Readers offer their own magic moments with John Bell

The article "Magic moments with John Bell" by Reinhold Bertlmann (PHYSICS TODAY, July 2015, page 40) was very enjoyable to read. As a research student in nuclear physics, I was fortunate to spend my final year, 1959–60, in the theoretical physics division at the UK Atomic Energy Research Establishment at Harwell, where I had my own "magic moments." Nominally, my supervisor there was Tony Skyrme, the brilliant inventor of a soluble field theory; skyrmions have had many applications in solid-state physics. However, I learned much more from conversations with John Bell.

In particular, during my career in research, I have tried to follow John's advice to construct the simplest possible model to deal with a problem. During that year at Harwell, many researchers in nuclear physics were trying to formalize an optical model potential that included the antisymmetry of nucleons in its derivation. As a referee of those attempts, John had constructed a simple model to test their validity. I recall that Herman Feshbach was among the several authors for whom John was able to point out where their derivation failed.

In the theoretical physics tearoom, John loved interacting and arguing with his colleagues—arguments he pursued totally without rancor. When someone voiced an opinion, he would sometimes express a contrary view just to enjoy an argument. That once upset a rather staid member of the division when John suggested that her desire for a pay increase was contrary to his view that those, including her, who loved their work should be paid less than those who did not.

John's presence at divisional seminars could be a serious trial for the visiting speakers, as both he and Skyrme were prone to question the basic equations they presented. I felt sorry for one speaker on numerical weather forecasting who was asked about the derivation and validity of the equations he was using. The ensuing discussion turned into one mainly between John and Tony as to whether the approximations used were justified.

I have always thought that John would have been an excellent teacher in a university and an inspiration to research students. It is good to know, however, that in addition to me, others enjoyed magic moments interacting with him.

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Here are some memories of my own "magic moments" with John Bell. I first met John in 1962, when I spent a sabbatical year at CERN from MIT. We had fruitful discussions on longitudinal sum rules arising from moments of the density–density correlation function in N-body systems, which were later incorporated into my book on statistical mechanics.[1]

The second time I saw John was in 1964, I think, at the physics summer school at the University of Washington in Seattle. Rudolf Peierls was also present, and the three of us discussed a question I brought up: why a point source of light on a lake shore produces a point image in calm water but a long pencil of light when there are ripples. We did not reach a conclusive answer.

On a weekend trip, I drove John and his wife, Mary, to visit Olympic National Park in Washington State. Coming back from a rest stop, John reported that a group of children had pointed at his beard and said, "Look! Hippie! Hippie!" John said children learn things fast because "childhood is boring." We talked about various subjects on that trip. He was a vegetarian because of Mary, but one time on the road he had no choice but to stop at McDonald's for a hamburger. "It was delicious," he said.

At the Pacific Ocean shore in the park, John decided he had to take a dip. We both waded in but could only stay a few minutes because the water was very cold. When we walked back to the car, there was another car beside mine, and an elderly couple had gotten out, apparently waiting to speak to us. The wife said that they saw the MIT parking sticker on my car; did I happen to know their son, a senior in physics at MIT? His name is Ising. I said, as a matter of fact, I did know a student named Ising. And she said, "This is my husband, Dr. Ising. He teaches physics at the Central College in Peoria, Illinois. Have you heard of the Ising model?" I said, "Of course! I wrote a textbook with a chapter devoted to it." Dr. Ising was diffident and embarrassed.

Reference

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I greatly enjoyed reading Reinhold Bertlmann's article "Magic moments with John Bell." Sprinkled with levity, it discussed Bell's life, work, and outstanding insights and achievements in the foundations of quantum mechanics (QM). In my view, the reading should also make one acutely aware that scientists, despite our expectations of them, are sometimes still prone to the same failings that beset the rest of humanity.

Bertlmann outlines very nicely Bell's theorem and inequalities and the consequences of their violation; that work led to the subsequent startling experimental results on "entangled" quantum mechanical states, which, Bertlmann writes, showed "that nature contains a nonlocality in its structure." Nonlocality, in Bell's conception of it, is clearly a violation of special relativity (SR), so we have a problem. The startling thing for me is that Bell actually does appear to solve the problem, at least in principle, but at the same time he seems afraid to say so and, in fact, distances himself from the proposed solution.

The essence of his ambivalence is conveyed in the following paragraph, quoted by Bertlmann:

> It may be that we have to admit that causal influences *do* go faster than light. The role of Lorentz

invariance in the completed theory would then be very problematic. An "ether" would be the cheapest solution. But the unobservability of this ether would be disturbing. So would be the impossibility of "messages" faster than light.

