Time, Finite Statistics, and Bell's Fifth Position

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Abstract

In this contribution to the 2002 Vaxjo conference on the foundations of quantum mechanics and probability, I discuss three issues connected to Bell's theorem and Bell-CHSH-type experiments: time and the memory loophole, finite statistics (how wide are the error bars, under local realism?), and the question of whether a loophole-free experiment is feasible, a surprising omission on Bell's list of four positions to hold in the light his results. Lévy's (1935) theory of martingales, and Fisher's (1935) theory of randomization in experimental design, take care of time and of finite statistics. I exploit a (classical) computer network metaphor for local realism to argue that Bell's conclusions are independent of how one likes to interpret probability. I give a critique of some recent anti-Bellist literature.

1. Introduction

It has always amazed me that anyone could find fault with Bell (1964)). Quantum mechanics cannot be cast into a classical mold. Well, isn't that delightful? Don't Bohr, von Neumann, Feynman, all tell us this, each in their own way? Why else are we fascinated by quantum mechanics? Moreover Bell writes with such economy, originality, modesty, and last but not least, humour.

I want to make it absolutely clear that I do not think that quantum mechanics is non-local. Bell also made it clear that his work did not *prove* that. In fact, in Bell (1981), the final section of the paper on Bertlmann's famous socks (chapter 16 of Bell (1987)), he gave a list of *four* quite different positions one could take, each one logically consistent with his mathematical

results. One of them is simply *not to care*: go with Bohr, don't look for anything behind the scenes, for if you do you will get stuck in meaningless paradoxes, meaningless because there no necessity for anything behind the scenes. If, however, like Bell himself, you have a personal preference for imagining a *realistic* world behind the scenes, accept with Bell that it must be *non-local*. You will be in excellent company: with Bohm-Riley, with Girardi-Rimini-Weber (the continuous spontaneous localization model), and no doubt with others. Alternatively, accept even worse consequences—on which more, later.

However at Vaxjo the anti-Bellists seemed to form a vociferous majority, though each anti-Bellist position seemed to me to be at odds with each other one. All the same, I will in this paper outline a recent *positive* development: namely, a strengthening of *Bell's inequality*. This strengthening does not strengthen *Bell's theorem*—quantum mechanics is incompatible with local realism—but it does strengthen experimental evidence for the ultimately more interesting conclusion: laboratory reality is incompatible with local realism.

You may have a completely different idea in your head from mine as to what the phrases *local realism* and *quantum mechanics* stand for. As also was made clear at Vaxjo, a million and one different interpretations exist for each. Moreover these interpretations depend on interpretations of yet other basic concepts such as *probability*. However let me describe my concrete mathematical results first, and turn to the philosophy later. After that, I will discuss some (manifestly or not) anti-Bellist positions, in particular those of Accardi, Hess and Philipp, 't Hooft, Khrennikov, Kracklauer, and Volovich.

I mentioned above that Bell (1981) lists four possible positions to hold, each one logically consistent with his mathematical results. Naturally they were not meant to be exhaustive and exclusive, but still I am surprised that he missed a to my mind very interesting fifth possibility: namely, that any experiment which quantum mechanics itself allows us to do, of necessity contains a loophole, preventing one from drawing a watertight conclusion against local realism. Always, *because of quantum mechanics*, it will be possible to come up with a local-realistic explanation (but each time, a different one). This logical possibility has some support from Volovich's recent findings, and moreover makes 't Hooft's enterprise less hopeless than the other four possibilities would suggest. (I understand that Ian Percival has earlier promoted a similar point of view).

Personally, I do not have a preference for this position either, but put it forward in order to urge the experimentalists to go ahead and prove me wrong. It is a pity that the prevailing opinion, that the loophole issue is dead

since each different loophole has been closed in a different experiment, is a powerful social disincentive against investing one's career in doing the definitive (loophole free) experiment.

2. A Computer Network Metaphor

To me, "local realism" is something which I can understand. And what I can understand are computers (idealised, hence perfect, classical computers) whose state at any moment is one definite state out of some extremely large (albeit finite) number, and whose state changes according to definite rules at discrete time points. Computers can be connected to one another and send one another messages. Again, this happens at discrete time points and the messages are large but discrete. Computers have memories and hard disks, on which can be stored huge quantities of information. One can store data and programs on computers. In fact what we call a program is just data for another program (and that is just data for another program ... but not ad infinitum).

Computers can simulate randomness. Alternatively one can, in advance, generate random numbers in any way one likes and store them on the hard disk of one's computer. With a large store of outcomes of fair coin tosses (or whatever for you is the epitome of randomness) one can simulate outcomes of any random variables or random processes with whatever probability distributions one likes, as accurately as one likes, as many of them as one likes, as long as one's computers (and storage facilities) are large and fast enough. In the last section of the paper I will further discuss whether there is any real difference between random number generation by tossing coins or by a pseudo-random number generator on a computer.

Computers can be cloned. Conceptually, one can take a computer and set next to it an identical copy, identical in the sense not only that the hardware and architecture are the same but moreover that every bit of information in every register, memory chip, hard disk, or whatever, is the same.

