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"What Really Happens in Those Bell Correlation Experiments?"

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Abstract

Do any real experiments demonstrate "non-local" correlations? Is apparent "quantum entanglement" evidence of faster-than-light effects? As I shall attempt to show, there are other possible explanations for the observations, and these involve nothing more sensational than classical ideas about the nature of light, shared information from the source, and realistic hypotheses as to how the apparatus works. The experts themselves admit that there is so far no valid evidence that the kind of entanglement that seems to require non-local actions ever actually happens. Yes, "Bell tests" are violated, but they are not *valid* versions of Bell's test, so they do not rule out local realism.

I shall attempt to explain what the debate is all about, and how the real experiments can be modelled without any need to invoke quantum weirdness. The weakness of the quantum theory case is known to those in the field – one sees occasional references in the literature to "loopholes". These, as it turns out, are the key to the realist solutions. Faith in the universal success of quantum theory leads editors, referees and the experimenters to believe that they will eventually be closed, and that meantime science is best served by choosing the quantum theory interpretation of the results. But in my view they are perpetuating a myth, one that is undermining science by the introduction of magic.

1. Introduction

I am not alone in my discovery that the actual "Bell test" experiments, conducted from about 1969 to the present, do not support quantum theory nearly as convincingly as is commonly stated. I propose to try and explain a few of the ways of arriving at this conclusion. The waters are murky, though. We have to deal with several variants of the Bell test, various different kinds of experiment, different ways of treating the data, and various misconceptions held by prominent people. There is a vast quantity of confusing literature, mostly with little bearing on my topic: the actual experiments. I shall try to tell you what I think really happens, but this is not something to be understood in five minutes! I hope, nevertheless, that the later sections of the paper will convince even those who remain bemused that the experimental evidence should not be taken on trust.

In 1993, spurred on by a book review in New Scientist and with no formal training in physics, only in maths and statistics, I began to study Alain Aspect's experiments[1] on the EPR (Einstein-Podolsky-Rosen) correlations – experiments using Bell tests. As it turned out, my training in statistics was invaluable. I had experience in design and analysis, having been employed for several years as independent advisor to experimenters. Within a few weeks I had discovered for myself some of the suspect assumptions and consequent possible realist explanations, later finding out that almost all had been explored already by people such as Marshall, Santos and Selleri[2], Gilbert and Sulcs[3] and Wesley[4].

In the course of the next few years I had considerable correspondence with many people in the field. Though I failed to get my articles into the mainstream journals, I have not been entirely ignored. I have received encouragement to some extent from those actively involved as well as from other "dissidents" (some of this is reported in an article[5] in "Accountability in Research"). It emerged that the "loopholes" were well known to all concerned, including the editors and referees of Physical Review Letters and Physical Review A. They have faith, though, that the same results *would* be obtained if all were closed. I have equal faith that they are wrong, and that the results that would prove this have so far been ignored, the investigations that could have clinched the argument either never done or left unreported.

There are plans to conduct "loophole-free" experiments using atoms[6] instead of the "photons" that have mostly been used to date, but these are invariably very complicated and highly dependent on theory. We might learn more by re-examining the existing schemes, seeing if we cannot improve our whole experimental approach. The exercise might, incidentally, help us to understand the nature of light, rejecting (for low-energies at least) the concept of the "photon" and returning to a more classical pure wave model . Those who insist on the "photon" will find some of my ideas unacceptable. By thereby ruling out most realist explanations, however, they will have little choice but to fall back on the action-at-a-distance interpretation of the experiments – this despite the total absence of any proposal for its mechanism. This mechanism, by the way, has to act not only in the laboratory but along several kilometres of fibre-optic cable[7].

2. What are Bell tests all about?

For a full treatment of the subject, the reader is referred to books such as the recent one by Afriat and Selleri[8], which includes, incidentally, a few pages on my own work. The original EPR argument[9] was intended as a challenge to mathematics and logic of quantum mechanics (QM). It concerned the possibility that a pair of particles, A and B, that had once interacted might remain correlated in a manner that conflicted with Heisenberg's Uncertainty Principle. By measuring momentum on one and position on the other, you might deduce both to high accuracy, which, of course, Heisenberg's rule disallows. This may be of academic interest, but something much more important is also at stake. The authors were investigating the consequences of the QM assumption that the system could be described by just one "wave function", which was not "separable" (see below). The system evolved – and the particles encountered various pieces of experimental apparatus – as a single system, not able to be considered as two independent parts. The unfortunate consequence of this, the mathematics said, was that the *setting* of the apparatus measuring A could affect the result of the measurement on B. The effect would be instantaneous, "non-local".

