A Non-Factorable Local Realist Model for EPR Experiments with Timing Variations

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ABSTRACT

The violation of Bell's inequalities in some EPR-type paired-photon experiments could be as much due to the sensitivity of the coincidence monitoring system to timing variations as to other causes of "enhancement". If there are systematic variations of processing time with polarisation angle, then the "factorability" assumption may not hold if window size and other parameters are not carefully chosen. The effect is illustrated using the chaotic ball model introduced in an earlier paper. Some little known facts from Alain Aspect's thesis are presented. It is argued that, though Aspect took considerable care with his choice of parameters, he did not take into consideration the factors discussed here and the experiments may in consequence have suffered bias. Part of this story has been known for some time, but appears to have received little attention. Further experimentation is needed.

INTRODUCTION

It is commonly believed that the assumption that must be dropped in order to give a local realist explanation for, say, Aspect, Dalibard and Roger's (ADR) 1982 results (Aspect et al., 1982) is "no enhancement". There is another possibility, which can arise surprisingly naturally: real experiments may not be "factorable"! Non-factorability does *not* always imply non-locality. When John Bell derived his inequality (Bell, 1964), he was assuming an idealised situation in which there was no need to take account of *time*. It will be seen that we have only to make realistic allowance for slight timing variations to find that we no longer have statistical independence between the two sides of the experiment: we have an active interaction at the coincidence monitor, so it is no longer reasonable to expect to find factorability. A few figures from Aspect's more detailed description in his thesis (Aspect, 1983) of the same experiment, and some plausible but untested realistic conjectures about the behaviour of actual apparatus, suggest that the possibility of serious bias – possibly sufficient to account on its own for the observed violations of Bell inequalities – cannot be ruled out.

The fact that timing variations can affect the results of Einstein-Podolsky-Rosen-type (EPR) experiments was expressed very clearly as long ago as 1982 (Fine, 1982; Pascazio, 1986). It seems largely to have been ignored, perhaps because the general area of the definition of time in these experiments is of such technical and theoretical difficulty (Pascazio and Reignier, 1987). This paper attempts to show, however, that the underlying logic is quite simple, so long as it is recognised that it takes *two* parameters to define a coincidence window, not just one. We need to specify a start time relative to a conceptual zero — the time corresponding to exact simultaneity — as well as a duration. There is an assumption in quantum electrodynamics that the origin of time for the second light signal of a cascade is the moment of "collapse of the first photon's wave function", *i.e.* the moment when the first light signal is *observed.* In actual experiments, however, this is quite evidently not the case, as the "second" signal can easily be detected before the "first"! This is commonly accepted as being due to imperfect time resolutions, with timing variations assumed to be random. It is suggested in this paper that they may be largely systematic, and that the particular type of polariser used may introduce additional bias. It so happens that all sources of bias that I have considered tend to act in the same direction, increasing the violation of Bell's inequalities and thus giving false evidence in favour of the continuation of the quantum myth of "non-local" effects.

MODIFIED CHAOTIC BALL

The "chaotic ball" analogy used in an earlier paper (Thompson, 1996), illustrating the bias that can be introduced in the "standard" ($-2 \leq S \leq 2$) Bell's inequality when there are missing values or variable detection probabilities *, can be modified to show the effect of non-random timing variations. The analogy is closer to the geometry of an idealised Stern-Gerlach experiment than to that of the paired light signals that have been used in actual "successful" EPR experiments, but the principle involved is the same.

We have, as before, assistants Anne and Bob, who stand and look at a ball on which are marked S at one point, N at the opposite point. In this version, we assume that the ball moves chaotically, with its centre fixed, then *stops*† while the assistants press buttons to relay back to a coincidence monitor whether they saw S or N. Anne looks immediately the ball stops, Bob one second later. The monitor is set to open a window when it receives Anne's signal and close it either when it receives Bob's or when the window time interval (4 secs, say) is up.

