Santos Replies: Although I do not understand completely the Comment by Ben-Ayreh and Postan [1], it seems that their essential point is the same as that of Rae [2], namely, that for a suitably defined ensemble of photon pairs the Bell inequality is violated. Before addressing this point let me clarify two preliminary and more fundamental ones.

The relevance of Bell's theorem derives from the possibility that it discriminates *empirically* between the whole family of local hidden-variable (LHV) theories and quantum mechanics (QM). Now the first point: If, for a given experiment, there exists a LHV model which makes the same predictions as quantum mechanics for *all quantities actually measurable*, then such an experiment cannot discriminate between QM and LHV theories, and this is true without any need of further discussion about Bell inequalities. Second point: If there are two theories, say A and B, such that no real (as opposed to *gedanken*) experiment is possible and able to discriminate between both theories, then A and B are compatible from the point of view of physics (as opposed to metaphysics). I hope that the authors, and readers, agree up to here.

First point.— In my Letter [3] I exhibited a LHV model for experiments measuring the polarization correlation of optical photon pairs that is in agreement with QM for all measurable quantities (the model did not agree exactly because the angular correlation factor a was set equal to 1 for simplicity; a more sophisticated model which also reproduces this factor has been presented recently [4]). The agreement with QM includes the quantity $p(u_1 a_1, u_2)$ calculated by Rae (unnumbered equation) and by Ben-Ayreh and Postan [their Eq. (4)], which was not calculated in the Letter, but can be trivially obtained from my model Eq. (14) with $\Omega/4\pi$ substituted for $P(\lambda_2, b_2)$. Consequently, *these experiments are not suitable tests of LHV against QM.*

Second point.— At present, 27 years after Bell's work, there is no real experiment, performed or planned, able to discriminate between QM and LHV. Therefore, *the question of whether QM and LHV are compatible remains open* (which obviously implies that *LHV theories have not been refuted,* in spite of repeated claims of the opposite in Physical Review Letters and other journals and books).

Having made these two points, I address the specific criticisms made. For an easier discussion, let us assume perfect polarizers and detectors, with apertures $\Omega \ll 4\pi$. It seems that we may use two different ensembles of photon pairs for the definition of probabilities: (a) the ensemble of all pairs produced by decaying atoms, or (b) the subensemble of these pairs in which each photon has passed the corresponding aperture. In case (a) the coincidence probabilities are a factor of order $\Omega/4\pi$ smaller than the single probability (due to the poor angular correlation between the photons) and the Bell inequalities are well satisfied. In case (b), single and coincidence probabilities are of the same order and QM probabilities

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violate the Bell inequality. We conclude that Bell's theorem does not forbid LHV models reproducing the QM results, provided it is required that they describe only the total ensemble (a), but not the "passed subensemble" (b). The model presented in my Letter does that at the price of assuming a photon detection probability with the polarizer removed $[P(\lambda,0) = \Omega/4\pi$ if it is defined for the total ensemble (a)], which may be greater than the probability with the polarizer in place $[P(\lambda,a)$ given by Eq. (12) of my Letter]. That is, for some values of λ my choice violates the *Clauser-Horne "no-enhancement" assumption* [5]

$$P(\lambda,a) \leq P(\lambda,0).$$

This implies that the model is not compatible with the existence of the passed subensemble, since for such a subensemble we would have, with perfect detector efficiency, $P(\lambda,a) = 1$ and hence $P(\lambda,0) > 1$.

In summary, I agree with the authors of the Comments that the Bell inequality, applied to the passed subensemble, is violated by QM predictions. But I should add that this violation is irrelevant from the point of view of physics, because the "probabilities" involved cannot be measured, as they refer to a hypothetical ensemble not defined operationally. It might be believed that the coincidence probability for the passed subensemble could be measured as the ratio of the coincidence counting rate with both polarizers in place and the coincidence counting rate with one polarizer removed, as is apparently suggested by Ben-Ayreh and Postan. I do not agree. This is a ratio of probabilities, not a probability. Then I argue, following the best tradition of Bohr, that QM should not be interpreted as making predictions for the (purely imaginary or metaphysical) passed ensemble and, consequently, that there is no contradiction between QM and LHV theories for these experiments.

Rae defends the existence of such an ensemble by saying that "light can reach the detector only by passing through a lens or aperture and there is no alternative path." This argument is typical and it has been used, implicitly or explicitly, by most authors dealing with the matter. But it is inconsistent. In fact, the whole argument of Rae (or Clauser, Shimony, etc.) appears to be as follows:

(A) *Nature is local.* Therefore, if something happening in the source produces an effect in the detector, it cannot be by an action at a distance, but, rather, something (a photon) should travel from the source to the detector. As the velocity of light cannot be surpassed, the travel should be more or less in a straight line, that is, through the apertures.

