical de Finetti theorem says that if one is willing to gamble that the order of the repeated trials in a multi-trial experiment does not matter, then he can act as though he is measuring an "unknown probability." QBism resolves the conundrum of "unknown quantum states" with a quantum generalization of the de Finetti theorem. In turn, those developments, born from QBist philosophical inquiries, have led to practical advances in quantum tomography.[1]

Furthermore the QBist drive to understand quantum states as states of belief has stimulated fascinating technical work on the old problem of how to reexpress quantum mechanics entirely in terms of the probabilities that the states catalog. That work has led to advances in areas ranging from quantum information theory to the pure mathematics of Lie algebras.[2]

QBism treats measurement—the intervention of one piece of the natural world (the physical system) into the experience of another (the agent)—as a fundamental primitive process of quantum theory. Seeking a better definition of "measurement" from conventional pre-QBist quantum theory is rather like expecting the standard axioms of arithmetic to define the fundamental primitive terms "number," "zero," and "successor": an exercise in missing the point.

Some theorists treat quantum theory as a noncommuting generalization of classical probability.[3] Others think of it as overlapping with classical stochastic mechanics.[4] QBism takes quantum theory to be a specialization of unadorned probability theory, in which the restriction in question is imposed by the character of the physical world. If this variety of options does not say that today is an exciting time to study quantum probability, what could?

References

1. C. A. Fuchs, R. Schack, P. Scudo, *Phys. Rev. A* **69**, 062305 (2004).
2. D. M. Appleby, S. T. Flammia, C. A. Fuchs, *J. Math. Phys.* **52**, 022202 (2011).
3. M. S. Leifer, R. W. Spekkens, http://arxiv.org/abs/1107.5849.
4. J. Baez, "Network Theory," http://math.ucr.edu/home/baez/networks.

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■ **Mermin replies:** Robert Griffiths takes my joke about an undergraduate collapsing a wavepacket to be an attempt to solve the measurement problem. The actual QBist solution—versions of which predate QBism (see, for example, reference 1)—is that a quantum state is not an objective property of the world but a compendium of probabilities constructed by an agent. When an agent updates her state assignment, nothing changes in her external world. The only change is in the agent's expectations for her subsequent experience of that world, and therefore there is no measurement problem. Consistent historians agree that collapse is not a physical process, but for entirely different reasons.

QBism takes the "mysterious whatever-it-is" that Griffiths criticizes in quantum orthodoxy to be the experience induced in an agent by the world. That, and the fact that different agents may have different experiences, drains the swamp, slays the dragon, and quiets the maddening paradoxes.

QBists hold that any agent can indeed apply quantum mechanics to her own external world, from quarks to quasars. Only an agent's personal, directly perceived experience is boxed off from the domain of applicability. That

exclusion disagrees with most versions of the many-worlds interpretation, but it does not limit the scope of science: Alice can apply quantum mechanics to the experience of another agent, Bob, as represented through his words, writings, or drawings, all of which do belong to her external world.

The way in which Bob's experience impinges on Alice resonates with Niels Bohr's emphasis on the need to state results of experiments in ordinary language. It also addresses Ching Hung Woo's question of how different agents can come to agreement on their state assignments.

Griffiths correctly notes that QBism violates the first desideratum he quotes from my 15-year-old "Ithaca interpretation." Like Barack Obama's view of marriage, my thinking about quantum foundations has evolved. But Griffiths wrongly claims that QBism rejects the other desideratum he quotes: locality. Actually, QBism insists on it. The absurdity of quantum nonlocality provides an independent argument that quantum states cannot be objective internal properties of the systems they describe, whether or not one regards probabilities as subjective. Griffiths is probably right, however, that John Bell would not have been pleased with how QBists fix his shifty split.

I would not call QBism an "antiquated approach." It is younger than Griffiths' consistent histories and, as I have remarked upon elsewhere,[2] taking reality itself to be relative to what he calls a framework, is vastly more radical than taking quantum state assignments to be relative to an agent.

But foreshadowings of QBism can indeed be found in antiquity. Since writing my commentary, I came across another striking anticipation in a 1931 letter from Erwin Schrödinger to Arnold Sommerfeld:[3] "Quantum mechanics forbids statements about what really exists—statements about the object. It deals only with the object–subject relation. Although this holds, after all, for any description of nature, it appears to hold in a much more radical and far-reaching sense in quantum mechanics" (translated by N. D. Mermin and Ruediger Schack).

When Bell and Michael Nauenberg wrote their delightful article in 1966, there was indeed much less interest in quantum foundations than there is today. Most textbooks have wisely steered clear of the many contemporary interpretive positions, since none of them are held by more than a fraction of those physicists with an interest in such issues.

QBists do not reject the Born rule for calculating probabilities, but they do reject the objective frequentist interpretation of those probabilities presented by Nauenberg and held by most physicists. The frequentist view is notoriously circular. It defines probability using such notions as "equally probable," "unlikely," or (with Nauenberg) "identically prepared," none of which make sense without a prior definition of probability. "Identically prepared" might seem safe from circularity, but two different preparations cannot be strictly identical. It would be more accurate to say they must differ in unimportant ways. Unimportant for what? For the probabilities of the outcomes.