Now, I shall not try and say what exact mechanism I am assuming (something with mirrors should do the trick), but the important part of this model is that we assume that Anne and Bob do not have fixed reaction times. There are certain regions in our probability space (the space of possible ball positions) such that Anne will react early ($E = 2$ sec reaction time, say) and others to which she will react late ($L = 6$ sec reaction time, say), and likewise for Bob, who has *identical* reaction times in response to the same relative positions of the ball. The regions are thus assumed to be determined by the relative geometry, as shown in Fig. 1. A quick guide for determining when there is a coincidence is given in Fig. 2, which shows in addition an X category, to be explained later.

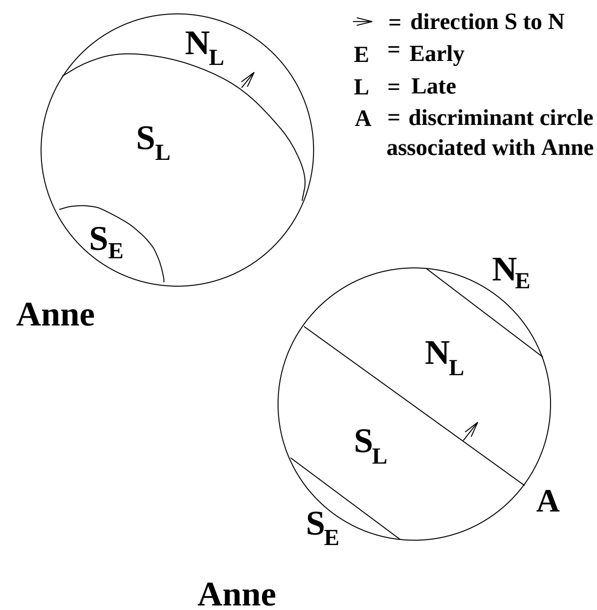

Fig. 1. Two views of chaotic ball with timing variations

Interval A to B at coincidence monitor

Anne \ Bob	X	E	L
X	1.0	2.5	6.5
E	-0.5	1.0	5.0
L	-4.5	-3.0	1.0

Probability of coincidence

Anne \ Bob	X	E	L
X	1	1	0
E	0	1	0
L	0	0	1

Initial interval : 1.0 sec **Window : 4.0 sec**
X: "no polariser" : 0.5 sec
E: "Early" : 2.0 sec
L: "Late" : 6.0 sec

Fig. 2. Coincidence probabilities

It is easily seen that when Anne and Bob stand either at

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*I define the "standard" Bell inequality as the one involving the conduct of four sub-experiments using 2-channel detectors. S is:

$$E(a,b) - E(a,b') + E(a',b) + E(a',b') \qquad (1)$$

where a, a', b, b' are detector settings and E is officially (Selleri, 1988, page 19) estimated by $(N_{++} + N_{--} - N_{+-} - N_{-+})/T$, where T is the total number of pairs emitted. In practice the "normalised" formula, in which T (which is not known) is replaced by the sum of the terms of the numerator (which *is*), is used instead. This is never justifiable in real experiments, and the inequality becomes meaningless.

†If we do not assume that it stops, we can have a more realistic model in which the source atom may be pictured as rotating slightly between the emissions of the two signals. This will reduce the correlation between them. For the purposes of the present paper, this would confuse the issue: we wish to look at the effect of systematic timing variations originating at the detectors, uncontaminated by any other sources of variation.

the same spot (not actually possible, but they must do their best!) or exactly opposite, the fact that response times vary makes no difference at all to their scores. For a given orientation of the ball, they either both respond early or both late. Their "synchronisation" is unaffected. Thus, if $C_\Phi(SS)$, for example, stands for the number of coincidences of type {Anne:S, Bob:S}, when their relative angle is Φ, then we would expect to find approximately, for $\Phi = 0$,

$$C_0(SS) = T/2,$$

where T is the total number of pairs. We are assuming here that there is no completely lost data, just some that gets processed slowly.

When the assistants stand "at right angles", however, if we look down on the scene and visualise the different regions we get the picture given in Fig. 3.