["**Separable**", in the context of the actual experiments, means effectively the same as "**factorable**". It is the consequence of the "local realist" assumption that the detections of the two separated particles are independent events and any correlation between them is fully accounted for by "hidden variables", l, conveying shared information from the source. Once l is fixed you can obtain the probability of detecting both "particles" (in practice, both light signals) by multiplying the individual probabilities. In other words, the joint probability, $p_{AB}(l, a, b)$ "factors" into $p_A(l, a)\, p_B(l, b)$, where a and b represent the settings of the two detectors.

Thus the basic realist model says that the expected average probability of a joint detection can be expressed as:

$$P(a,b) = \int dl \;\; r(l)\;\; p_A(a,\, l)\;\; p_B(b,\, l) \qquad (1)$$

Integration is performed over the complete "hidden variable" space spanned by l, which may be of any number of dimensions. The weighting factor, ñ, represents the relative frequencies of the different "states", l, of the source. Note well that it is the *integrand* that factors, not the integral.

In practice, in the early EPR experiments at least, there is nothing mysterious about l. It is simply the polarisation direction, with a possible second component, the amplitude.]

At the time, there was heated debate, opposition to EPR's challenge being led by Niels Bohr[10]. The realist, assumption is the common sense one, that the system *is* separable. The version of QM that Bohr was supporting (and he was very influential!) said that it was not. But there is no obvious reason for this. Few have pretended to understand Bohr's reasoning. Why should you not be able to assume the standard result, that independent probabilities are multiplied in order to get joint ones?

As Furry pointed[11] out in a paper published in 1936, "the assumption that a system when free from mechanical interference necessarily has independently real properties is contradicted by QM". In (1) above, l represents these real properties, or those of them that are relevant and were determined at the source. QM is incompatible with their existence. The theory implies that, however far a pair are separated, they will influence each other, inducing correlations that cannot be accounted for in this manner[12].

This is what the EPR problem is all about. Opinions differ as to whether we are talking about real influences or only apparent ones, but the fact remains that most people in the field go along with the idea that the experiments back QM. QM's reputation for giving uniformly correct predictions has no exception.

[If you find the argument already confusing, you are in good company! A great number of people with whom I have discussed it find that they just cannot believe that the two statements, that “the particles are correlated” and “the detection events are independent, so that you can multiply probabilities once you’ve fixed c” can both be true. In the abstract, there seems to be a problem, especially if you are introduced to the subject in terms of “deterministic” models, and try and make *l* *completely* determine the outcome. This can be done but it makes life more difficult. The problem becomes considerably more manageable if *l* is defined so as to determine only the *probability*. Look at the structure of the integral in (1) or at the real experiments and this begins to make sense.]

The majority of investigations into the EPR challenge deviate considerably from the original idea. They do not mention position or momentum but, instead, pairs of more closely-related variables such as “spins” of atoms measured from two different angles. A great many have been thought-experiments only, evolving from a proposal of David Bohm’s[13] of around 1950. He suggested that something similar to the EPR hypothesis could be tested using two “Stern-Gerlach” magnet setups to measure the spins of two atoms that had once been part of the same molecule (see Fig. 1[14]).

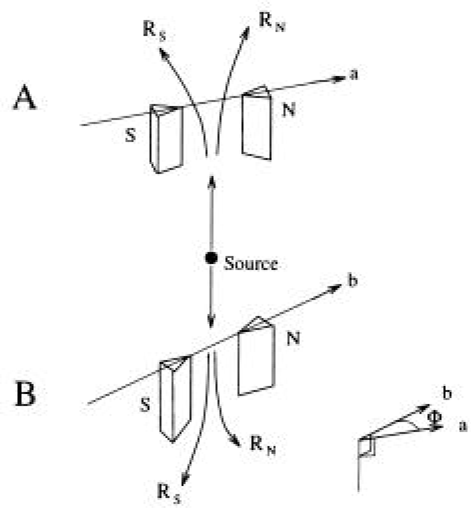

Fig. 1: Scheme for a Stern-Gerlach EPR experiment. N.B. This has never been done!

