LETTERS

CAN DEMOLITION OF THE 'ELEMENTS OF REALITY' PROCEED ON SCHEDULE?

I read with interest N. David Mermin's column "What's Wrong with These Elements of Reality?" (June 1990, page 9), in which he presents his nice refinement of the recent analysis by Daniel Greenberger, Michael Horne and Anton Zeilinger, which, he claims, "demolishes" the Einstein–Podolsky–Rosen "elements of reality."

Mermin's concluding recantation of his earlier statement that "no set of experiments, real or *gedanken*, was known that could produce such an all-or-nothing demolition of the elements of reality" definitely constitutes an admirable motion on the part of this scientist. At the same time it seems premature, as Mermin misses the following delicate point.

It is true that the state Ψ is the eigenstate of the commuting operators $S_{xyy} = \sigma_x^1\sigma_y^2\sigma_y^3$, $S_{yxy} = \sigma_y^1\sigma_x^2\sigma_y^3$ and $S_{yyx} = \sigma_y^1\sigma_y^2\sigma_x^3$, in which each of these operators has the eigenvalue $+1$. However, the product of these eigenvalues has nothing to do with the result of all nine measurements of individual spins of particles, as the state Ψ is not an eigenstate of individual operators σ_k^i. When one measures three spins of particles, as dictated by any one of the operators S_{xyy}, S_{yxy} or S_{yyx}, the product of the resulting three spin values indeed has the value $+1$. One can easily work this out upon considering the measurement S_{xyy} as a sequence of three steps where one measures first the operator σ_y^3, then the operator σ_y^2 and finally the operator σ_x^1, performing the appropriate reduction of the wave packet after each step. But after these measurements have been done, the system is no longer in the initial state Ψ. Therefore what Mermin considers is in fact three independent measurements of S_{xyy}, S_{yxy} and S_{yyx}, each measurement consisting of three spin measurements on the system described by the state Ψ.

But this is entirely different from measurement of all nine individual spins on a single system. When the analysis of the wave packet reduction is done for such a sequence, none of Mermin's paradoxes survives. This voids his refutation of the elements of reality.

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I read with pleasure David Mermin's Reference Frame column concerning the ingenious extension of the EPR argument by Daniel Greenberger, Michael Horne and Anton Zeilinger. However, I do not agree that the GHZ argument "demolishes the elements of reality."

When, many years ago, I learned about Bell's theorem, I deduced that quantum mechanics is wrong, because for me—as for Einstein, I believe—realism and locality are not renounceable. Here, "wrong" means "an extremely good approximation of a more correct local, realistic theory." (In a similar way, we know that Newtonian gravitation, being nonlocal, is wrong. It is just an approximation of general relativity. A very good approximation, indeed, because it allows calculating gravity on Earth with a relative error of about 10^{-11}, not so far from the accuracy of quantum electrodynamics, which is the paradigm for a precise theory.)

Like many people, I was not really worried by Bell's theorem until 1982, after Alain Aspect's experiment. Then I felt obliged to search for a loophole in the refutation of local realism. I discussed the problem with my colleagues Trevor W. Marshall and Franco Selleri, and we arrived at the conclusion that the best candidate was the loophole pointed out by John Clauser and Horne eight years before, which has to do with the low efficiency of photon counters. My view of this loophole is as follows: If something is wrong with quantum mechanics, it is likely the measurement theory rather than the evolution equations. (For instance, I do not want to reject the quantized Dirac plus Maxwell fields, which are responsible for the most spectacular successes of QED.) And the most likely mistake in quantum measurement theory is the underestimation of noise. In an Aspect-type experiment, for instance, blocking the loophole requires discriminating between 2, the maximum value allowed for some quantity by local realism, and $(\sqrt{2}+1)\eta$, the quantum mechanical prediction for that quantity when detectors of efficiency η are used. (Of course only efficiencies above $2/(\sqrt{2}+1)\cong 0.82$ will allow blocking the loophole, a well-known fact.) But a big increase in counting efficiency above the current value of $\eta \cong 0.2$ will lower the signal-to-noise ratio, as is the case for any alarm-type system. Noise will appear in the form of a high background counting rate, which we are not allowed to simply subtract if we want to block every loophole. Therefore I expect that noise will prevent a disproof of local realism when more efficient detectors are used, even without the need for a dramatic disagreement with quantum mechanics.

Certainly the situation has now changed, because the GHZ *gedanken* experiment seems almost insensitive to the efficiencies of the measuring devices. Again I feel obliged to find an escape. I am now convinced that, perhaps in addition to the problem of noise discussed above, what is wrong with quantum mechanics is the assumption that all vectors in the relevant Hilbert space represent actual states. This postulate was established by John von Neumann and, although recognizedly too strong, it was widely accepted as long as there was no reason to weaken it. The first weakening came with the discovery of superselection rules. Now there are reasons for further weakening, namely, the demand for depriving all vectors violating local realism of the condition of representing physical states.

Let us analyze, for instance, the state characterized by the wavefunction Ψ displayed in the unnumbered equation between (1) and (2) in Mermin's column. It exhibits only the spin part of the three-particle system, but from the description Mermin gives in ordinary prose, it seems that the spatial part contains the product of three Dirac's deltas determining the positions of the particles, or maybe three deltas in momentum space. In any case, the angular momentum of every particle should be highly undefined, its position or momentum being very well defined. The question is, How can you prepare a system such that the spin part of the angular momentum is well defined, but the orbital part is not defined at all, if the partition between spin and orbital is not Lorentz invariant?

