Getting the drop on quantum droplets

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Changing the paradigm for research publishing

The commentary by Detlef Lohse and Eckart Meiburg, "On the quality and costs of science publication" (PHYSICS TODAY, August 2019, page 10), criticizes the Plan S initiative of some European funding agencies, which would require that results of publicly funded research be published in open-access journals. Lohse and Meiburg's points are fair, given the present research publication paradigm.

The point they are missing, to my mind, is that the paradigm is—and should be—changing. Many aspects of the present model come from the time when journals appeared only on physical paper, and some ways are so deeply rooted in the community that they are hardly questioned. Among them are the enormous and ever-growing number of journals and our reliance on the use of journal names to screen for quality. I think those practices are neither optimal nor indisputable.

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TODAY'S RESEARCH PUBLICATION MODEL was developed when journals appeared on paper only and submissions were either accepted or rejected. Perhaps it's time to consider a new model.

Instead of questioning Plan S based on the existing publishing model, I see the plan as an opportunity to revise the model. For example, a key criticism to open-access publishing is that it favors bad journals. That is, publishing in "good" journals that are highly selective is expensive, whereas publishing in non-selective "bad" journals is much less costly. That is true if we do not question the present accept-or-reject publishing model. However, if we consider using peer review for quality discrimination by grading papers instead of rejecting them, the scenario changes radically. Imagine an all-physics journal like the new *Physical Review Research* using grades for its papers to correlate with levels in the old model—including letters (grade 3), rapid communications (grade 2), regular articles (grade 1), and even higher and lower levels. Not only would such a journal be perfectly geared for Plan S, since the rejection rate would be minimized and publication costs thereby reduced, but it would also represent significant progress in research publishing, as explained in www.emilio-artacho.blogspot.com.

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Getting the drop on quantum droplets

The article "Ultradilute quantum droplets" by Igor Ferrier-Barbut (PHYSICS TODAY, April 2019, page 46) was really nice to see. It reported on the creation, at last, of real ultradilute liquid droplets and on the tremendous progress that has been made in that area. However, I was disappointed to see no mention or discussion that ultradilute liquid quantum droplets were predicted a long time ago.[1,2] That early work was a major source of inspiration for Dmitry Petrov's 2015 paper on the subject,[3] at least according to what he told me years ago.

In his discussion of mean-field quantum gases, Ferrier-Barbut doesn't note that the Efimov effect, which involves the creation of an infinite number of three-body bound states, can also allow Bose liquids to exist when the scattering length is negative (that is, when the two-particle interaction is attractive). Such a system is not always unstable. Tsung-Dao Lee, Kerson Huang, and Chen Ning

Yang (LHY) proposed in 1957 a leading-order correction to the mean-field approximation due to two-body collisions, which was used by Petrov;[3] 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,[1,2] the strength of three-body interactions can dominate over LHY corrections and can be infinite even if the two-body scattering length is finite.[1] A liquid model based on the Efimov effect[1,2] 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,[4] 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.[2]

Moreover, in a rather special system—an ensemble of spin-polarized tritium atoms—three-body recombination processes are most likely absent.[2,5] 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.

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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),[1] 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 understanding 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[2] 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,