> Note that the diagram illustrates one of the important features of “quantum” measurements: we do not measure the direction of spin of each atom, only, indirectly, the component of it when the detector magnets are set at some chosen angle.

Now, at last, John Bell enters the story, and with him the possibility of real experimental tests. In the mid 1960’s he devised a statistical test, soon to acquire his name, for use in Bohm-type experiments. It would discriminate between separable (factorable) and non-separable models, and hence between quantum theory and local realism. A set of experiments was needed, measuring coincidences – simultaneous detections on A and B sides – for a few selected pairs of settings. If Bell’s test statistic is exceeded, this indicates that “nonlocality” is a fact of this real world.

[When you read that the actual experiments can only be explained if there are instantaneous influences between the two detectors, this is by no means a direct conclusion! All that is actually seen is that the test statistic seems regularly to come out larger than it should, if formulae of type (1) above hold. But it is only a test statistic! It depends on the data, which depend on the apparatus and on any transformations and adjustments that may have made. In reality, there are a great number of free parameters available, and hence a number of other possible reasons for that high "Bell test" statistic, appearing to support QM.]

To continue our story: Bell tests evolved towards the end of the 1960's into "CHSH" (Clauser, Horne, Shimony and Holt) tests[15], and, more recently, into a deceptively simple one: that of "visibility". (Reader beware: there is little agreement on nomenclature for the various tests!)

The modified tests were needed because nobody has been able to devise a satisfactory way of conducting Bohm's experiment using atoms, so they have performed instead what they believe to be equivalent ones using light. There is an important difference, though. Whereas with atoms it might reasonably be supposed that you detect every single one, with light it is well known that we do not have "perfect" detectors, able to register every "photon". (In a purely wave model of light, this is inevitable, and there is no prospect of ever achieving this kind of perfection.) The variants of Bell's test devised by Clauser *et al.* all required auxiliary assumptions. Neither the officially-recognised ones nor the many others implicit in the experimental schemes (many of which are hinted at in footnotes to Clauser and Horne's 1974 paper) have received the attention they deserve.

Some of the tests – the majority of those used in practice – are not valid unless one knows the number of photon pairs emitted. It is possible to make a strong assumption – that of "fair sampling" – that makes them usable, but this is not, unfortunately, as reasonable as it sounds. The kind of fair sampling that is involved is not something under the experimenters' control. It is associated with the inefficiency of those detectors, and with the "detection loophole", which has been known since 1970, if not before[16].

In this talk I shall concentrate on the two factors that are now widely accepted as potentially invalidating the tests: the inefficiency of the detectors and the subtraction of "accidentals". I shall not strain your patience by telling you about others, such as the failure of "rotational invariance" or possible problems with timing[17,18].

3. The Chaotic Ball Model

The principle behind the detection loophole is illustrated in a paper I published in 1996[19]. I shall not go into details here, but just present enough to give you the flavour of my argument.

It is based around the analogy of two assistants looking at a moving ball (Fig. 2), in which is embedded a magnet. "Detections" correspond to the cases in which the assistants can see one or other end of the magnet. They will not see quite the *whole* surface (Fig. 3)!

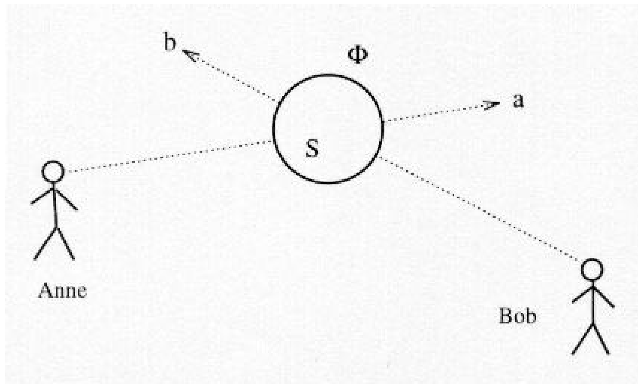

Fig. 2: Anne, Bob and the moving ball

S marks the South end of the magnet (**N** is out of sight). **a** and **b** are directions in which the assistants are viewing the ball; Φ the angle between them.