Computer connections can be cloned. Conceptually one can collect the data coming through a network connection, and retransmit two identical streams of the same data.

Consider a network of five computers connected linearly. I shall call them $\mathbb{A}$, $\mathscr{X}$, $\mathscr{O}$, $\mathscr{Y}$ and $\mathbb{B}$. The rather plain "end" computers $\mathbb{A}$ and $\mathbb{B}$ are under my control, the more fancy "in between" computers $\mathscr{X}$, $\mathscr{O}$ and $\mathscr{Y}$ are under the control of an anti-Bellist friend called Luigi. My friend Luigi has come up with a local realistic theory intended to show that Bell was wrong,

it is possible to violate the Bell inequalities in a local realistic way. I have challenged my friend to implement his theory in some computer programs, and to be specific I have stipulated that he should violate the Clauser, Horne, Shimony, and Holt (1969) version of the Bell inequalities, as this version is the model for the famous Aspect, Dalibard, and Roger (1982) experiment, and a host of recent experiments such as that of Weihs et al. (1998). Moreover this experimental protocol was certified by Bell himself, for instance in the "Bertlmann's socks" chapter of "Speakable and Unspeakable", as forming the definitive test of his theory.

Another of my anti-Bellist friends, Walter, has claimed that Bell neglected the factor *time* in his theory. Real experiments are done in one laboratory over a lengthy time period, and during this time period, variables at different locations can vary in a strongly correlated way—the most obvious example being real clocks! Well, in fact it is clear from "Bertlmann's socks" that Bell was thinking very much of time as being a factor in classical correlation, see his discussion of the temporal relation between the daily number of heart-attacks in Lyons and in Paris (the weather is similar, French TV is identical, weekend or weekday is the same ...). In the course of time, the state of physical systems can drift in a systematic and perhaps correlated way. This means that the outcomes of consecutive measurements might be correlated in time, probability distributions are not stationary, and statistical tests of significance are invalidated. Information from the past is not forgotten, but accumulates. The phenomenon has been named "the memory loophole". More insidiously, in the course of time, information can propagate from one physical subsystem to another, making everything even worse. (Think of French TV, reporting events in both Paris and Lyons with a short time lag.) In order to accomodate *time* I will allow Luigi to let his computers communicate between themselves whatever they like, in between each separate measurement, and I will make no demands whatsover of stationarity or independence. I do not demand that he simulates specific measurements on a specific state. All I demand is that he violates a Bell-CHSH inequality. I suggest that he goes for the maximal $2\sqrt{2}$ deviation corresponding to a certain state and collection of measurement settings, but the choice is up to him, since he has total control over his computers, and these choices are out of my control. My computers are just going to supply the results of independent fair coin tosses.

The experiment can only generate a finite amount of data. How are we going to decide whether the experiment has proved anything? How large should N be and what is a criterion we can both agree to? A physicist would say that we have a problem of *finite statistics*.

One of my pro-Bellist friends, Gregor, an experimental physicist, has claimed that his experiment shows a thirty standard deviations departure from local realism. As a statistician I am concerned that his calculation of "thirty standard deviations" was done assuming Poisson statistics, which comes down to assuming independence between succesive measurements, while the anti-Bellist, because of the memory loophole, need not buy this assumption, hence need not buy the conclusion. As a statistician I realise that I must do my probability calculation from the point of view of the local realist (even if in my opinion this point of view is wrong). I must show that, assuming a local realist position, the probability of such an extreme deviation as is actually observed is very small. This is not the same as showing that, assuming quantum mechanics is true, the probability that my experiment would have given the "wrong" conclusion (i.e., a conclusion favourable to the local realist) is very small. Of course it is a comfort to know this in advance of doing the experiment, and retrospectively it confirms the experimenter's skill, but to the local realist it is just irrelevant.

Now here an interesting paradox appears: a local realist theory is typically a deterministic theory, hence does not allow one to make probability assumptions at all. However I think that even local realists agree that there are situations where one can meaningfully talk probability, even if any person's stated interpretation of the word might appear totally different from mine. However he interprets the word probability, most local realists will agree that in a well equipped laboratory we could manufacture something pretty close to an idealised fair coin (by which I mean a coin together with a well-designed coin tossing apparatus). It could be close enough, for instance, that we would both be almost certain that in 40 000 tosses the number of heads will not exceed 20 000 by more than 1 000 (10 standard deviations). Behind this lies a combinatorial fact: the number of binary sequences of length 40 000, in which the number of 1's exceeds 20 000 by more that 1 000, is less than a fraction $\exp(-\frac{1}{2}10^2)$ of the total number of sequences.

So I hope my anti-Bellist friends will let me (the person in control of computers $\mathbb{A}$ and $\mathbb{B}$) either, ahead of the experiment, store the outcomes of fair coin tosses in them, or simulate them with a good pseudo-random number generator, and more importantly, will be convinced when I give probability statements concerning this and only this source of randomness in our computer experiment.

