Towards A More Realistic Interpretation of Quantum Mechanics

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What's Ultimately Possible in Physics?

Philosophers of science and theoretical physicists may eventually come to a common understanding of the implications of the experimental successes of quantum mechanics. Currently, however, they still have a lot to discuss, as can be seen by the variety of competing interpretations. Some of them even seem to relish rejecting the possibility of a realistic interpretation. I would like to remind them that the ultimate goal of physics is to discover the nature of reality. As Yogi Berra might say, "If you take away reality, what's left?"

We do need a definition of reality, though. It seems best to start with elementary particles. The reality assumption I am hoping can eventually be accepted views such particles as stable localized features of the metric of space, time and whatever other dimensions may exist. This view of reality is intended to reconcile input from all our senses, including artificial ones such as microscopes, oscilloscopes, telescopes, and so on. Therefore I accept special relativity, general relativity, and superposition as refinements that apply in situations that are not usually accessible with our natural senses directly. Such a realistic interpretation seems desirable because non-local interpretations cast a shadow of doubt on relativistic causality.

By the way, as my particles are of space and not merely placed in it somewhere, it should be clear that interchanging the locations of a pair of like particles does not result in a new configuration.

The EPR-Bohm Experiment

In the EPR-Bohm experiment as discussed by Bell [1], for example, the singlet state constructed from two spin ½ particles has no free parameters. We are free to express this state in a coordinate system that is aligned with our detectors, of course, but the singlet state takes the same form in any other (rotated) coordinate system. We are also free to orient the detectors with respect to some other conveniently observable physical system, such as the Milky Way for example. However, it follows from the isotropy and homogeneity of space that we are not able to orient the detectors in any absolute sense.

Now when one of the detectors, say detector 1, registers particle 1 as being oriented along some chosen direction, say Galactic South, we infer from the singlet state that detector 2 will respond to particle 2 as if it were oriented opposite to that direction, *i.e.* along Galactic North. This includes the Malus probability of $cos^2\theta/2$ where θ is the angle between detector 2 and Galactic North. The unconscious assumption is easily made that this perfect reproduction of the probability curve for all values of θ (in repeated trials) conveys more than one bit of information about the spin state of particle 2. Locality is also questioned because the result at detector 2 seems to reflect the setting of detector 1 even though the adjustment of detector 1 and the firing of detector 2 may be separated by a space-like interval.

But actually, a measurement on particle 2 can only reveal one bit of information about its spin state, as

Anton Zeilinger [2] emphasized in 1999. In this case, it responds as though initially, that is before the measurement, it must have been oriented opposite to particle 1. In other words, the measurement on particle 1 gives the one and only one bit needed to determine the spin state of particle 2. If we add the distributions for the two possible results at detector 1, we get $cos^2\theta/2 + cos^2(\theta+\pi)/2$, which is constant for all values of θ. That is, when both possibilities at detector 1 are post-selected, the distribution at detector 2 contains no angular information. It would be correct to say that the correlation in the data from the two detectors, after repeated trials, can depend only on the angle between their respective settings. One cannot conclude anything about spin states of the particles individually because they aren't even correlated with the initial state (because it has no angle dependence to be oriented), much less with any laboratory frame of reference.

Bell's Theorem

It is usually inferred that local realistic interpretations of quantum mechanics are ruled out by Bell's theorem. In the original version, Bell started with the following observation:

> “**Since we can predict in advance the result of measuring any chosen component of σ_2 (representing particle 2) by previously measuring the same component of σ_1, (representing particle 1) it follows that the result of any such measurement must actually be predetermined.**”

Bell then assumes that measurements at the two detectors depend on their settings and a common hidden variable. This seems like a plausible way to introduce hidden variables. Presumably, the hidden variable represents the actual state of the spins of the particles when they become separated and can no longer interact. From these apparently modest assumptions, Bell showed that the correlation of the firing probabilities of the two detectors set at different angles must differ from the confirmed predictions of quantum mechanics.

Note that if any component was actually predetermined, there would be a conflict with superposition. For example, suppose the x-component of σ_2 is predetermined (with the particles moving off in the $\pm z$-directions, say). The two possible eigenstates of the x-component are expressible as different superpositions of the eigenstates of the y-component. In either case, a measurement of the y-component will give ±1 with equal probability. Therefore the x and y components cannot both be predetermined simultaneously. Of course, Bell actually said any *chosen* component must be predetermined, which is not the same as saying that *any* component must be predetermined. This choosing involves alignment of the two detectors and, when that has been done, the correlation between the spin states of the particles can be reflected in the data.

Obviously, there must be some problem with Bell's assumptions. I think the problem is that Bell's assumptions do not guarantee conservation of angular momentum. It is not sufficient that aligned detectors respond oppositely. There are actually two possible ways of assigning the spin components of the two particles so that they cancel. An antisymmetric combination of these assignments is required if the initial state has zero total angular momentum.[3] The symmetric combination with canceling components along the chosen direction belongs to the family of assignments with total angular momentum J=1. However, the particles will always be emitted in the antisymmetric state because the initial state has zero total angular momentum. By the way, both of the superpositions with definite total angular momentum are unfactorizable in the spin degrees of freedom of the two particles and can therefore be said to be "entangled," which is a term that was introduced by Schroedinger.[4]

The problem for a realistic interpretation, though, is the putative non-local effect of the measurement of σ_1 on the state of σ_2. This hypothetical effect arises from Bell's inability to get the angular correlation to come out right using his hidden-variable assumptions. But since the initial state has no free parameters, there never was room for any hidden variables. I argue that Bell obtained his incorrect result by violating conservation of angular momentum. Still, it's hard to shake off the impression that the measurement of particle 1 influences the outcome at detector 2. The important lesson is that only one bit of information is needed from the outcome at detector 1. With this bit in hand, one can predict the output of detector 2. Without it, the output of detector 2 will be a random sequence of its two possible values.