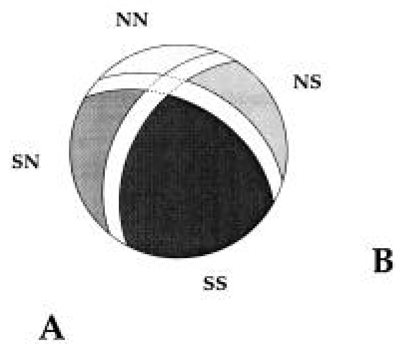

Fig. 3: The registered coincidences: Chaotic Ball with missing bands

The first letter of each pair denotes what Anne records, the second Bob, when the **S** is in the region indicated. There is no coincidence unless both assistants make a record, so some data is thrown away.

Now the "standard" form of Bell's test (the one of form $-2 \le S \le 2$) depends on the difference in frequency between "SS or NN" and "SN or NS" records. But how are we to estimate these frequencies? In this simple analogy it is clear that, in the limit, they will be proportional to the shaded areas, but what do we divide by to convert observed numbers to frequencies? If we knew (as indeed in the analogy we do!) the number of pairs we started with, clearly this is what we would divide by. It would be equivalent to the total surface area of the ball. But in practice this number is not known. Experimenters divide instead by the total number of observed coincidences, or in other words by something equivalent to the total shaded area. And they manage to infringe the "Bell test"!

The model makes it very clear what has happened. They are dividing by a quantity that is too small. In fact it is not hard to see that the statistic they are actually using has an upper limit of 4, not 2. Despite the fact that a slightly different geometry is needed to model optical experiments, this is equally true here, and when dealing with "detection efficiencies" known to be of the order of only 5% there is clear potential for trouble.

Ignoring the detection loophole, or making the fair sampling assumption, amounts to affirming the belief that if there are non-detections they are spread evenly over the surface of the ball. Although in optical work this is perhaps not quite as unreasonable as in my analogy, I can see no scientific justification for it. The loophole invalidates the test, yet it or its equivalent continue to be used, the results quoted as disproving local realism!

My chaotic ball is based on Stern-Gerlach-type experiments, which have never been done. It is possible to adapt it to cover the actual experiments using light, but perhaps easier to see the logical fault in the tests used for these if we start again.

4. An Actual Bell Test

Let us consider one of Alain Aspect's experiments – his first, reported in 1981 (see Fig. 4). This experiment differs from the setup discussed so far in that (a) it uses light, so that it is not so reasonable to assume a deterministic model, and (b) the analysers (polarisers, P_A and P_B of Fig. 4) each have only one output channel. With only the one channel, the standard Bell test is not applicable, but I shall leave discussion of the actual test used to my papers[20], discussing here only the "visibility" test, currently in vogue[21].

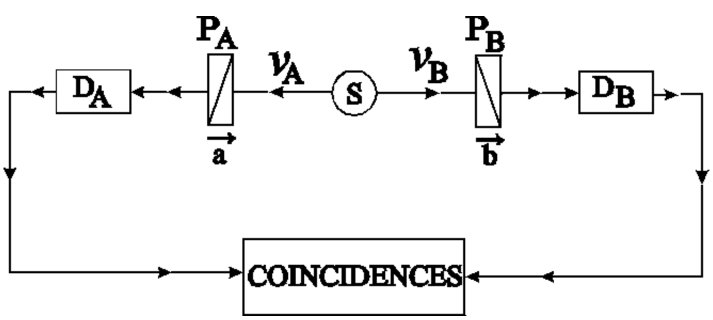

Fig. 4: Aspect's first EPR experiment, 1981.

Light from the source, S, is filtered so that wavelength n_A can proceed to the left, n_B to the right.