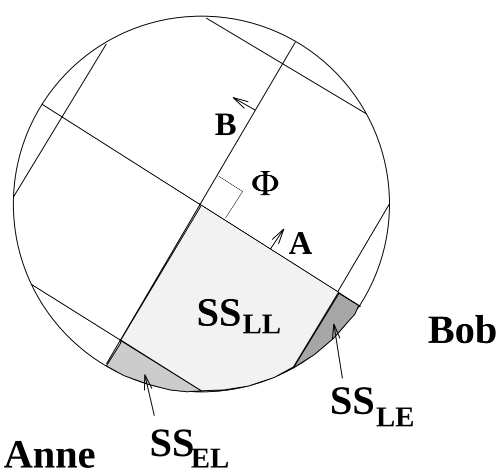

Fig. 3. Time variations: 90^0 orientation

We now find that Anne and Bob are poorly synchronised when the ball is in certain positions. So poorly, in fact, with the delays and coincidence window used here, that *no* coincidences are scored in these positions. So our expected number of coincidences is less than the $T/4$ value that the absence of ordinary missing values would have led us to expect. Thus

$$\begin{aligned} C_{\pi/2}(SS) &= SS_{LL} + SS_{EE} \\ &= T/4 - SS_{EL} - SS_{LE}, \end{aligned}$$

using the notation shown on the diagram.

Of course, in real EPR experiments, one would hope that the various times involved – interval between emission of the two signals, timing variations during flight, and coincidence window[‡] – would not combine to produce this unfortunate result, but there is no way of knowing from the published data whether or not this effect exists. Mechanisms that might produce it, at least in some mild form, are discussed later.

If such an effect exists, then it is easy to see that it will tend to make coincidences for fairly large values of Φ relatively smaller than those for very small Φ. This could make the value of the Clauser/Horne/Shimony/Holt (CHSH) test statistic[§] larger than it "should" be, thus contributing to the violation of the test. (This timing effect could also come into play in other Bell's tests, but those that involve both + and − results are, in practice, sensitive to ordinary missing values, and the effect

[‡]In actual experiments (see later) there is a second controllable parameter associated with the coincidence system: a delay (that can be negative) applied to one or other path so as to eliminate the more extreme of the out-of-order events – those that produce large negative intervals.

[§]The version of the CHSH test used in ADR says that, given that the assumptions of "no enhancement" and "factorability" are true,

$$-1 \leq S \leq 0,$$

where

$$\begin{aligned} S = & \tfrac{N(a,b)}{N(\infty,\infty)} - \tfrac{N(a,b')}{N(\infty,\infty')} + \tfrac{N(a',b)}{N(\infty',\infty)} \\ & + \tfrac{N(a',b')}{N(\infty',\infty')} - \tfrac{N(a',\infty)}{N(\infty',\infty)} - \tfrac{N(\infty,b)}{N(\infty,\infty)}. \end{aligned}$$

N is the number of coincidences, a, a', b, and b' are polariser settings and '∞' indicates the absence of a polariser. The dashes on the signs refer to different routes, details of which are in the original paper. The version of the test statistic actually used may be written:

$$\begin{aligned} S = & \; 3R(\tfrac{\pi}{8}) - R(\tfrac{3\pi}{8}) \\ & -(\text{two terms with polarisers absent}), \end{aligned}$$

where R replaces a ratio of Ns and the pairs of angle arguments are replaced by the angle between them (this assumes rotational symmetry, as is customary).

The test that can be violated in real experiments is the right hand side of the inequality: we can find $S > 0$. If we assume, as seems reasonable, that there are no systematic differences between the various $N(\infty,\infty)$ denominators, then the fact that it is only the sign of S that is of interest means that we can, for most purposes, restrict attention to the numerators.

of these is likely to dominate.)

Thus variable response times can increase the likelihood of violating the CHSH test. Have we violated any of its assumptions? We certainly have *not* done anything "non-local". If we look into matters, we find that situations of the above general type involve a failure of the "factorability" assumption. It is no longer reasonable to expect that the probability of a coincidence should be simply the product of the probabilities of the two "singles", for we can no longer ignore the fact that, to get a coincidence, we need *three* events to happen:

- Anne sees S
- Bob sees S
- Their signals reach the monitor in correct order (A then B) and within the correct time interval (the coincidence window, 4 secs)

For the angles considered so far, it so happens that it *would* be possible to "factorise" the coincidence probability (though the factors would not be simply the probabilities of the singles), but there are certain ranges of Φ for which it is not possible.