When I started to learn about subjective probability, I was surprised to discover that most of the books were not in Cornell University's physics or mathematics libraries but in the business school library. In our recent election, Americans have been told that business experience is necessary for being president; I would suggest that it may be even more helpful for understanding quantum mechanics.

I cannot tell from Nauenberg's remarks about the Liouville equation whether he agrees with the view of QBists (and many others) that the collapse of the wavefunction is no more than the updating of a collection of probability distributions.

Nauenberg's example of the Rydberg atom does not contradict the QBist resolution of Bell's shifty split. The Rydberg atom is part of the world external to the agent, and therefore on the quantum side of her split. There are indeed circumstances under which fully quantum mechanical behavior can look very classical.

Art Hobson repeats the common view that only mixed states can be associated with subjective probabilities. QBism takes that to be the very mistake that got us lost for so long in the quantum swamp. Hobson also believes that wavefunctions were collapsing before there were physicists. Does he believe that probabilities were updating before there were statisticians?

Hobson wrongly claims that Chris Fuchs "might dispute the notion that some kind of physical reality actually exists." What he and other QBists do deny is that there are properties of the external world—"elements of reality"—that underlie quantum state assignments. (The argument of Matthew

Pusey and coauthors that Hobson cites, implying that quantum states are physically real, relies on such hidden variables.) An agent's state assignment rests entirely on her experience of the external world. It is neither solipsistic nor superstitious to maintain that any agent's experience comes fully into being only at the moment it is experienced.

Hobson's treatment of Eugene Wigner and his friend relies on decoherence producing an objective mixed state, which both of them must agree on. But since quantum mechanics holds all the way out to infinity (as Griffiths puts it), even from an objective view of quantum states, that switch from superposition to either/or is at best a FAPP solution, to use Bell's wonderfully sardonic abbreviation of "for all practical purposes."

QBism does not, as Hobson concludes, give up on a realist interpretation of nature. But it does warn us not to confuse nature with the abstractions we have ingeniously constructed to help any agent deal with the very real impact of nature on his or her own internal experience (see my Reference Frame, PHYSICS TODAY, May 2009, page 8).

I'm glad that Blake Stacey has called attention to some of the more applied spinoffs of QBism. I'm also pleased that he mentions Bruno de Finetti, one of the great 20th-century pioneers of subjective probability. Indeed, since there are objective as well as subjective Bayesians, if I had my way, the B in QBism would stand not for Thomas Bayes, but for Bruno de Finetti, who put the crucial point like this: "The abandonment of superstitious beliefs about the existence of Phlogiston, the Cosmic Ether, Absolute Space and Time . . . , or Fairies and Witches, was an essential step along the road to scientific thinking. Probability, too, if regarded as something endowed with some kind of objective existence, is no less a misleading misconception, an illusory attempt to

Open letter to the associate director for DOE's Office of Fusion Energy Sciences

We are early-career research scientists and professors, all under 40, who work in plasma and fusion science. We are concerned about the proposed fiscal year 2013 budget for the Office of Fusion Energy Sciences (OFES) in the Department of Energy's Office of Science and about the future plasma and fusion science funding trajectory it represents.

The current US administration has affirmed its "world-class commitment to science," with the goal of attracting more US students to science and engineering now, and to ITER, the international tokamak fusion project, as it reaches full operating capacity 15 years from now. Those commitments should be applauded, and they should be acted on sensibly to maximize the return on investment for US taxpayers in today's tough fiscal environment.

With a price tag upwards of $20 billion, ITER is the cornerstone of the world's fusion energy program. It represents a leap forward on the path to a viable fusion reactor. Yet ITER is more than an engineering project. It will have to create, confine, and control a self-sustained, burning plasma. The challenge of studying that plasma state is matched by the anticipation of what we will learn. We have theories of how a burning plasma will behave and how associated heat loads and energetic particles will impact the ITER wall materials. And there is one thing we know: ITER is discovery science, and a burning plasma will produce plenty of surprises once we get there. Some surprises may be advantageous, others will need to be mitigated.

US plasma and fusion scientists must be in a position to understand and expand on those new physics insights. The vibrant *domestic* program must be maintained and nurtured, so that today's graduate students and postdocs can become experienced scientists and leaders 15 years from now.

Instead, the administration's FY 2013 OFES budget redirects one-sixth of the FY 2012 domestic spending to the ITER project (see PHYSICS TODAY, June 2012, page 25). If that trend continues, within the next two years hundreds of scientists and engineers at premier US institutions will be laid off. Over time, those layoffs will lead to the permanent loss of some of the brightest young minds from the US plasma and fusion program, and likely from the academic and research communities altogether.