Clearly, faster-than-light propagation is *not* possible in SR. But it *is* permissible in the approximately 100-year-old ether-based Lorentzian theory of relativity (LR), developed by Joseph Larmor, Hendrik Lorentz, Henri Poincaré, and others. Bell was explicitly cognizant of LR and discussed it in his book *Speakable and Unspeakable in Quantum Mechanics* (2nd edition, Cambridge University Press, 2004). It is well known that LR and SR are totally congruent in their kinematics and dynamics, for speeds equal to or less than *c*. All predictions are identical. However—though one cannot find this in any standard textbooks—the LR ether theory differs from SR by allowing, without contradiction, speeds greater than *c*.

Historically, Einstein's SR gained ascendancy, and LR was relegated to the scrap heap of history for many reasons, but I think predominantly for the following. SR was viewed as being far more elegant and simple to develop—from the perspective of theory formation—because it followed deductively from Einstein's two simple axioms. On the other hand, LR theory was developed from a realism perspective, where waves were thought to need a medium to wave in (the luminiferous ether).

Considering that Bell was clearly aware of LR and was entertaining the idea, albeit reluctantly, of "causal influences [going] faster than light"—that is, being consonant with LR—I find it difficult to understand his expressions in the quote above about cheap solutions and disturbing features. It seems to be part of human psychology that once a set of ideas or a theory is accepted as valid for a long enough time by a community of people, as has been the case for SR, it gets transformed into belief, psychologically not unlike religious belief. Once that happens, those who would question the established dogma are treated as if the questioning was ill-intentioned, if not downright sacrilegious.

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■ **Reinhold Bertlmann's article** "Magic moments with John Bell" was a pleasure to read. I add a note concerning some later developments that are significant for understanding the implications of Bell's quantum foundations work.

The crucial point is that under the Bell locality hypothesis for a joint measurement as given in the article,

$$E(\mathbf{a}, \mathbf{b}) = \int d\lambda\ \rho(\lambda)\ A(\mathbf{a}, \lambda) \cdot B(\mathbf{b}, \lambda),$$

the quantity λ that represents the hidden variable(s) is a classical, not a quantum, object, and $\rho(\lambda)$ is a classical probability distribution. Such expressions only make sense in the context of quantum theory when the operators of interest commute with each other, but that is not the case when this expression is used to derive a Bell inequality.

Many years ago Arthur Fine pointed out that hidden variables and Bell inequalities were "imposing requirements to make well defined precisely those probability distributions for noncommuting observables whose rejection is the very essence of quantum mechanics."[1] Thus the real issue is not one of locality but, instead, the proper use of probabilities in quantum theory.

Fine's point has been confirmed by the later development of a fully consistent way of introducing probabilities in quantum mechanics.[2] That approach has shown that when quantum mechanics is interpreted using subspaces of the Hilbert space rather than classical hidden variables to represent physical properties, it is local: There are no ghostly nonlocal influences.

We cannot know how Bell would have responded to those new developments, which only came to full fruition after his untimely death. The reader willing to dig into my somewhat tedious discussion[3] of the Einstein-Podolsky-Rosen and Bohm situation will find an explicit proof that a measurement by Alice has not the slightest effect on the spin on Bob's side. More recent is a general argument that quantum mechanics satisfies a principle of Einstein locality: What is done to system A has no effect on system B as long as there is no interaction between the two.[4]

Those proofs of quantum locality begin by taking very seriously John von Neumann's proposal that quantum properties correspond to Hilbert subspaces, whose projectors do not, in general, commute with one another.[5] Bell, though, assumed that such properties can be represented by classical hidden variables, which always commute. Thus Bell's theorem teaches that if one uses classical hidden variables in place of quantum properties, the result will contradict quantum theory and disagree with experiment. Indeed, while Bell started off by pointing out deficiencies in von Neumann's argument against hidden variables, the end result of his work is an even better and much more conclusive argument, backed by experiment, for rejecting classical hidden variables.

References

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■ **I read with pleasure** Reinhold Bertlmann's article about remembrances of John Bell and his contributions to physics. Bell is famous for establishing an inequality between the prediction of quantum mechanics and local hidden-variable theories, and Bertlmann wrote that "so far, all experiments looking for violations in Bell inequalities have found them, so we have to conclude, along with John, that nature contains a nonlocality in its structure." But that conclusion is valid only in the context of Bell's conviction that classical "realism is the proper position for a scientist."

Experiments have revealed that the nature of reality in the quantum world is different from our experience in the classical world.[1] Properties like the direction of an electron spin and its position and momentum remain undetermined until a measurement has been performed,[2] but the colors of Bertlmann's socks are fixed after he puts them on. Albert Einstein's famous quotation, "spooky action at a distance,"[3] that appears in Bertlmann's amusing cartoon, is also misleading, because quantum correlations for entangled electron or photon pairs also occur at atomic distance of separation—for example, in the ground state of the helium atom. What would be spooky is if those correlations were altered when the entangled pair moved apart without further interactions, but experiments have shown that this is not the case.

References

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