In this experiment, the hidden variable is "polarisation". Each pair of emitted light signals has a common polarisation direction, at angle l. When the signals pass through the polarisers, "Malus' Law" tells us, using classical theory, that the intensity is reduced by the factor $\cos^2(a-l)$ or $\cos^2(b-l)$ as appropriate. *If* (but see later!) our photodetectors (D_A, D_B) are obeying the laws of quantum mechanics, it follows that the probabilities of detection will follow these same laws.

If (and again, this is not to be taken for granted!) all l values are equally likely, we can immediately translate expression (1) into an actual prediction for the coincidence rate:

$$P(a,b) = 1/p \int_0^p dl \ \cos^2(a-l)\ \cos^2(b-l) \qquad (2)$$
$$= 1/8\,(1 + 2\cos^2(a-b))$$

Aspect himself used a "CHSH" Bell test on his data (see my paper, ref 20), but, for simplicity, I shall talk here about the modern test, which uses the visibility ((max – min)/(max + min)) of the coincidence curve[22]. This is a test that is almost equivalent to the standard Bell one, and it declares that visibilities in excess of 0.5 cannot be accounted for by realist models.

Consider, however, the properties of the model represented by expression (1). It translates into (2) *if* we assume detectors obeying quantum theory, but what if our detectors in the real situation do not? There is no logical reason for the *probabilities* to obey $\cos^2$ curves, and in fact there is good reason to suppose that they never obey them[23]. To have high visibility, the curve has to have a low minimum, and if we keep to $\cos^2$ laws, and assume uniform distribution of polarisation angles, then the minimum is in fact 1/8, which leads to the 0.5 figure in the test. If, however, we replace the law by one that has a broader trough than $\cos^2$, we can get a lower minimum and higher visibility! The test is not at all robust. In my view it is totally unreasonable to consider only realist models that involve perfect detectors and then, when visibilities are observed to be high, declare that you have ruled out *all* of them!

But, in point of fact, this 1981 experiment did not infringe either the Bell test actually used or the visibility test until after adjustment of the data.

5. Data Adjustments

All Bell tests, including the visibility test, are biased towards quantum theory if you adjust the data by subtracting an approximately equal quantity from every observed coincidence rate[24]. Such an adjustment has frequently been performed, in the guise of subtraction of the "accidental coincidence rate". This is the feature I have concentrated on in several of my papers, including those of ref. 20. Partly, this is because it is of historical importance, since it was done by Alain Aspect (in his PhD thesis[25] he goes to some lengths to try and justify it) but also because you do not need to know much about the experiments or the Bell tests to be able to see that there is room for doubt here.

The main data that I discuss in my papers is that from an unsorted table (VII-A-1) in Aspect's thesis. My summary is given in Table 1. A graphical representation (Fig. 5) reveals clearly that with no adjustment the data is closer to the realist prediction than to the quantum theory one!

Though my attempts at getting a paper published on this subject in PRL or PRA have failed, my campaign to get the practice stopped has had some success. I do not think you will find future experimenters who use Bell tests or equivalent will be subtracting accidentals without at least making it very clear that they have done so.

Angle between polarisers	0.0°	22.5°	45.0°	67.5°	90.0°	One polariser absent	Both absent
Raw coincidence rate	96.0	86.8	63.8	38.3	28.0	126.0	248.2
"Accidental" rate	22.6	22.8	23.0	22.5	23.3	45.5	90.0
Adjusted rate	73.4	64.0	39.8	15.8	4.8	80.5	158.2

Table 1: Raw and adjusted data for Aspect's 1981 experiment.

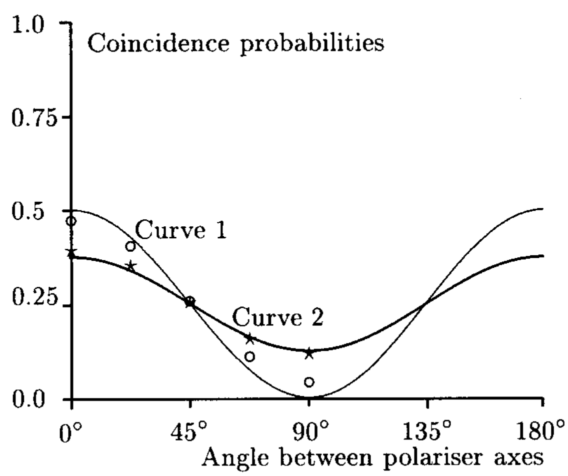

Fig. 3: Realist and QM predicted coincidence curves, and Aspect's 1981 data.

