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The Tangled Methods of Quantum Entanglement Experiments

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Experiments in an area of science that is admitted to be incomprehensible and, until recently, has had no known applications, have been represented as supporting its predictions. This area is part of quantum mechanics dealing with 'entanglement'—the supposed link between particles that have once interacted, enabling them to influence each other instantaneously over indefinite distances. The new applications are in computing and encryption, but when experiments were first done none were envisaged. With the absence of any 'policing' by independent bodies, and the fact that the subject is difficult for referees and editors as well as for everyone else, experimental method appears to have deteriorated. The natural tendency to select data has gone unchecked, along with failure to explain assumptions or data adjustments. Data has effectively been suppressed, and social pressures appear to have dominated the scene, ever since the first experiments were found (after adjustment of the many free parameters) to be not only compatible with quantum mechanics predictions but to agree to great accuracy. This agreement is spurious, a result of the experimenters' decisions, yet faith in theory has left it almost unchallenged: the pursuit of the Nobel prize that many think will reward the disproof of quantum mechanics has not been considered in practice a good career move.

Keywords: Data adjustment; bias; peer review; quantum entanglement; Bell inequalities; photon

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INTRODUCTION

This paper concerns quantum theory, but you do not need to be familiar with the subject to appreciate the problems I discuss. These are matters more of our limitations as human beings—the conflict between our natural ways of doing things and the rigors of science, especially science that is beyond our everyday experience.

I became involved in the story of quantum entanglement in 1993, when I stumbled upon a statement in a book review that scientists had shown **'instantaneous action at a distance'**, which was, to my way of thinking, impossible. I simply could not imagine how the claim could be taken seriously. A *magician* might make such a claim, but not a *scientist*! How could twiddling a knob *here* instantaneously—not just fast but in *zero time*—produce an effect *over there*? There had to be something wrong with the experiment. There had to be some built-in bias or artifact that they had not understood.

Acceptance of the idea that this kind of mysterious quantum effect really happens has wide implications. If you add to it Einstein's ideas on relativity, with his doubts on the concept of absolute time, you open the door to the paranormal, time-travel, whatever you wish, for you can no longer distinguish the rational from the irrational. The universe might not be rational!

I had good reason for taking a straightforward view of things. I had reached the age of 50 living in a world that seemed to behave entirely rationally, and had had a limited amount of experience of working with scientists. Moreover, I had *not* invested years of my life in the study of quantum theory. If I had been told that all its predictions were correct, I would have been frankly skeptical. It was only a man-made model: we mere mortals just could not know enough to make a model that perfect!

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*indicates supplementary material available at http://www.aber.ac.uk/~cat

IS 'UNBIASED SCIENCE' POSSIBLE?

The scientists I had worked with were at an agricultural research station (East Malling, Kent, which dealt mainly with fruit crops), where I had been a statistician for about eight years. The system there was—or so it seemed from my vantage point—almost perfect. It did seem to produce valid science, but then, all the factors were in its favor, so why should it not? Our 'science', our common sense, and our practical know-how were never in conflict. Was it really 'unbiased'? Probably not, but it was perceived to be of practical use, so did it matter?

The part of the system of which I was most aware was the very rigid procedure whereby every single research project was supposed to have its own assigned statistician. The statisticians were as independent as they could be, all belonging to the Statistics Department. No project could be started without an experimental design, approved by the Head of Statistics! All results were supposed to be processed by the Statistics Department, who graciously marked the 'significance levels' that the experimenter had reached. This rigid protocol guaranteed that we had at least done our best to ensure that experiments led to valid advice to our 'members'—fruit growers, largely from the immediate vicinity. In addition to this procedure, there was a great deal of contact with growers, who had no inhibitions about telling us their problems, and there was in practice enough scope for established experimenters to just play with new ideas on a small scale, investigating anything that took their fancy.

Thus we were constantly aware that we were accountable to our local community. There was also a notable lack of theoretical models, other than the basic ones underlying the analysis of variance: empirical results were all that most experiments were seeking. All that was required was that the trials be conducted as fairly as possible, and reported in such a way as to enable the growers to draw useful conclusions. On the rare occasions when we did find ourselves constructing mathematical models, we would follow the obvious and necessary course, investigating their behavior over as wide a range of parameter values as possible.

At the time, I must admit that I could not quite understand the need for rigid experimental protocol. Why could the experimenters not be trusted to do their own statistics? It seems that the answer lies in human nature. East Malling, though funded mainly by the

Government, was answerable to its members, to whom the annual report was an important source of information. If we had shown benefits of x%, with standard error of y%, then the growers expected to be able to trust this in commercial decisions. Human nature, unmoderated either by commercial accountability and social responsibility or by a team of policing statisticians, is bound to be selective in publishing results (see for example Matthews, 1998). The subconscious cannot really avoid designing experiments in a manner that is biased towards pet theories. This tendency of ours, the finding of patterns given only the slightest hint, and the attachment to them, persuading others to join our school, has probably been of great value to us in the past. It has enabled progress in agriculture. Every now and again, someone would notice what they thought was an improvement, and tell their friends. However, the whole purpose of organized research is to speed the process up and make it more reliable: one well-designed experiment should be able to replace many individual judgments, but statistical analysis depends on us keeping to new rules. If we do not keep our natural ability to 'leap to conclusions' in check, we may invalidate the experiment. Hence the need for that police force.