Consider the situation shown in Fig. 4. Define

$$p_A(\lambda, a) = \begin{cases} 1 & \text{where Anne sees } S \\ 0 & \text{elsewhere} \end{cases}$$

$$p_B(\lambda, b) = \begin{cases} 1 & \text{where Bob sees } S \\ 0 & \text{elsewhere} \end{cases}$$

$$p_R(\lambda, a, b) = \begin{cases} 1 & \text{in } S_{EE} \text{ and } S_{LL} \text{ regions} \\ 0 & \text{elsewhere} \end{cases}$$

where λ is the vector from S to N and a and b specify the directions in which the assistants are looking. The symbol p_R signifies the "probability of recognition by the coincidence monitor".

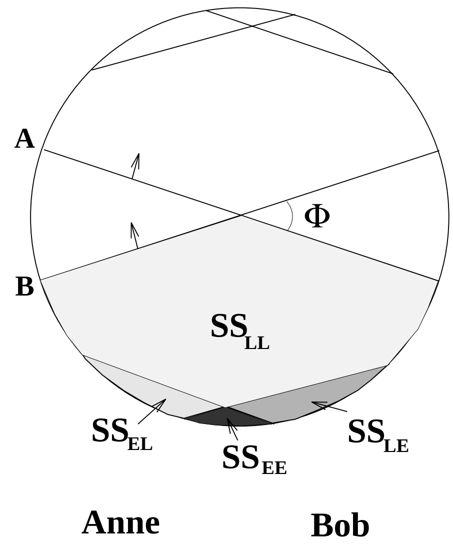

Fig. 4. A non-factorable situation

The expected value of $C_\Phi(SS)$ can be written:

$$\frac{T}{4\pi}\int_\Lambda p_A p_B p_R d\lambda,$$

where Λ is the whole surface of the ball (or, rather, of the abstract fixed sphere that is to be imagined surrounding the ball), and we have assumed, as usual, a uniform distribution of orientations.

It can be seen from the diagram that p_R cannot be expressed as a product of functions of A and B parameters separately: it is not factorable, so neither is the complete integrand**.

What happens if we vary our parameters?

We have produced non-factorability and biased estimates of coincidence numbers because we have set the initial

**It is perhaps more natural to continue to think in terms of two factors, but to recognise that these are not both probabilities in the usual sense and the second depends on a as well as on b and λ. This is the course chosen by Santos (Santos, 1984), who investigates a class of timing effects that is, in a sense, much broader than that considered here, but which is restricted by the assumption that the time from the source to the detector is constant.

time between the signals, the possible delays and the coincidence window so that it is very easy for signals to arrive at the monitor out of order or too late. We could have avoided this. We could have, for example, artificially put in an extra delay for the B signal, to make sure it was *always* after the A one (ADR's logic was slightly different, but an equivalent option was open to them, as discussed later). If we also increased our window sufficiently, we could ensure that all pairs registered, however late. In effect, we could make our time mechanism much more "coarse-grained", so that it was no longer sensitive to the time variations. This could completely eliminate this source of bias.

Summary so far

Failure of factorability need not be anything to do with non-locality. It can arise in this perfectly ordinary macroscopic *gedankenexperiment*, and is directly attributable to the facts that (a) we are using a coincidence monitor and (b) we have time variations that depend on our "hidden variables". The hidden variable in this case is, of course, simply λ, the orientation of the ball. Timing variations do not always cause bias. It all depends on the relationships between the various times.

Enhancement

A further modification of our analogy will allow us to see that timing variations can be a source of "enhancement††", so that they can help to make the last two terms in the CHSH inequality (see footnote earlier) smaller than they should be, contributing to the likelihood of violation of the test. A suitable modification would be to model "absence of polariser" by the rule that the assistants press a third button as soon as the ball stops, regardless of which letter they see. Their reaction time could be assumed to be $X = 0.5$ sec, say, faster than either E or L. (This explains the X entries on Fig. 2.) It is easy to see that this will result in many signals out of order or relatively late when one assistant is using this rule and the other is behaving as before, "set" in a particular position and making decisions based on what he or she sees. Thus the number of coincidences will be very low or, equivalently, the relative number of coincidences with "both polarisers in place" will be high: we have "enhancement".