Curve 1 is the QM, curve 2 the realist prediction, both for the ideal case.
* indicates raw data, ° adjusted.

6. How to Prove Realism is Right!

There are tests that could be done and have not been done, or, at least, not been reported.

Aspect himself realised that if he had reduced his emission rate he ought to have been able to reduce his accidental rate to negligible values without reducing his "true coincidence rate" so much. With low emission rates he should have been able to show the same infringements of Bell inequalities without needing to do any subtraction. This should have been done.

Aspect found some "anomalous" results (a few are mentioned in my Chaotic Ball paper), and reported them in this PhD thesis but not elsewhere. I think they all deserve further investigation.

Re the detection loophole: the response characteristics of detectors should be investigated, and EPR experiments repeated using a range of emission intensities and detector settings so as to see how the curves and the test statistic vary. I would expect to find that there are cases in which the supposedly sinusoidal curves produced are not truly sinusoidal. (There are hints of this in some published graphs.)

There are many parameters that can be varied, and the law of "causal inference"[26] would, I believe, show that the quantum theory model followed the wrong logic, the realist model the right one.

7. So what has gone wrong?

The scientific community seems to have gone off on the wrong track. Why? Why have the experimental checks not worked?

I could (and may in the near future) write a whole book on the subject. I'll just suggest a few reasons here.

1. The human mind is not well-adapted to understanding probabilities and the kind of logic involved, especially in this context that is so divorced from the every-day world.
2. Most people accept all too readily that they have no hope of understanding either what the experiments are trying to do or the technical details of what they really do.
3. Hardly anyone realises that the different versions of Bell test involve different assumptions and are not equivalent.
4. "Realists" can see at a glance that the subject has no relevance to the actual world!
5. They therefore have not studied the experiments and hence have not realised that they should have been demanding to see more information – more raw data and more results for different parameter settings.
6. Important contributors to the field, notably John Bell and Alain Aspect have made mistakes, copied by others[27].
7. The literature is voluminous and confusing. Very little is relevant to the actual experiments.
8. Theorists realise that quantum theory itself is at risk. Regardless of all the "conceptual difficulties", it is too "successful" to abandon without good experimental evidence.
9. "Quantum computing" etc depends on quantum theory being right[28], and the computing industry is currently an important source of funds. (It has so far tolerated the fact that nothing spectacular has been achieved. Hopefully the research will produce useful results – advances in optical computers, for example – even if the original idea is totally misguided.)

8. Conclusion

I hope I have told you enough to see that the scientific community has two choices: dismiss the whole idea of (non-local) quantum entanglement as obvious nonsense or carry out the tests more carefully, reporting them in a way that allows for a fair comparison between the rival theories.

If your own theory suggests that instantaneous action at a distance really happens, so be it. The Bell test experiments, however, give no support for this idea. At best, they could give only indirect evidence, but my investigations indicate that they deserve to be forgotten. No valid test has ever been done.

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[1] Aspect, A *et al.*, Physical Review Letters 47, 460 (1981); 49, 91 (1982) and 49, 1804 (1982)

[2] Marshall, T W, E Santos and F Selleri: "Local Realism has not been Refuted by Atomic-Cascade Experiments", Physics Letters A98, 5-9 (1983)

[3] Gilbert, B and S Sulcs: "The measurement problem resolved and local realism preserved via a collapse-free photon detection model", Foundations of Physics 26, 1401 (1996)

[4] Wesley, J P: "Experimental Results of Aspect et al Confirm Classical Local Causality", Physics Essays 7, 240 (1994)

[5] Thompson, C H: "The Tangled Methods of Quantum Entanglement Experiments", Accountability in Research, vol. 6 (4), 311-332 (1999), http://www.aber.ac.uk/~cat/Tangled/tangled.html

[6] Fry, E S, Walther, T and Li, S, Proposal for a loophole-free test of the Bell inequalities, Physical Review A 52, 4381 (1995)