In any event, this is the system I was presented with, and it preconditioned me to expect all branches of science to at least give the semblance of impartiality! Scientists were trained, I thought, to accept experimental evidence as it stood, without reading more into it than was there. Even the authoritarian system at East Malling could doubtless be bypassed with sufficient determination, but why should anyone want to? We were not expecting to be able to confirm mathematical theories, only find out what happened. My limited experience had shown scientists to be happy to discuss their experiments with anyone who came up with queries on them, especially if those queries were relevant or led to testable hypotheses. As to publications, we naturally expected all valid results to be accepted, and if any result had later been found to be in error a published explanation would be a matter of course.

THE ENTANGLEMENT PROBLEM

Now quantum theory is rather different from agriculture! It is not an area where we have as yet had the opportunity to develop 'common sense' and indeed, its proponents will tell you unashamedly that nobody understands it. One might hope to be able to investigate its claims experimentally and thus come nearer to understanding, only something seems to happen to prevent this. Experiments seem to tell us less than the mathematics! What has gone wrong?

Quantum theory was invented around the beginning of this century when a few experimental facts emerged that seemed to demand it. Light seemed sometimes to behave not as waves but as if it were composed of particles—'photons'—which could not be divided. At first, this was just a different way of describing things, but then, in the mid 1920s, it was incorporated into a new mathematical theory: quantum mechanics (QM).

And QM turned out to have some very odd properties. First and foremost to my mind (and indeed, the subject of much dispute at the time) would have been that it made the understanding of some of the basic behavior of light—interference effects—very difficult, where before it had been no problem. But, once they had decided that this was a necessary sacrifice, the problem that the scientists focused on was 'entanglement'. The theory said that two particles that had once interacted became somehow bound together so that what you did to one of them could instantaneously affect the other. In the everyday world, this is impossible, and in most areas of science it is taken for granted that this kind of 'magic' cannot happen, that the aim of science is to find rational explanations for things in terms of cause and effect. Real causes cannot leap across space instantaneously. (QM not only disregarded 'causality' but also invented its own rules of probability, thus starting an era in which it was impossible for statisticians to work comfortably with fundamental physicists.)

It did not look feasible to test this entanglement property, as the quantities concerned were too small and too easily destroyed, but Einstein, together with colleagues Podolsky and Rosen, led a rebellion, publishing their famous 'EPR'—Einstein, Podolsky, and Rosen—paper (Einstein *et al.*, 1935). No theory that allowed 'spooky action at a distance' (Born, 1971: 158) could be accepted as truly fundamental.

Niels Bohr argued that perhaps the quantum world obeyed different logic, and such was his prestige that this has become the official view. The fact that he organized congresses in Copenhagen that nobody would have liked to have missed may also have played a part, not to mention his reputation as a formidable adversary (Heisenberg, 1971: 73)!

This situation continued for a few decades, and QM consolidated its position, being taught to the next generation despite its 'conceptual difficulties'. Eventually a method of testing entanglement experimentally was found. John Bell, in the mid 1960s, discovered what has become known as the **'Bell inequality'**. This was intended to distinguish between QM and 'local realist' alternatives. It was based on the fact that QM predicted correlations in 'coincidence rates' (see Supplements A and B) that cause a certain test statistic to be greater than would otherwise have been possible. In certain simple situations, Bell's original rather complicated test is equivalent to the observation that QM predicts a sine curve that has a minimum of zero and **'visibility'** ((max−min)/(max+min)) of 1.0, whilst the local realist alternative predicts one with minimum definitely greater than zero and visibility 0.5. Observe a visibility of over 0.71 and you have evidence of something unexplainable under ordinary logic (but see later!). The literature does not make it easy for the general reader to see that the issue is really this simple.

For a little more detail on all these matters, the reader is referred to the various supplements to this article. Suffice it to say that from the late 1960s onwards, there have been various attempts at doing **'EPR experiments'** (see Figure 1), testing for infringement of Bell inequalities. The vast majority of these have seemed to back QM, but every single one has needed many assumptions (see Appendix) in order to be interpreted at all, and closer inspection shows that these assumptions are unlikely to be true unless QM is true! It is possible that the whole argument is circular, as 'realists' have been trying to point out for many years now (Marshall *et al.*, 1983).

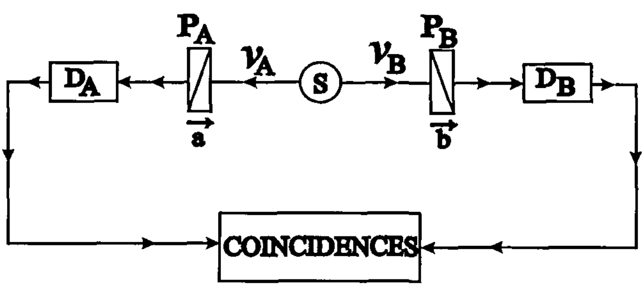

Figure 1 A typical (single-channel) EPR experiment. S is a source producing pairs of 'photons', of frequency ν_A and ν_B. These pass through polarizers P_A and P_B and are either detected or not by detectors D_A and D_B. If both A and B are detected within a small time-interval, we score a 'coincidence'.