What is ultimately mysterious here is probably the non-classical restriction of the result of a measurement of the orientation of a spin ½ particle to one of just two values along any given direction. Specifying a direction requires the same number of parameters as in classical physics, but specifying the possible results does not. Now this restriction to a finite number of values of the spin component happens to be a property of irreducible representations of the rotation group in 3 dimensions. The isotropy of space implies that we must conform to the rotation group, but the applicability of the finite representations to elementary particles was a major discovery. [5]

Half-odd integer representations of the rotation group have an additional non-classical property: they change sign under rotation through 360°. The fact that nature also uses these representations freely, in electrons for example, suggests to us, at least, that the Lie algebra that generates rotations is somehow more fundamental than our classical concept of 3 spatial dimensions. Now if we take the symmetries in nature to be suggestions for refinements of our classical notion of reality, there is a lot more to think about.

The SU(3)XSU(2)XU(1) symmetry underlying the standard model of elementary particles might suggest that extra dimensions could be added to the familiar 4 dimensions of space and time. Of course, string theory is such a program. I personally hope that particles will turn out to be topological features, perhaps in the metric of such higher dimensions, which happen to be caught partially in ordinary 3-dimensional space. This is the best chance I can see for reducing physical constants as well as the dynamics of physical systems to pure mathematics. Some string theorists evidently have a similar idea that equates my topological features with event horizons in higher-dimensional space-time.

The EPR Thought Experiment

The original EPR thought experiment [6] focused on the complementary momentum and position of particle 1. If it is assumed that the momenta of the particles are known (say because the energy of the metastable state would be divided equally between them) the dilemma is that the position of particle 2 can be inferred from the time at which detector 1 fires. Thus either the position of particle 2 is not real before detector 1 fires, or quantum mechanics is incomplete. This "realization" of the position of particle 2 would be an instant or "non-local" effect of the firing of detector 1. While this effect cannot actually be used to transmit information or energy faster than the speed of light, it suggests the presence of a conceptual flaw and raises doubts about realistic interpretations.

I claim that the flaw lies in assigning a variable x to the position of particle 2 before particle 1 is detected. My question would be, *x* with respect to what? It could only be with respect to the position of particle 2, which is equally unknown. Moreover, at the times in question, the experimenter doesn't even know if the particles have been emitted yet.

In the original paper, EPR actually assumed that the two particles were initially at rest at some definite separation without explaining how this initial state could be prepared. What was the reference point from which the position *x* of particle 2 is supposedly derivable (from the known position of detector 1 and the time at which it fires)? The initial state no longer exists, so its position at the moment that the particles ceased interacting cannot be measured after the fact. If it's position had been measured sometime before that, its position and total momentum (with respect to the laboratory) would have satisfied an uncertainty relation $\delta x\ \delta p \geq \hbar/2$. Of course, the uncertainty products of the particles individually, after they separate, cannot be less than this minimum.

It can also be said that EPR assumed, without justification, that their thought experiment takes place in the rest frame of the initial state and that the particles separate from some arbitrary position that is not known from interactions with other material bodies. I have to wonder how the founders of modern physics, especially Einstein, could have forgotten about the relativity of space? It had been clearly stated by Poincare [7]

> “**Whoever speaks of absolute space uses a word devoid of meaning. This is a truth that has been long proclaimed by all who have reflected on the question, but one which we are too often inclined to forget.**”

Writing this in 1897, Poincare seems to have been looking for a loose end that could be unraveled to make sense of the Lorentz-Fitzgerald contraction. He didn't find it, but he was able to anticipate the possibility of higher-dimensional and non-Euclidean spaces. Although Poincare rejected the concept of absolute space, he was unable to take the next step. Speaking of the Earth's motion relevant for the contraction:

> “**... its true velocity (I mean this time, not its absolute velocity, which has no sense, but its velocity in relation to the ether), this I do not know and have no means of knowing.**”

we see that it evidently appeared to Poincare that nature conspired to conceal this “true velocity” from all mechanical and electromagnetic means of detecting it. About eight years later, Einstein made the next cut [8]:

> **“...unsuccessful attempts to discover any motion of the earth relatively to the 'light medium,' suggest that the phenomena of electrodynamics as well as of mechanics possess no properties corresponding to the idea of absolute rest.”.**

By the way, Newton[9] imagined that physical quantities can be expressed in relation to absolute space and time, but these concepts were always controversial. For example, Julian Barbour's elegant exposition of the relativity of time [10] won the previous essay contest in this series.

Conclusion

The mystery of entanglement has been (at least partially) dispelled by the principle of the relativity of space. Consequently there is still room for a realistic interpretation of quantum mechanics.

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