The fusion program has a public-image problem: It was supposed to deliver cheap and safe nuclear energy long before many of us young scientists entered the field. But the plasma and fusion program is much broader than energy research. It encompasses the study of supernovae explosions, solar coronal mass ejections, galaxy clusters, wakefield accelerators, the basic complexity of dynamical systems, and many other plasma phenomena.

Plasma science, with its enormous breadth, draws on many funding agencies, but the 2007 National Research Council report *Plasma Science: Advancing Knowledge in the National Interest* has called on the DOE Office of Science to take the stewardship role in guiding the multifaceted and exciting research field forward. The Office of Science must act on this deed of trust and enable us to capitalize on the public curiosity and interest in the 99.9% of the visible universe we call a plasma.

The US Congress has consistently said that ITER funding should not come from the domestic fusion program, which is already underfunded, yet the contributions to ITER are threatening to consume the entire domestic OFES-funded program. The proposed FY 2013 US contribution to ITER is $150 million and is scheduled to double or even triple in the next few years. That makes us deeply concerned for the ability of the Office of Science to allow and encourage domestic plasma and fusion research to survive and thrive.

The under-40 crowd, those expected to lead our field in the ITER era, respectfully request that you not let the world-leading US plasma and fusion program weaken in comparison to our partners and competitors. Instead, let us capitalize on the taxpayers' domestic R&D and ITER investments. Let us build a stronger and broader program to advance knowledge in basic plasma and fusion science and to prepare the scientific workforce of this country for the era of burning plasma.

In addition to the two of us, 61 other early-career scientists from 27 organizations across the country have signed this letter. The original version, with all its signatories, is available at http://fire.pppl.gov/under_40_letter_2012.pdf.

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exteriorize or materialize our [actual (*vero*)] probabilistic beliefs."[4]

References

1. J. B. Hartle, *Am. J. Phys.* **36**, 704 (1968).
2. M. Schlosshauer, ed., *Elegance and Enigma: The Quantum Interviews*, Springer, Berlin (2011), pp. 177, 280.
3. K. von Meyenn, ed., *Eine Entdeckung von ganz außerordentlicher Tragweite: Schrödingers Briefwechsel zur Wellenmechanik und zum Katzenparadoxon*, Springer, Berlin (2011), p. 490.
4. B. de Finetti, *Theory of Probability*, Interscience, New York (1990), preface.

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Delivering science to the public

David Kramer's Issues and Events item (PHYSICS TODAY, July 2012, page 23) contains timely and important advice. It is far too easy for scientists to lay the blame for the public's overall discomfort with science on the early education system, the media, or political leaders. But in doing so, we miss an important component—that we, as scientists, have a responsibility to share our knowledge, not only with each other but with the general public. Communication must be a two-way street; scientists must work harder to improve and increase the dialog with nonscientists. As Kramer's article points out, scientists have an excellent vantage point from which to begin this process; the scientific community does hold a position of trust within the minds of the US public.

Although not all scientists have the interest or inclination to be the next scientific TV personality, smaller efforts can make positive changes. One such effort is a new project, Why-Sci (http://www.Why-Sci.com), funded by an American Physical Society Public Outreach Grant. Why-Sci is a website and forum that presents a rotating and expanding collection of snippets written by scientists for nonscientists. The site's offerings are short, straightforward, multipurpose descriptions of the what, how, and why of a research project accompanied by a single image. The snippets give the scientist experience in describing and communicating research to a nonscientific audience. For the nonscientist, they are an approachable introduction to a research project and its potential applications, and they offer an opportunity to connect with a real scientist.

During this time of difficult research funding—and defunding—choices, we scientists must step up to the plate and explain why what we do is fascinating, inspiring, and important, not just to us, but to society as a whole.

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■ **I wholeheartedly agree** with many of the points raised by David Kramer in his July 2012 article. As a science writer and communicator for more than 20 years, I have seen the enormous, not yet fully realized potential of scientists to convey the excitement and relevance of their research to the general public. Inside Science is a news service that is supported by the American Institute of Physics, numerous AIP member societies, and other STEM organizations. As its director, I help deliver news articles and video segments on scientists and their research.

Last year we introduced Inside Science Minds, an ongoing series of articles in which those in the science community speak directly to general audiences, whether to offer scientific perspectives on societal issues or to share new ideas. A year ago, I saw Nikodem Poplawski, a young theoretical physicist at Indiana University, present his explorations of the notion that our universe exists inside a black hole. I believed the idea would capture the imaginations of the general public while presenting some basic concepts in general relativity and quantum mechanics, so I invited Niko to write an article for us. After many months of working together to express his work in nonscientists' language, we posted the article (http://insidescience.org/?q=content/every-black-hole-contains-new-universe/566), which was immediately picked up online by Fox News and has become one of the most popular pieces on our website.

I welcome new article proposals for Inside Science Minds, and encourage PHYSICS TODAY readers, their friends, and family members to check out the Inside Science website (http://www.insidescience.org).

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Correction

September 2012, page 68—The citation for Fang Lizhi and Li Shuxian's book *Creation of the Universe* is for the English translation. The Chinese edition was published by Science Press, Beijing, in 1987. ■