Not only do the reported EPR experiments rely on many assumptions, but there is absolutely no attempt to help the reader understand their implications. Some of the assumptions allow them to use tests that are, in my view, completely invalid. The best known is concerned with the so-called '**detection loophole**' (see Appendix), and this is generally mentioned, though in rather a casual manner. But there is another, often of great numerical importance, that is made regularly and yet never mentioned at all. It is that of the independence of the emission events, and it is used to justify a data adjustment—the '**subtraction of accidentals**' (see Supplement B)—that, in almost all cases, forces their test statistic up and over its limit. The subtraction changes results that can be explained realistically into ones that require quantum magic. There is no mention in published papers of the assumptions behind the adjustment, and insufficient information given for the reader to work out what the unadjusted data was.

And how big is this adjustment? Well, I have searched through Alain Aspect's thesis (Aspect, 1983), found some data (not a lot) and summarized it:

TABLE I Raw and Adjusted Coincidence Rates

Angle between polarizers	0.0°	22.5°	45.0°	67.5°	90.0°	One polarizer absent	Both absent
Raw coincidence rate	96	87	63	38	28	126	248
'Accidental' rate	23	23	23	23	23	46	90
Adjusted rate	73	64	40	16	5	81	158

One can judge the significance of the adjustment just by looking at the first five columns. They show that the raw coincidence rate decreases as angle increases, and this follows a sinusoidal curve as expected. It does not, however, decrease to zero, as QM predicts. Its visibility is 0.55, not significantly above the expected realist value. Subtract the 'accidentals', however, and you get 0.87, a very considerable change!

It is my belief that publication of the above would have meant the rejection of QM 20 years ago. A very few others (notably Marshall, Santos and Selleri, 1983) have realized that the subtraction is unjustified, but it has fallen to me, a complete outsider, to unearth the full extent of the bias (Thompson, 1997, 1998a, b).

MY OWN EXPERIENCE

In 1993, as I said, I stumbled into the EPR story. It was a different world, as indeed the participants realize! They have recently published articles on entanglement with titles such as 'Quantum Theory still crazy after all these years' (Greenberger and Zeilinger, 1995).

I have been shocked by the whole approach, both the design of experiments and the way in which they are reported. Is this really a reasonable way of testing QM as a model, and are they reporting results in such as way as to leave the reader with a true picture? The strategy employed appears to be to use whatever methods necessary to coax the apparatus into producing the high correlations in the 'coincidence counts' that will violate Bell's inequality. (Admittedly, this is a considerable technological achievement.) They do not probe too deeply for 'realist' explanations, as they believe that infringement of Bell's inequality shows that none are possible. And they give the reader only the most gentle of reminders that maybe there are doubts, that the interpretation presented depends on assumptions that depend on the validity of QM.

Faith in theory has been supplemented by faith in authority. For example, Clauser and Shimony, in an otherwise excellent survey (Clauser and Shimony, 1978), mention the so-called 'detection loophole' (see Appendix and published papers such as my own 'Chaotic Ball' paper (Thompson, 1996)) but dismiss it as most unlikely to apply. They express the opinion that 'Virtually any conceivable error will wash out a strong correlation so as to produce results in accordance with Bell's theorem, rather than speciously strengthen a weak correlation', and their word has been accepted. Within weeks of reading Aspect's papers, I knew this statement was misleading, something that was true only if QM was true.

After a few months in the library checking my facts (and finding that I was not alone) I started to circulate a paper showing primarily that the detection loophole can never be ignored in two-channel experiments, as you cannot prove it is not there and, if present, it is almost certain to bias the results in favor of QM. I realized that it would take time to publish, so I wrote directly to some of the experimenters, trying to warn them that they had been misleading themselves: their experiments could not legitimately be reported as 'excluding the possibility of a local causal explanation', for I could produce one. I explained the matter as simply as possible, for it seemed evident that there had been a breakdown of communications. The 'detection loophole' was 'well known', yet it seemed to me that it was not *understood*, or it would not be ignored so freely. (The material of this early paper is now covered in my 'Chaotic Ball' paper and various others.)

Alain Aspect (whose EPR experiments have won him world acclaim) never replied, either to this first or to subsequent letters. Others listened, sometimes even praised my work. Alan Duncan, from Stirling, said he was sorry but his department had been closed down. When I wrote a second time he said (Duncan, 1994): 'I was pleased to see you still have the 'bit between your teeth' but I think you will have great difficulty in convincing people of the validity of your ideas in this area.'

John Rarity said (Rarity, 1994): '[it] is similar ... to the work of Santos ... I prefer your analysis because it is not cloaked in mathematical terms ...', but he also said: 'If you want anyone in mainstream physics to read beyond [the introduction] you cannot make such strong statements that [QM] is wrong.' He and his associates all continued to publish with no change, reporting results as if the possibility of QM being wrong was almost out of the question.

Looking back now at Rarity's first message, I can see that he raised all the arguments that are still confronting my ideas: 'Why are the assumptions you use ... any better than those of [QM]?'. Whether I agreed with his assumptions or not was just my own opinion. He argued that he was justified in presenting results as 'evidence in support of' QM, as this was not the same as claiming to have proved it incontrovertibly.

The 'realists' I contacted were on the whole enthusiastic about my ideas. Franco Selleri responded to my first paper with '[It] is incredibly good: who are you?' (Selleri, 1995a). But perhaps from the practical point of view my early ideas were *not* in fact very important—their expected numerical consequences were small.

More recently, however, as mentioned above, I have [re-]discovered another 'loophole', a small matter of a data adjustment, that has *large* numerical consequences in many experiments. In many EPR experiments the data is adjusted by 'subtracting accidentals' before the Bell test is calculated. But are there really any accidentals to be subtracted?