††"Enhancement" can be defined as the existence of λ values such that

$$p(\lambda, \infty) < p(\lambda, a),$$

i. e. for these λ, a detection is *less* likely if the polariser is removed.

PHYSICAL CAUSES OF TIMING VARIATIONS

According to quantum theory, dating from experiments in the 1920s, timing variations, if they exist at all, can only be random, because all "photons" are the same.

However, semiclassical methods suggest that there may be various reasons why light signals that are polarised near the direction of the polariser axis may produce photomultiplier output slightly earlier (of the order of 1 ns‡‡) than those at other angles.

This could be simply because they have greater intensity. The photomultiplier is likely to respond when only the first fraction of the wave system has reached it. Although the rest of the wave may have the potential to trigger an output, it tends to arrive to find that the instrument is "dead", having already responded. When a weaker signal arrives, however, the first fraction is less likely to trigger a response, so later parts have more chance. This argument is similar to one used recently by Raymond Chiao in relation to "faster than light" signals (Brown, 1995).

There are other possible reasons, related to accepted characteristics of photomultipliers and discriminators, for strong signals to be detected relatively early. It is

‡‡A variation of one nanosecond may sound trivial, but we are talking here of systematic bias, not random error. Whether or not the variation matters depends on factors such as the shape of the time spectrum of the emission interval between the light signals. Is this shape known? It is taken in Aspect's work to be Poisson with half-life of 5 ns, but, when all is said and done, this assessment must have been made using photomultipliers!

known[§§] that there are timing variations (of order 0.6 ns) associated with primary electron emissions from the centre and the border of the photocathode. These could be relevant if considered in conjunction with the effect of the polariser, as discussed presently. There can also be variations of order 0.35 ns for central primary emissions.

An interesting, and possibly related, fact is that the timing resolution of a photomultiplier improves as the voltage across it is increased. Thinking in wave terms, would not an increase in amplitude of the input signal be expected to have a similar effect to an increase in voltage? And, indeed, in a sense this is well known, in that, as Aspect comments, nuclear physicists, dealing with situations in which large numbers of primary electrons are associated with a single input signal, find that they can achieve resolutions of 0.1 ns (using constant fraction discriminators?). As Aspect firmly declares, *his* inputs each generate only *one* primary electron. A realist must needs question whether this has ever actually been proved?

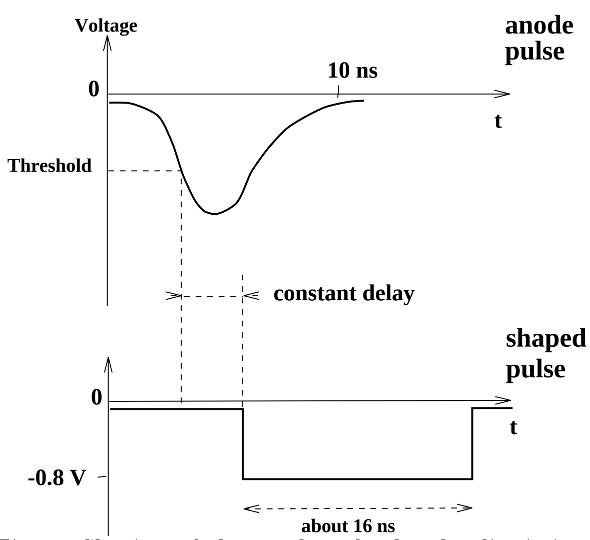

Fig. 5. Shaping of the anode pulse by the discriminator. (*copied from Aspect's thesis, page 214, fig. VI-12*)

[§§]Figures quoted here for precisions are taken from Aspect's thesis, using sometimes manufacturer's specified figures and sometimes his own estimates. They are in broad agreement with those quoted by Kleinknecht (Kleinknecht, 1986), but it is likely that more recent experiments will have better precision.

