

Yang (LHY) proposed in 1957 a leading-order correction to the mean-field approximation due to two-body collisions, which was used by Petrov;<sup>3</sup> that “game-changing correction,” as Ferrier-Barbut calls it, can alter the nature of the Bose–Einstein condensate.

However, as was discussed quite some time ago,<sup>1,2</sup> the strength of three-body interactions can dominate over LHY corrections and can be infinite even if the two-body scattering length is finite.<sup>1</sup> A liquid model based on the Efimov effect<sup>1,2</sup> is more robust than the one Petrov envisioned and much more flexible than the van der Waals model. Unlike the quantum liquid droplets created in mixtures of Bose–Einstein condensates,<sup>4</sup> which have practically the same size for particle numbers up to tens of thousands, the quantum liquid droplets I suggested are truly saturating systems, with basically constant interior density. A droplet can have any size, and it can be formed even from a single element. It is a real liquid, with constant density inside and a well-defined surface, and its density and surface tension can be controlled. Also, it is stable against quantum corrections to the mean field.<sup>2</sup>

Moreover, in a rather special system—an ensemble of spin-polarized tritium atoms—three-body recombination processes are most likely absent.<sup>2,5</sup> Although I did not make the estimates, which should be straightforward, I am sure that by controlling the density and thus the rate of four-body recombination, one could create droplets with basically arbitrarily long lifetimes. A droplet of spin-polarized tritium atoms would be a totally unique object, perhaps as unique as macroscopic superfluid helium, but amenable to precise quantum many-body calculations, both static and time-dependent. Quantum turbulence could be studied in a large class of systems, for which a microscopic theory exists, and unlike in the case of superfluid helium, theory could be directly confronted with experiment.

Quantum liquid droplets could be either boselets or fermilets and would undergo at least two types of phase transitions, from superfluid to normal and from liquid to gas. Their physics should be fascinating. Mixing bosons and fermions can lead to even more interesting and complex objects.

## References

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► **Ferrier-Barbut replies:** I am grateful for Aurel Bulgac’s insight about three-body stabilized quantum droplets. I was aware of his work, but space constraints made it impossible for me to cite the broad swath of related literature. A tritium droplet would certainly be a peculiar object, though as an experimentalist I think making a Bose–Einstein condensate of tritium would be quite challenging.

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## Reviews of quantum foundations

I enjoyed the February 2019 issue of *PHYSICS TODAY* on *Reviews of Modern Physics* at 90 but was disappointed with the article “Quantum foundations” by David DiVincenzo and Christopher Fuchs (page 50). The most useful part of that article was the reference list, which shows *RMP*’s diversity of papers on the subject. My 1970 article on the statistical-ensemble interpretation of quantum mechanics (QM),<sup>1</sup> which people tell me has encouraged them to continue research on quantum foundations (QF), was omitted from the list.

Unfortunately, DiVincenzo and Fuchs continue to mystify measurement in QM, as if it were some deep philosophical concept that must be treated before QM has even been fully formulated. They assert that “physicists and philosophers are still debating what a ‘measurement’ really means.” What is important for QF is not the meaning of the word but an un-

derstanding of the physical process. The authors do not cite any of the published papers that provide such an understanding. And they give too much attention to two marginal interpretations: the many-worlds interpretation (MWI) and quantum Bayesianism (QBism).

In QM, a measurement of an observable should yield an eigenvalue of the observable. If the initial state of the measured object is a superposition of eigenstates corresponding to different eigenvalues, then the interaction of the measurement apparatus with the object will lead to a final state of the whole system—measured object plus apparatus—that is a superposition of different measurement results. The squared amplitude of each term yields the probability of obtaining that result in an individual measurement. That statistical prediction, the Born rule, is common to the Copenhagen and statistical-ensemble interpretations. But the MWI takes a radically different turn. It postulates that the universe branches into several parallel worlds, with each term of the superposition corresponding to the unique result of the measurement in one branch world.

The usual role of an interpretation of QM is to begin with the established mathematical formalism and provide an intuitively comprehensible idea of the physical process that the math describes. The MWI does not do that. Instead, it adds a mysterious process of world-splitting, a strange new cosmology that is alien to the mathematics of QM and not really an interpretation of QM at all. A typical QM measurement, such as that of a spin component in the Stern–Gerlach experiment, is a local and very low energy event. It is not credible that the measurement could have the huge cosmological effect of bifurcating the universe.