I have made sure that people are aware of the problem through emails and the Los Alamos Quantum Physics archive. In October 1997 I sent a message to Rarity asking very specific questions regarding his 1994 paper with Tapster. The most important question was what was the accidental rate. (I had on a previous occasion asked another question that might be vital for understanding the actual physics of parametric down-conversion: 'Had he ever done the experiment he intended that altered both settings at once?' I had no replies. If he had not altered both settings—and I suspect that nobody else these days has either—then it is quite possible there is an area of theory here that has never been tested. But that is another story.)

Partly as a result of circulating early editions of this paper, I am now corresponding again with Rarity and am hopeful that the discussion will be fruitful.

I have now met a few of the people concerned at conferences. They have shown very little interest in the experimental details. They prefer talking about the applications of entanglement in quantum computing, or devising yet more devious ways of persuading Nature to demonstrate quantum magic. It seems to me that the idea of gently posing questions so as to find out how Nature really works has been lost. Niels Bohr had declared that we could not hope to find this out, that how it 'really works' was one of the meaningless questions that we should not ask. But why did they accept this so meekly? In these optical experiments, at least, the actual mechanism did not look too difficult to me—it was something I could hope to simulate, though I would have been hard put to it to come up with a neat formula.

To give concrete examples: in July 1997 news of the experiment in Geneva came out in the popular press (even before publication in the scientific journals!). This was supposed to show instantaneous influences acting over distances of about 10 km. When details were available in the Los Alamos archive (Tittel *et al.*, 1997), I contacted Nicolas Gisin and Wolfgang Tittel, two of the workers concerned. They said that yes, they were interested in realist explanations. When I met Gisin at a conference in Hull later that year, though, he did not appear to have understood the importance of my objections to the paper, which included the apparently casual subtraction of accidentals (see Supplement B). He did say that he would investigate some of my points, and I am now hopeful that some progress will be made: he confirmed in March, 1998, that they are 'working on further experiments' (Gisin, 1998). Hopefully these include various critical tests, such as varying settings on both sides at once and (as promised in 1997) investigations at lower emission rates.

Theory says that if you reduce the emission rate you should be able to reduce estimated 'accidentals' faster than you reduce the counts you are interested in. There ought, therefore, to be no practical reason why the experiments should not be repeated at lower rates, so that accidentals are negligible, as they have been in at least one experiment. This is the obvious step that should be taken. The findings of a reduced-rate experiment would be of great interest, whether or not they conform to QM expectations.

At this same Hull conference I met (not for the first time) Lucien Hardy. He presented results from his 'Ladder' experiment (see Boschi *et al.*, 1997). The matter of the subtraction of accidentals was discussed (Hardy, 1997), and Hardy agreed that it was not justifiable and he had not done it. He had, however, relied on being able to ignore the 'detection loophole'. This I simply cannot understand! He is one of the first people to whom I showed my Chaotic Ball model, in early 1995 when the late Euan Squires invited me to Durham for the purpose. Hardy knew that this was a real weakness, but he did not seem to think that it mattered! He said he thought I would be able to see a realist explanation, as indeed I can. There is a glaring assumption that all 'photons' go to the + or − channels, and none disappear, at least not until the detection stage, when a fixed proportion do. How, in the circumstances, can his statement in the abstract—'The experimental results violate locality (modulo, the efficiency loophole)'—be classed as 'science'?

One of my concerns has been the difficulty of extracting supplementary information from individual experimenters. Perhaps their reluctance to release information can be explained by the fact that I am a private individual, with no PhD, but I have reason to think not. Professors Franco Selleri (1995b) and Emilio Santos (1995) have had no more success. With experimenters such as Alain Aspect, one can understand that he would not have time to sift through all the mad-cap papers that must come his way. I am confident that my ideas are not in this category, having presented them at several conferences now and argued my case in the sci.physics newsgroup on the Internet. In other cases, such as that of Alan Duncan, the problem has been that the department has been closed down and the data effectively lost.

The major concern, though, is the evidence I have found of apparent lack of interest in how Nature really works, and this, I realize, is built into the very philosophical basis of quantum theory. Niels Bohr decreed that certain questions should not be asked.

Others, such as Werner Heisenberg (1971), said: '... the taboo need not really upset us. There will always be young people enough to think about the wider context, if only because they want to be absolutely honest in all things.' He added, incidentally: 'And that being the case, their number is unimportant.' I fear that he was wrong: the number of people who challenge taboos *is* important, as the lone rebel will be crushed by the establishment.

PUBLICATION PROBLEMS

In addition to problems with individuals, I have had first hand experience now with the editor and referees of *Physical Review Letters* (PRL). *Foundations of Physics Letters,* whose editor, Alwyn van der Merwe, is a friend of Selleri's and a supporter of realism, was happy to publish my Chaotic Ball paper, but it does not seem to have been seen by many. I have yet to see it cited, and the journal is not viewed by magazines such as *New Scientist* to be of sufficient repute for them to take into consideration. I felt that PRL was the correct place for my next ideas, as the majority of the papers I am concerned with are published there and because, if the referees never see my work, they are going to continue for ever, it seems, to publish with scarcely a question every paper submitted that 'confirms' quantum entanglement.