[7] Tittel, W *et al.*: "Violation of Bell inequalities by photons more than 10 km apart", Physical Review Letters 81, 3563 (1998) and http://arXiv.org/abs/quant-ph/9806043

[8] A. Afriat and F. Selleri, "The Einstein, Podolsky and Rosen Paradox" (Plenum Press, New York and London, 1999)

[9] Einstein, A, B Podolsky and N Rosen: "Can Quantum-Mechanical Description of Physical Reality be Considered Complete", Physical Review 47, 777-780 (1935)

[10] Bohr, Niels: Physical Review 48, 696 (1935)

[11] Furry, W H: "Note on the Quantum-Mechanical theory of measurement", Physical Review 49, 393-99 (1936)

[12] Perhaps to say that there is a claim that they influence each other is going a little beyond the facts. Strictly, what is claimed to have been demonstrated it that the *setting*, *a*, of one detector influences the probability of detection at the other.

[13] Bohm, D: "Quantum Mechanics", Prentice-Hall, 1951

[14] Spot the mistake in this diagram! A Stern-Gerlach setup in which the magnets were identical would not work: there has to be asymmetry otherwise when one is attracting so is the other, with equal strength, so the atom does not know which way to go. I inherited this mistake from Bohm. It does not affect the validity of my argument or his.

[15] Clauser, J F, M A Horne, A Shimony and R A Holt, Physical Review Letters 23, 880-884 (1969); Clauser, J F and M A Horne, Physical Review D, 10, 526-35 (1974)

[16] Pearle, P: "Hidden-Variable Example Based upon Data Rejection", Physical Review D 2, 1418-25 (1970)

[17] Thompson, C H, "Rotational invariance, phase relationships and the quantum entanglement illusion", http://xxx.lanl.gov/abs/quant-ph/9912082

[18] Thompson, C H, "Timing, "accidentals" and other artifacts in EPR experiment", http://xxx.lanl.gov/abs/quant-ph/9711044

[19] Thompson, C H: "The Chaotic Ball: An Intuitive Analogy for EPR Experiments", Foundations of Physics Letters 9, 357 (1996) and http://xxx.lanl.gov/abs/quant-ph/9611037

[20] Thompson, C H: "Subtraction of 'accidentals' and the validity of Bell tests", various versions submitted to PRL and PRA and rejected, 1998-9, http://xxx.lanl.gov/abs/quant-ph/9903066

[21] See Tittel, ref 7 above. He, incidentally, gives a figure of 0.707 instead of 0.5, for the test limit, but this is presumably because he looks at a different curve.

[22] A coincidence curve is a plot of coincidence rate of a selected type, usually "++", against the setting of one of the detectors, the other being held fixed. (It is, incidentally, assumed that the curve is independent of the value chosen for the fixed detector – a point that is not always carefully checked.) The question is, is its visibility of any significance?

[23] It is not easy to test what curve probabilities really obey, as all sources of light produce signals of varying strength. Experimenters in practice make adjustments until the system appears to obey Malus' Law, but they have no guarantee that they are not seeing the result of averaging over different shapes of curve, with the cancellation of any discrepancies from pure sinusoidal shape. There are circumstances in which correct behaviour of the average is not enough.

[24] Marshall and Santos recognised this problem back in 1985 and challenged Aspect, unsuccessfully. Paul Wesley discovered it independently and has published a paper on it (ref. 5 above). Aspect relied on many QM arguments to defend his case.

[25] Aspect, Alain: "Trois tests expérimentaux des inégalités de Bell par mesure de correlation de photons", PhD thesis No. 2764, Université de Paris-Sud, Centre D'Orsay, 1983

[26] Cartwright, N: "How the Laws of Physics Lie", Clarendon Press, 1983

[27] John Bell made a famous remark about not understanding how an experiment that worked so well with imperfect detectors could fail with perfect ones. Aspect convinced himself that the likely error from the detection loophole was negligible in his experiment, and that subtraction of accidentals was legitimate. I have reason to believe that his error estimate was erroneous, and his arguments re accidentals based on quantum theory assumptions that have not been proved.

[28] Zeilinger, A: "Fundamentals of quantum information", Physics World, March 1998, 33-40.