(B) As a photon is localized at birth (at an atom) and localized at death (when detected), it should maintain localization through its whole life. Then either it passes fully or not at all through the apertures.

(C) We define the subensemble of pairs passing through the apertures and, applying Bell's theorem, we

conclude that *nature is nonlocal.*

What I do not understand is why if Rae is able to accept (C), he does not reject (A) and (B). Up until now I find no reason to reject (A) and so I am compelled to reject (B). But then I argue that this rejection fits fairly well with the quantum formalism. In fact, as I stressed in my Letter, QM needs to define probabilities only between the (initial) preparation and the (final) measurement. For instance, in the two-slit experiment, QM allows us to calculate the *amplitude,* say ϕ_1, for a photon of the source going to the upper slit or to the lower slit, ϕ_2. Also, QM allows us to calculate the *amplitude* for a photon going from the upper slit to the point x on the screen, say $\psi_1(x)$, and similarly for the lower slit, $\psi_2(x)$. However, although in QM all probabilities are squares of amplitudes, not all amplitudes squared should be taken as probabilities. In the two-slit experiment, $|\phi_1\psi_1(x) +\phi_2\psi_1(x)|^2$ is a (measurable) probability, but neither $|\phi_1|^2$ nor $|\phi_1\psi_1(x)|^2$, etc., is measurable (I mean in the same, interference, experiment). If one insists in taking them as probabilities, then one is led to the necessity of changing the laws of probability, or even logic, in dealing with quantum mechanics. Such a dramatic change should be avoided, according to Occam's razor, not being strictly necessary. (This does not exclude the usefulness of quantum logic and quantum probability in purely formal approaches.) Consequently, we should not assume that "the subensemble of photons passing through the upper (or lower) slit" does exist. We should assume that all photons pass, in some sense, through both slits (or better, we might avoid speaking about photons at all).

Also in the (atomic cascade) experiments that we are discussing, we should assume that all photons pass partially, in some sense, through the apertures, and this follows naturally from the quantum formalism. In fact, quantum electrodynamics (as well as the classical theory) shows that an emission in the source produces an electromagnetic field which propagates—according to Maxwell's equations even in the quantum case—through the apertures and, possibly, polarizers to the detectors. There, as the field is quantized, it does not act deterministically, but statistically, and all we can calculate is the probability of a count. For a popular explanation it may seem good (in my opinion it is very bad) to say that this is because the electromagnetic quantized field consists of a set of particles (photons), but this does not follow strictly from QM. Indeed, photons are not particles, but (nonlocalized) quanta of the electromagnetic field.

I conclude by using this opportunity to put my Letter in due context by comparing it with the well-known article of Clauser and Horne [4].

The first half of my Letter was devoted to showing that atomic cascade experiments are not suitable tests of LHV theories against QM due to the poor angular correlation of the photons involved. I acknowledge that this fact had

of the photons involved. I acknowledge that this fact had been already very clearly stated in the paper by Clauser and Horne. My difference from these authors on this point may be qualified as a matter of opinion, but I think it is important. While they attempted to solve the problem by deriving new tests with the introduction of "plausible auxiliary assumptions," in my opinion plausibility cannot be used as a valid criterion of scientific truth. Consequently I have attempted to make clear that LHV theories might only be refuted if there were a *real* experiment where no LHV model is possible that makes the same predictions as QM for *measurable quantities.*

In the second part of my Letter, I exhibited a LHV model for atomic cascade experiments which agrees with QM even for perfect apparatus. (After publication of the Letter I realized that the model was inspired by a previous one used by Caser [6] for a different purpose, which this author had discussed with me several years ago.) In contrast, Clauser and Horne were only able to find a model resting upon the low efficiency of the available photon detectors (their model was untenable with efficiencies above 38%). In my view this is substantial improvement because the Bell inequalities are not sufficient conditions for the existence of LHV models, and before my Letter it was unknown whether they actually existed in agreement with QM *even for ideal polarizers and detectors.* Indeed, the lack of such a model has misled most people to believe that LHV theories have already been empirically refuted, modulo the low efficiency loophole. This wrong belief, not attributable to the paper of Clauser and Horne, has stooped the search for truly reliable experiments, and so it has delayed the solution of an extremely important open problem, by almost a quarter of a century.

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[6] S. Caser, Phys. Lett. A 121, 331 (1987).