Anyway, I submitted my 'Timing and Other Artifacts' paper (the title did not at that stage include 'accidentals') in April, 1997, and its progress has not been straightforward. At first they sent it in error to the Divisional Area Editor (incidentally exceedingly well qualified, having been involved with Aspect's experiment personally), because they thought it exceeded the required four pages. He read it, and proclaimed that 'scientifically, it looks basically sound'. Regarding my coverage of the detection loophole, he said (PRL, 1997a): 'The specialists in the field acknowledge that this loophole exists, so that there is no special need to write a letter on the subject (although the fact will probably continue to be rediscovered again and again by clever newcomers).'

Of course, the main purpose of my 'letter' was other matters—timing problems and 'accidentals'. He went on to condemn my ideas on the latter as *ad hoc*. In his opinion 'Generally speaking inventing *ad hoc* models is not, it seems to me, how physics makes real progress.' It did not occur to him that perhaps it was the QM model that was *ad hoc*! He concluded, after making a few reasonable points about my style, that 'this text has too little chance to be eventually accepted and my advice to the author would not be to submit a new version.'

I objected, on the grounds that my work had not even been seen by a single referee, saying with regard to my pet subject:

> The proper scientific procedure would, in the circumstances, have been to publish both raw and adjusted data. This was not done. In the interests of science, I ask that this data should be published, together with sufficient supporting facts and ideas to show why there is reason to question the adjustment.

I requested them to send my explanatory letter with the paper, but they chose not to. As expected, the one referee who commented rejected it. He did not appear to have realized that it did more than re-open the detection loophole debate. He said, among other things (PRL, 1997b):

> I think that there is nothing to add to this question, until we have an experiment with detectors efficient enough to close the loophole. To my knowledge, the present state of the art allows us to hope that such an experiment is feasible, and I know of two such experiments in progress. So we will have an experimental answer for people who do not find the fair sampling hypothesis reasonable. That is their right...

(This, I have reason to think, is wishful thinking. As I try to explain in Supplement B, there is no future in experiments using 'photons', as high 'detection efficiency' is not possible without unacceptable side-effects. I see little prospect either for other kinds of 'particles'. For example, a recent experiment (Hagley *et al.*, 1997) claims to have produced entangled pairs of atoms, but these produce a 'modulation depth' (visibility) of only 25%, not the 100% predicted. Further, the paper shows little sign that the authors understand the assumptions behind Bell tests: unless these are met, even 100% would not necessarily 'violate locality'.)

The referee continued:

> ...but I would like to quote here John Bell (who was *a priori* an advocate of hidden variables), about this very question: 'It is difficult for me to believe that quantum mechanics, working very well for currently practical setups, will nevertheless fail badly with improvements in counter efficiency...' (J S Bell, *Speakable and Unspeakable in Quantum Mechanics*, page 109, Cambridge University Press).

So I do not think that it is worth to reanalyze 15 year old experiments, confirmed by many more recent ones. The only important next step is the new generation of loophole free experiments, and/or new type of experiments with different schemes.

(It seems that John Bell made the same mistake as Clauser and Shimony, in thinking that because various imperfections made QM less likely to violate his inequality, they must also make the real coincidence rates less likely to violate the modified inequality used in practice!)

I waited till after the Hull conference—my meeting with Gisin—before submitting a revised version, including a reference to the Geneva experiment as well as my original ones, which were mainly from Aspect's work. (I had hoped at one point that the resubmission could be a joint effort with the Geneva team!)

They have, as expected, treated my resubmission as an appeal. I have now heard (June 2, 1998) that the appeal has failed. This had been preceded by a ridiculous farce in which the paper was sent to another referee, who totally misconstrued it. It seems that I had no right of reply. The editor decided (PRL, 1998), on the basis of the new review and on his 'own understanding', that the manuscript was 'not appropriate for PRL'. He added that: 'It is not a new idea and is in contradiction with experiment at least as far as the conclusion about hidden variables is concerned.' I should dearly love to know what this is supposed to mean! What it amounts to is that no new idea is ever going to be accepted, because all referees and editors will react against them and the author never even gets a chance to try to persuade them to change their minds. Be that as it may, the editor-in-chief confirmed the rejection, on the basis that he considered that the correct procedures had been followed. My final letter (email) had not been passed to him in time, and would probably have made no difference.

PRL has been rejecting papers like mine for a long time. This is surely, as Bryan Wallace has explained so clearly (Wallace, 1993), not in keeping with the official policy of the American Physical Society of 'unfettered communication at the Society's meetings or in its sponsored journals of all scientific ideas and knowledge that has not been classified.' Emilio Santos, who co-authored in 1983 the important paper with Marshall and Santos that *did* manage to gain acceptance in Physics Letters A, has been trying ever since to get one into PRL—preferably an uncensored one, giving *all* the facts. Indeed, in 1985 he submitted one specifically on the subtraction of accidentals. This was later published in some conference proceedings, where it quietly gathers dust. As he told me two years ago, he has grown tired, wasted too much time in this battle. He encourages me, but for himself has turned mainly to other more constructive things. I have in front of me a copy of the rejection notice for one of his papers (Santos, 1997). It could have been mine! The very same Divisional Area Editor refers to the same belief of John Bell: 'He could not believe that the laws of physics should change so drastically depending on the efficiency of photodetectors.'