From another point of view, we may state the problem as follows: Any test of locality should involve measuring positions as well as spins, polarizations or other properties. In the GHZ experiment we must measure, besides the operator $S=\sigma_x^1\sigma_x^2\sigma_x^3$, considered by Mermin, another operator such as $R=\rho_a^1\rho_b^2\rho_c^3$, where ρ_a^1 takes the value $+1$ (-1) if particle 1 is (is not) inside the small region a, and similarly for the other particles. For any actual preparation procedure of the three-particle system (by a spin-conserving *gedanken* decay, in Mermin's words) the probability that the measurement of S gives -1, conditional on a result of $+1$ for the observable R, will likely not be unity. It is far from obvious that this probability cannot be reproduced by a local hidden-variables model where spin and linear momentum are conveniently entangled. Consequently, I remain unconvinced that, parodying EPR, "quantum mechanical *destruction* of physical reality can be considered complete."

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MERMIN REPLIES: The nine successive spin measurements that Mikolaj Sawicki contemplates have nothing to do with the EPR argument. The crucial property of the GHZ state for making the EPR argument is simply that regardless of which particle (1, 2 or 3) or spin component (x or y) you are interested in, as a matter of perfectly orthodox quantum mechanics the result of measuring that particular spin component of that particular particle can be determined in advance by measuring an appropriately chosen spin component of each of the other two faraway particles. No other measurements are made, except, if you want, for a final measurement of the spin of the original particle to check that the prediction was indeed correct. There are only three spin measurements on three distinct particles associated with three commuting observables.

As Sawicki points out, however, you cannot do all the measurements necessary to learn the values of both spin components of all three of the particles. This is also correct, and has something of the flavor of Bohr's reply to EPR, who actually anticipated this objection in their original paper. Their compelling rejoinder, in the terms of the GHZ setup, is that if you agree (as quantum mechanics does) that a given spin component has a definite value only if you have actually determined that value by two faraway measurements, but insist that it does not have a definite value if you have not, then whether or not a spin component of a given particle has a value depends upon your choice of what to measure far away from that particle. This struck them as entirely unreasonable, whence their conclusion that both components must have had values in advance of the measurements, even though only one of those values can actually be determined. [Those who say (as many do) that there is nothing spooky about EPR, since the actual measurements merely give us additional information about the faraway particle, are implicitly embracing the EPR position, but simply refusing to take the next step.] GHZ refute EPR not by the irrelevant (from the EPR point of view) fact that you cannot do all the measurements needed to reveal all the values, but by the elementary observation that there is no possible way to assign all those values that can produce the right data for each of four different choices of what to measure in each of the three far-apart wings of the experiment.

A clarification of this issue might be found in *American Journal of Physics* **58**, 731 (1990), where I describe how to extract the EPR argument and its subsequent refutation directly from the data produced by spin measurements in the GHZ state, avoiding any reference to quantum mechanics, except for an unproblematic calculation of those data.

Emilio Santos is too quick to dismiss the ability of detector inefficiencies to cover a variety of conspiracies. If in each run of the three-particle GHZ experiment one of the particles (1, 2 or 3, randomly selected) is designed so as to evade detection if its spin is measured along a particular one of the two directions (either x or y, randomly selected), then it is easy to specify the other five "elements of reality" m_μ^i so that the data I described are always observed in those runs in which all three detectors do fire. Whether or not one finds loopholes like this attractive depends on whether one is more appalled by quantum nonlocality than by particles that, in order deceptively to imitate quantum nonlocality, conspiratorially exploit our failure to realize that our detectors are more efficient than we thought they were.

Santos's other suggestion, that there should be something like superselection rules prohibiting states like those of EPR and GHZ that demonstrate "spooky actions at a distance," has been argued with great eloquence and fervor by Oreste Piccioni. Santos's specific suggestion that constraints between spin and orbital angular momentum might do the trick is ruled out by a new version of the GHZ experiment proposed by Greenberger, Horne, Abner Shimony and Zeilinger (to appear in the *American Journal of Physics*), in which spin (or polarization) plays no role.

Finally, I must emphasize that although "the unnumbered equation between (1) and (2)" was undisplayed and therefore not in violation of Fisher's rule (see my Reference Frame column, October 1989, page 9), I hereby enunciate and plead guilty to a violation of Santos's rule (display—and, of course, number—all equations you think readers might want to refer to, whether or not you think they should) and promise to try not to do it again.

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Enumerating α's Calculators

The interesting Reference Frame column by David Gross on the calculation of the fine-structure constant α (December 1989, page 9) mentions Paul Dirac's matrices but not Dirac's work on the problem. Dirac[1,2] sought to explain why the smallest electric charge e was given approximately by $1/\alpha=\hbar c/e^2\approx137$. By considering the wavefunction of an electron in the field of a magnetic charge g he obtained the (charge quantization) relation $eg/c=n\hbar/2$ (where n is an integer) connecting the two charges, but not a relation in e alone. This was a great disappointment to him. Still, he made the best of it and published in 1931 a paper[2] that has stimulated much of the work on magnetic monopoles. (The introduction to this paper reads like an epic poem and does not betray Dirac's disappointment.) Although he thought in 1931 that magnetic monopoles might exist, he was "inclined to believe that monopoles do not exist" on the eve of a conference in Trieste celebrating the 50th anniversary of his 1931 paper.[3] However, he remained fascinated with the fine-