When I first heard of the world-splitting assumed in the MWI, I went back to Hugh Everett’s paper<sup>2</sup> to see if he had really said anything so absurd. I found that he had not said so explicitly, but he sometimes used words that could be interpreted in more than one way. The MWI is a possible interpretation of them, but not the most natural one, so I thought. And Everett’s framework still has value even without resorting to the MWI’s world-splitting. His concept of a “relative state” is useful, for instance,

and he is correct in rejecting the notion of the quantum state “collapsing” after a measurement.

QBism begins with the assumption that all kinds of probability can be regarded as subjective Bayesian probabilities. That assumption can be maintained only by ignoring the literature on interpretations of probability, from which it is clear that several different kinds—or interpretations—of probability exist. DiVincenzo and Fuchs may have ignored the classic philosophical writings on the subject because they were written by philosophers for philosophers and so do not address the needs of physicists.

I have published a paper on the foundations of probability theory, written from the point of view of a quantum physicist.<sup>3</sup> I classify the main kinds or interpretations of probability into three groups: inferential probability, of which Bayesian theory is an example; frequency or ensemble probability, commonly used in Gibbsian statistical mechanics and in QM; and propensity theory. Propensity, a degree of causality that is weaker than determinism, is not merely another interpretation of probability. Its mathematical theory must also differ from that of probability theory, as Paul Humphreys showed<sup>4</sup> in 1985. Although the axioms of propensity<sup>3</sup> differ from those of probability, the two axiom sets overlap. Both support the law of large numbers, so propensity theory is compatible with the most useful part of the frequency interpretation of probability.

In general, QM states do not determine the results of a measurement, only the probabilities of the possible results. That a state's influence on the results is not deterministic suggests strongly that the quantum probabilities given by the Born rule should be interpreted as propensi-

ties. They refer objectively to the physical system and its environment, not to any agent's knowledge, so they are not naturally interpreted as subjective Bayesian probabilities.

Interpretations of probability may differ not only in philosophy but also in substance. As I discuss in reference 3, John Bell's theorem illustrates how local hidden-variable theories are incompatible with QM. E. T. Jaynes was a well-known supporter of the Bayesian theory of probability. In 1989 he repeated Bell's derivation of inequality but carefully treated all instances of probability as Bayesian. He found that the derivation could not be completed without invoking an extra assumption that was not justifiable in the Bayesian theory. Bell's theorem involves questions about causality, so it is natural to use propensity theory to treat it. That method is successful in deriving Bell's inequality.<sup>3</sup>

Not all probabilities occurring in QM can be treated as subjective Bayesian probabilities. That limitation disqualifies QBism, a Bayesian-based theory, as an interpretation of QM that can succeed in quantum foundations. The initial assumption of QBism is not valid.

## References

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► **DiVincenzo and Fuchs reply:** We deeply regret our oversight of Leslie Ballentine's influential 1970 *Reviews of Modern Physics* article on the ensemble interpretation. We were well aware of the paper but had not realized that it appeared in *RMP* so as to be appropriate for the retrospective. We apologize to Ballentine and to our readership.

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# Celestial background of 1869 eclipse

I enjoyed Deborah Kent's article on American efforts to document and study the 1869 total solar eclipse (*PHYSICS TODAY*, August 2019, page 46). At the April 2019 meeting of the American Physical Society, we were treated to a session titled “Centennial of the Eddington Eclipse Expedition.”

I'm curious. Were stars visible in any of the photos of the 1869 eclipse—or other eclipses in the days before general relativity? And would it have been possible that someone noticed the displacement of the stars' positions as Arthur Eddington did in 1919, but before Albert Einstein published his theory in 1915?

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► **Kent replies:** I'm glad Robert McAdory enjoyed the article. Although I'm not aware of any photos from the 1869 eclipse that show visible stars, there were images on plates from eclipses before 1919. Expeditions from the Lick, Yerkes, Smithsonian Astrophysical, and US Naval Observatories took large-format images of the corona during the 28 May 1900 eclipse, when the star field was similar to that during the 29 May 1919 eclipse. The images taken by Lick and their possible connections to the relativity test are explored in chapter five of Jeffrey Crellin's *Einstein's Jury: The Race to Test Relativity* (2006) and chapter two of *No Shadow of a Doubt: The 1919 Eclipse That Confirmed Einstein's Theory of Relativity* (2019) by Daniel Kennefick.

The 19th-century searches for an intramercorial planet resulted in many images in which some background stars might be visible. The *Lick Observatory Bulletin*, number 24 (1902), reported that half of the observatory's plates from the eclipse of 18 May 1901 included star images. That report also has more specific information about Lick's capabilities to capture stars in images.

*My thanks to Tom English of the Cline Observatory, Jamestown, North Carolina.*

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