Twice in the notice he gives his own opinion, that it is a 'matter of personal taste' if you choose to query assumptions and hence 'escape' from the consequences of the observed inequality violations! Though, as he says 'colleagues point out that, rigorously speaking, a violation has not been proved', at the end of the day he does not think the material sufficiently original and also decides that it is 'more appropriate for private correspondence than publication'. Why?

A referee rejecting one of Werner Hofer's papers said (Hofer, 1997): 'There is now very strong experimental evidence, based on Bell's inequalities, that [a local realist theory] cannot be correct. It is true [that there are] small loopholes ...'

Another realist, Al Kracklauer, said his papers had been rejected 'for reasons and with arguments that are a disgrace to the profession' (Kracklauer, 1998). They appear to be rejecting papers that endanger the accepted dogma even when, as in my case, they are viewed as *scientifically sound.* There is *no* discussion of such papers with referees, who thus remain ignorant of the strength of the opposition—or the importance of those 'small loopholes'.

This system has grave consequences for science. New theories are not being considered, the experiments that would force re-assessment of the old ones not being done. For some of my own ideas on suppressed ideas, see Supplement C.

WHAT IMPROVEMENT COULD BE MADE?

As a statistician, I was taught that you should always present sufficient information so that readers could check the significance of the results for themselves. This means that all assumptions should be clearly stated, together with discussion of their importance and what attempts have been made to check their validity. In order to assess the EPR experiments, one also really needs to know what happened when parameters were set differently.

Now, in a subject such as particle physics, I fully understand that this is not feasible in a published paper, as there is simply too much data, but in these 'entanglement' experiments there is relatively little. At the very least, supplementary information should be available on request, but some should be in the original report. It is clearly bad practice to publish adjusted data without making clear both the assumptions behind the adjustment and its size. In the case of a major adjustment, it would not go amiss to publish the effect on the final test statistic!

If the referees of the early reports had insisted on minimum standards, this would have helped, though in point of fact the faults in those days were obscure. The moral really is that there should *never* be any relaxation of standards. The doubts are known. They should be re-iterated in every report until they are resolved.

It seems reasonable to suppose that the reason for this poor standard is two-fold. There is the incomprehensibility of the subject, together with the lack of accountability to the world at large. There is beginning to be a degree of accountability now, in that computing firms and governments have become interested in possible applications, but in the early days there was no purpose other than academic interest. Hopefully this new accountability will help, but given the aura of mysticism surrounding the subject it may be hard to break the spell!

Thus there are improvements that could be made—improvements of access to the experimental facts, an obligation, perhaps, on the experimenter to answer legitimate queries—but incomprehensibility seems to be an insurmountable barrier to the conduct of science. What does not seem to have been realized is that it affects not only the accessibility of the subject to the general reader but also the referees and editors, who have no choice but to appeal to authority! One can, I believe, query the usefulness of the whole enterprise.

APPENDIX: ASSUMPTIONS IN REAL EXPERIMENTS

Real experiments do not all involve all the assumptions at once, but each involves more than one. Some are untestable. All deserve thought.

1. Fair sampling:
The pairs that are recognized and analyzed are a fair sample of the total emitted.

2. Detection (efficiency) loophole not operating:
The sum of the probabilities of + and − results is constant, i.e. does not vary with the hidden variable.

3. Malus' Law holds exactly for detected counts (or, if not exactly, then the difference is only a matter of a couple of constants: there is always a sinusoidal curve):
The probability of detection for a signal that had polarization at angle x to the axis of a polarizer is proportional of the $\cos^2 x$. (This is a QT assumption related to an empirically verified result involving *electromagnetic intensities* of whole beams. Single photon detectors are designed so that the *frequency of detection* reproduces it as closely as possible.)

4. Detectors can be fully characterized by their quantum efficiencies:
The probability of detection of every photon is the same, equal to the quantum efficiency of the detector. (This ignores facts such as detector dark rates, and makes no sense unless light really does come in photons.)

5. No enhancement:
The presence of a polarizer cannot ever increase the chance of detection.

6. No synchronization problems:
A and *B* photons take the same time from emission to detection apart from purely random variations. (A systematic trend such as weak signals tending to be detected late would spoil the logic).

7. Emissions are stochastically independent:
For atomic cascades, each atom acts independently. For parametric down-conversion, each pump photon acts independently. Unless this is true, the subtraction of accidentals (as estimated by using a delay on one channel) is not legitimate.

8. Rotational invariance:
Each possible hidden variable value occurs equally frequently. For cascades, this means there is no preferred polarization direction; for parametric down-conversion it means (in most experiments) that the spread of frequencies is relatively broad so that all phase differences are equally frequent.

9. Symmetry:
Symmetry failures, whether of A versus B sides or + versus − channels, can be satisfactorily corrected for by, e.g., adjustment of detector thresholds. (In practice, asymmetries can invalidate Bell tests by opening the 'detection loophole' wider, as detector responses may not be linear.)

SUPPLEMENT A: EINSTEIN AND QUANTUM WEIRDNESS

As Einstein once said, quantum theory did not deserve its success as it was invented with the left hand not knowing what the right hand was doing (Born, 1971: 10). This was in 1919, before it had been formalized, so that it was still quite flexible, intended simply as a modification to cover newly-discovered facts of classical theory, which had been made into almost a theory of everything then known (Lorentz, 1916). Among the 'facts' that it was thought could not be incorporated in classical theory was the corpuscular nature of light, an idea for which Einstein himself had been responsible. He had invented 'light quanta', now known as 'photons', which were supposed in the photoelectric effect to donate their entire energy to individual electrons. All photons corresponding to a given frequency had, in quantum theory, exactly the same energy.