Moving on to the discriminator (the thresholding device that shapes the initially very variable photomultiplier output pulses), modern ones reduce the timing variations caused by variations in photomultiplier anode pulse sizes by using a "constant fraction" system instead of a fixed threshold size as illustrated in Fig. 5. Aspect states that he tried such a device and found no improvement in resolution times, so he returned to using a fixed threshold. It is not entirely clear, though, if his comparisons were made in actual experimental conditions, with polarisers in place. It would be understandable if he considered it unnecessary to pursue this question far, as his estimates for variations at this stage were of order 0.1 ns, which is negligible in comparison with the photomultiplier figures.

Another question that might be relevant to timing is whether the shape (the time-profile) of the wave system could be be transformed by the polariser? We are talking here of pile-of-plates polarisers, about 1m thick, comprising many thin plates set at the Brewster angle, with space in between. The spaces are supposed to allow the "unwanted component" to reach the walls, which should absorb it. Little appears to be known for certain about what actually happens (Haji-Hassan et al., 1989). There could be interference between the parallel and antiparallel components if one were to some extent twisted into the other. Altogether, it is possible that output intensity could be an oscillatory function of time, with characteristics varying with the initial polarisation. The detection of such a wave would depend more on peak than average intensity.

It is notable that one EPR experiment that does *not* support the quantum mechanical prediction uses calcite polarisers, not pile-of-plates. This is Holt and Pipkin's experiment, described on page 1909 of Selleri's book (Selleri, 1988). (See also Marshall and Santos' paper (Marshall and Santos, 1989), which gives alternative ideas, in terms of stochastic optics, to explain why differences of polariser type could affect the results.)

FURTHER REMARKS ON TIMING IN ASPECT'S EXPERIMENTS

Aspect approaches the problems associated with timing with great care, but entirely, as far as I can determine, on the assumption that the "photon" is a particle, of fixed energy. It is not therefore conceivable that it can have variable amplitude, so that timing variations can be anything other than random.

When he comes to discuss, therefore, the question of window definition, though he states that this should be chosen so that all coincidences are detected, and he recognises the importance of the imperfect timing resolutions, he is quite happy to accept a definition that will, he thinks, include 97% of the true coincidences. But there are two points I should like to raise here: firstly, it is very doubtful if he has sufficient detailed control over timing to ensure that the window is actually defined as he wishes; secondly, it is doubtful whether he would have noticed if his time spectra showed slightly greater spread for certain settings of Φ. This is because each run was generally about 100 sec long, which would, judging by figures in his thesis, produce spectra with too much scatter.

Aspect's estimate of 97% is likely, to my mind, to be an overestimate. Even if correct, if the whole of the missing 3% were due to systematic causes, this would be a serious factor as regards the violation of the Bell inequality. The observed S value of 0.126 could be attained if there were reinforcing biases of around 4% in each of the six terms.

As pointed out earlier, the logic of Aspect's coincidence circuitry is slightly different from that assumed in the current paper. Instead of opening a window with the A signal and closing it either with the B signal or after w seconds, where w is the window size, he has a more symmetrical system (see Fig. 6).

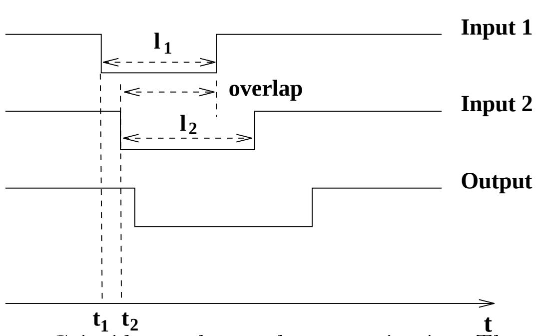

Fig. 6. Coincidences by overlap-type circuits. The circuit produces an output pulse if the overlap (here $t_1 + l_1 - t_2$) exceeds a minimal duration, u. (*copied from Aspect's thesis, page 235, fig. VI-18*)

Either A or B can open a window, and it stays open in fact for about 16 ns. This is reduced to effectively 10 ns by the requirement of about 6 ns overlap before a coincidence is recognised. This scheme is adjusted to give the effect of a single window, opened at time -3 ns and closed at time 17 ns relative to the arrival time of A, by applying a "delay" of *minus* 7 ns to the B path. So the parameters under the experimenter's control (though not always to the accuracy he might desire) are in fact *two* windows (chosen in actual experiments to be equal) and one delay.