Now, as the physicists of the time realized, this was a very artificial idea, making various phenomena that had previously been well understood (interference effects in particular) impossible without great mental contortions. When the theory became a matter of formal mathematics, in the mid 1920s, its intrinsic problems led to some strange predictions. Among these was 'quantum entanglement'. Two elementary particles that had once interacted remained linked in a mysterious way, so that a change of decision by the observer as to what to measure on one of them could instantaneously affect the outcome of a measurement on the other. Einstein referred to this as 'spooky action at a distance', and his objections to it were so strong that it is more than likely that, had he realized that the choice lay between that and the photon idea, he would have discarded the latter. He did realize that quantum theory, despite its steadily growing reputation as a predictive model, never wrong, had become a monster, no longer under anyone's control. He does not seem to have realized fully his own part in its creation.

We are told that quantum theory has never been wrong, but what is the truth? The experiments that I have been studying for the past few years grew from an idea of Einstein's, published in 1935 in the now-famous 'EPR' paper (Einstein *et al.*, 1935). The idea was developed into an apparently feasible critical test of quantum theory versus 'local realism' by John Bell (Bell, 1964), and then further modified by other workers into tests that really *were* practical, allowing for the fact that real instruments did not detect by any means every 'particle'. The modified 'Bell's inequalities' all rely, though, on supplementary assumptions (see Appendix), and these are the only inequalities that have ever been investigated. Their violation means that either local realism is wrong (Nature can work by magic; some facts will never have scientific explanations) or one or more of the assumptions is wrong.

The first attempts at these experiments were in the late 1960s. The 'particles' used were 'photons', these being the only kind that could be persuaded to exist in the required 'correlated pairs'. Bell's test was based on the fact that quantum theory predicted a correlation that was stronger than any that could arise if the ordinary laws of cause, effect, statistics, locality, etc. applied. The correlation showed up if you counted 'coincidences' (numbers of simultaneous detections of the two 'photons') and plotted them against the angle between the settings of polarizers set in their paths (see Figure 1, main paper). Quantum theory predicted a high visibility ((max−min)/(max+min)), up to the maximum possible value of 1 if conditions were perfect. The only well-known local realist model predicted, under similar conditions, a value of only 0.5. Slight deviations from perfection could reduce the quantum theory value. The fact that they could at the same time increase the local realist one was unwittingly 'assumed away'.

All experiments to date have clearly shown the expected general trend of variation of coincidence rate with angle, and the curve has seemed to have high visibility. The one or two experiments that produced results within the range of the basic local model could easily be explained by high experimental error. Moreover, it proved difficult to get these inconclusive results published, as Holt and Pipkin discovered (Holt and Pipkin, 1974). Their work is quite well known through a preprint, but never appeared in a journal.

So we had a situation in which some experiments seemed to produce results that supported quantum theory, and other experiments that did not could be explained away quite easily. Nobody seems to have realized that this was getting nowhere: whatever the outcome, local realism could not win! What was needed was a long hard look at what actually happened—those messy experimental details—and the follow-up of all realist suggestions (Marshall, Santos and Selleri, 1983). What happened instead was that each experiment in turn was recognized as to some degree flawed, but the flaws were given exceedingly little publicity. The theorists devised yet more elaborate experiments, but always opening up new 'loopholes' faster than they closed old ones. This has not of course been done intentionally, but more as a result of not understanding the realist alternatives.

But the consequence has really been quite amazing, as somehow quantum theory's repeated 'successes' has led to the belief that it is more than just a mathematical model: it really does represent the quantum world. What these experiments have been taken as showing is that quantum weirdness extends to the scale of macroscopic laboratory experiments. This is Nature actually demonstrating non-locality—infinitely more serious than, say, Newton inventing a non-local theory of gravity. Newton knew that his theory was a temporary expedient, to be replaced some day by one in which the nature of the force was better understood, and this force had no reason to act instantaneously at a distance. Yet here are the fundamental physicists accepting an effect that can only be described as magic, and the theory offers no attempt at physical explanation—it simply follows from the algebra.

The story of the 'EPR experiments', now succeeded by other demonstrations of quantum entanglement ('teleportation', etc., as described, for example, in a recent supplement on the subject in *Physics World* (March 1998) illustrates in my view a complete breakdown of the both method and communication of science. The early workers thought hard about the assumptions they had made, and reported them in their PhD theses. But in their published papers the story came over slightly differently, so that the media, *and others in the field*, gained the impression that once again quantum theory had been vindicated, *even though what was supposed to have happened was physically impossible*!

Thus assumptions that one worker accepted after careful consideration have come to be taken for granted by later workers. The most commonly recognized one is known as the 'detection loophole', and this is indeed a wide one! The one I concentrate on, however, is that of the independence of the emission events. This, as it happens, is another area where Einstein influenced quantum theory. The quantum theory idea is that individual atoms emit light as they change energy levels, and Einstein's formalism of this assumed that they all act independently. There is now a great deal of evidence that this is not always so, yet the experimenters drifted into making this a standard assumption. If the emissions were independent, then they could justify the practice of *adjusting their data* by the 'subtraction of accidentals'. So far as the 'visibility test' is concerned, the adjustment is simply a matter of subtracting a constant from every term, and it is a simple fact of algebra that this will increase the value.

In his published papers, this is what Alain Aspect (the most famous of the experimenters concerned) did. He went to great lengths *in his PhD thesis* (Aspect, 1983) to justify the adjustment, but in his published papers he presented it as a matter of course. He thereby effectively suppressed the actual data, for he did not publish sufficient information for the reader to be able to reconstruct it. This is not the only fault with the publications, but it is probably the easiest for the outsider to understand. It did not go unchallenged, but the challenge attracted little attention, and Aspect's refutation of it (Aspect and Grangier, 1985)—using, at it turns out, data from the one experiment out of his three for which the subtraction was unimportant—was accepted. Since the data that could have invalidated his argument was not published, even his challengers felt obliged to accept the verdict, though they (Emilio Santos in particular) have continued to reiterate that the data adjustment should not be done.

The situation today is that it is regarded in fundamental physics circles as heretical to challenge the 'fact' of quantum entanglement. The workers who do so (Marshall and Santos, for example) are ignored. Yet if their ideas, the most important of which is the purely wave nature of light, were taken into account we might be starting the next millennium with a totally different world view. The repercussions of dropping the photon are not confined just to the microscopic. It is entirely possible that a purely wave theory would allow a better understanding of how astronomical data is actually measured, which might lead to yet more possible causes for the famous 'red shifts' and hence challenge the evidence for the expansion of the universe!

REFERENCES

Aspect, A. (1983) *Trois tests expérimentaux des inégalités de Bell par mesure de corrélation de polarisation de photons*. PhD thesis No. 2674, Université de Paris-Sud, Centre D'Orsay.

Aspect, A. and Grangier, P. (1985) *Lettere al Nuovo Cimento*, 43: 345.
Bell, J. (1964) *Physics* 1: 195.
Born, M. (1971) *The Born-Einstein letters*. Macmillan.
Boschi, D. *et al.* (1997) *Physical Review Letters* 79: 2755.
Clauser, J.F. and Shimony, A. (1978) *Reports in Progress in Physics* 41: 1881.
Duncan, A.J. (1994) Private communication, September 1994.
Einstein, A., Podolsky, B. and Rosen, N. (1935) Can quantum-mechanical description of physical reality be considered complete? *Physical Review* 47: 777.
Gisin, N. (1998) Private communication, March 31, 1998.
Greenberger, D. and Zeilinger, A. (1995) Quantum theory still crazy after all these years. *Physics World*, September 33.
Hagley, E. *et al.* (1997) *Physical Review Letters* 79: 1.
Hardy, L.J. (1997) General discussion at the 6th UK Conference on Conceptual and Mathematical Foundations of Modern Physics, University of Hull, UK, September 8–12, 1997.
Heisenberg, W. (1971) *Physics and Beyond*. George Allen and Unwin.
Hofer, W. (1997) Private communication, May 2, 1997.
Holt, R.A. and Pipkin, F.M. (1974) *Harvard University preprint*.
Kracklauer, A. (1998) Private communication, May 10, 1998.
Lorentz, H.A. (1916) *Theory of Electrons*. Teubner.
Marshall, T.W., Santos, E. and Selleri, F. (1983) *Physics Letters A* 98: 5–9.
Matthews, R. (1998) Hidden truths. *New Scientist* 158 (23 May) 28.
PRL (1997a) Communication from Divisional Area Editor, *Physical Review Letters*, May 23, 1997.
PRL (1997b) Referee report, *Physical Review Letters*, July 18, 1997.
PRL (1998) Communication from Editor, *Physical Review Letters*, May 12, 1998.
Rarity, J. (1994) Private communications, October 1994.
Santos, E. (1995) Private discussion, University of Cantabria, Santander, Spain, June 1995.
Santos, E. (1997) Private communication, December 14, 1997.
Selleri, F. (1995a) Private communication, February 21, 1995.
Selleri, F. (1995b) Private discussion, University of Bari, Italy, May 1995.
Thompson, C.H. (1996) The chaotic ball: an intuitive model for EPR experiments. *Foundations of Physics Letters* 9: 357. Available at http://xxx.lanl.gov/abs/quant-ph/9611037.
Thompson, C.H. (1997) Timing, 'accidentals' and other artifacts in EPR experiments. Submitted to *Physical Review Letters*, April 1997; re-submitted November 1997; rejected, June 1998. Available at http://xxx.lanl.gov/abs/quant-ph/9711044.
Thompson, C.H. (1998a) Behind the scenes at the EPR magic show. In Selleri, F. (ed). *Open Questions in Relativistic Physics*. Montreal Apeiron, pp. 351-359. (Proceedings of Athens conference, June 25–28, 1997, on 'Relativistic Physics and some of its Applications'.)
Thompson, C.H. (1998b) EPR, magic and the nature of light. In Hunter, G., Vigier, J.-P. and Jeffers, S. (eds). *Causality and Locality in Modern Physics*, Kluwer, pp. 209–218.
Tittel, W., Brendel, J., Gisin, B., Herzog, T. and Gisin, N. (1997) Experimental demonstration of quantum-correlations over more than 10 kilometers. *Physical Review A* 57: 3229. Available at http://xxx.lanl.gov/abs/quant-ph/9707042.
Wallace, B. (1993) *The Farce of Physics*. Available electronically at http://www.europa.com/~rsc.