If I have interpreted Aspect correctly (and my French may not be perfect), there may in fact be a large (2 ns) error in the delay (see Section VI-C-4, paragraph (a), where he states that the error in one method of estimating w is largely due to that in the delay). If this is the case, then perhaps we need look no further, as it would mean that the start of the window is not well defined. This could be critical: accuracy in estimating w cannot compensate for errors in the start.

INVESTIGATIONS INTO TIMING VARIATIONS

From a realist point of view, the need for further investigation is evident. Experiments could be conducted either using atomic cascades, as in the present instance, or using parametric down-converted "photon pairs". Much might be learned by conducting experiments similar to Aspect's but inserting attenuators in one path, or putting polarisers in series on one side and none on the other. Aspect's coincidence logic could conveniently be modified so as to look at coincidences that are out of order separately from those with B following A as expected. Time spectra need to be analysed carefully, with generous replication. Of course, many different parts of the apparatus are relevant, something of which the experimenters are well aware but which is not reflected in any quantum theory model that I have seen.

Computer simulations would be invaluable***. We may have enough information already to assess the likely magnitude of any timing effect. Is it likely on its own to be able to account for the observations, or do we need to invoke also some other source of enhancement (Pascazio, 1989; Marshall and Santos, 1989; Marshall and Santos, 1988)?

***A simulation in which a radio analogy of an EPR experiment is modelled has been produced by Barry Gilbert and Sue Sulcs (Gilbert and Sulcs, 1996). It may well owe its "success" in violating a Bell inequality to a subtle timing effect.

[Recent studies suggest that timing is much less important in Aspect's experiments than his "subtraction of accidental coincidences" (Thompson, 1997), but I feel the matter is of great interest none-the-less. It may be a factor in some experiments, in particular that of Freedman (Freedman, 1972; Freedman and Clauser, 1972), but more importantly any evidence of non-random timing variations would draw attention to the extremely unsatisfactory nature of the Quantum Theory model in this area.]

CONCLUSION

This paper has drawn attention to a possible source of bias that ought, even if it turns out to be very small, to have been investigated. Perhaps it is not reasonable to expect experimenters to go beyond theory in looking for systematic errors (which can, of course, be just as reproducible as desired effects), but are we not seeing here a symptom of a serious malaise? Can *any* experimenter be expected to make a completely rational assessment of his results when working with an illogical theory, in an aura of magic? Stuart Freedman, in his thesis in 1972 reporting an EPR experiment, has such faith in John Bell that he ends with the statement:

> "Since the results violate the inequalities, the discussion of systematic errors is greatly simplified. The inequalities are derived without reference to the specific experimental arrangement, so most systematic errors are irrelevant".

If Aspect's experiments were repeated now, in the light of current realist ideas, they would undoubtedly be taken to support Einstein and his colleagues, not Bohr, as a wealth of possible explanations have already been thought out and there may be yet more to be discovered. Both Einstein and John Bell will some day be able to sleep more peacefully in their graves, when it is finally accepted that no *valid* Bell inequality will ever be violated[†††].

Whether the observed effects are in fact due to timing variations or are mainly due to some direct form of enhancement is not critical to the wider debate over the conceptual basis of quantum mechanics: the really important factor, common to this paper and to other realist theories such as Stochastic Optics and Stochastic Electrodynamics (Pena and Cetto, 1996) is that the concept of corpuscular "photons" must be abandoned in order to obtain a rational explanation.

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[†††] John Bell did not expect EPR experiments to turn out the way they appeared to. He is quoted by Feyerabend (Merwe et al., 1992) as saying, with reference to the apparent success of quantum mechanics in EPR experiments, that: "So, for me, it is a pity that Einstein's idea does not work. The reasonable thing just does not work."

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