Now here are the rules of our game. We are going to simulate an idealised, perfect (no classical loopholes) Bell-CHSH type delayed choice experiment. For the sake of argument let us fix $N = 15\,000$ as the total number of *trials* (pairs of events, photon pairs, ...). In advance, Luigi has set up

his three computers with any programs or data whatsoever stored on them. He is allowed to program his chameleon effect, or Walter's B-splines and hidden-variables-which-are-not-actually-elements-of-reality, or Al's theory of QEM, whatever he likes.

For $n = 1, \ldots, N = 15\,000$, consecutively, the following happens:

1. Computer $\mathscr{O}$, which we call the *source*, sends information to computers $\mathscr{X}$ and $\mathscr{Y}$, the *measurement stations*. It can be anything. It can be random (previously stored outcomes of actual random experiments) or pseudo-random or deterministic. It can depend in an arbitrary way on the results of past trials (see item 5). Without loss of generality it can be considered to be the same—send to each computer, both its own message and the message for the other.

2. Computers $\mathbb{A}$ and $\mathbb{B}$, which we call the *randomizers*, each send a *measurement-setting-label*, namely a 1 or a 2, to computers $\mathscr{X}$ and $\mathscr{Y}$. Actually, I will generate the labels to simulate independent fair coin tosses (I might even use the outcomes of real fair coin tosses, done secretly in advance and saved on my computers' hard disks).

3. Computers $\mathscr{X}$ and $\mathscr{Y}$ each output an outcome ± 1, computed in whatever way Luigi likes from the available information at each measurement station. He has all the possibilities mentioned under item 1. What each of these two computers do not have, is the measurement-setting-label which was delivered to the other. Denote the outcomes $x^{(n)}$ and $y^{(n)}$.

4. Computers $\mathbb{A}$ and $\mathbb{B}$ each output the measurement-setting-label which they had previously sent to $\mathscr{X}$ and $\mathscr{Y}$. Denote these labels $a^{(n)}$ and $b^{(n)}$. An independent referee will confirm that these are identical to the labels given to Luigi in item 2.

5. Computers $\mathscr{X}$, $\mathscr{O}$ and $\mathscr{Y}$ may communicate with one another in any way they like. In particular, all past setting labels are available at all locations. As far as I am concerned, Luigi may even alter the computer programs or memories of his machines.

At the close of these $N = 15\,000$ trials we have collected N quadruples $(a^{(n)}, b^{(n)}, x^{(n)}, y^{(n)})$, where the measurement-setting-labels take the values 1 and 2, the measurement outcomes take the values ± 1. We count the the number of times the two outcomes were equal to one another, and the number of times they were unequal, separately for each of the four possible com-

binations of measurement-setting-labels:

$$N_{ab}^{=} = \#\{n : x^{(n)} = y^{(n)}, (a^{(n)}, b^{(n)}) = (a, b)\},$$
$$N_{ab}^{\neq} = \#\{n : x^{(n)} \neq y^{(n)}, (a^{(n)}, b^{(n)}) = (a, b)\},$$
$$N_{ab} = \#\{n : (a^{(n)}, b^{(n)}) = (a, b)\}.$$

From these counts we compute four empirical *correlations* (a mathematical statistician would call them raw, or uncentred, product moments), as follows.

$$\widehat{\rho}_{ab} = \frac{N_{ab}^{=} - N_{ab}^{\neq}}{N_{ab}}.$$

Finally we compute the CHSH contrast

$$\widehat{S} = \widehat{\rho}_{12} - \widehat{\rho}_{11} - \widehat{\rho}_{21} - \widehat{\rho}_{22}.$$

Luigi's aim is that this number is close to $2\sqrt{2}$, or at least, much larger than 2. My claim is that it cannot be much larger than 2; in fact, I would not expect a deviation larger than several times $1/\sqrt{N}$ above 2. Weihs et al. (1998) obtained a value of $\widehat{S} \approx 2.73$ also with $N \approx 15\,000$ in an experiment with a similar layout, except that the measurement stations were polarizing beam-splitters measuring pairs of entangled photons transmitted from a source through 200m of glass fibre each, and the randomizers were quantum optical devices simulating (close to) fair coin tosses by polarization measurements of completely unpolarized photons, see Appendix 1. A standard statistical computation showed that the value of $\widehat{S}$ they found is 30 standard deviations larger than 2.

Please note that Luigi's aim is certainly achievable from a logical point of view. It is conceivable, even, that $N_{12}^{\neq} = 0$ and $N_{11}^{=} = N_{21}^{=} = N_{22}^{=} = 0$, hence that $\widehat{\rho}_{12} = +1$, $\widehat{\rho}_{11} = \widehat{\rho}_{21} = \widehat{\rho}_{22} = -1$, and hence that $\widehat{S} = 4$. In fact if Luigi would generate his outcomes just as I generated the settings, as independent fair coin tosses, this very extreme result does have a positive probability. The reader might like to compute the chance.

In order to be able to make a clean probability statement, I would like to make some harmless modifications to $\widehat{S}$. First of all, note that the "correlation" between binary (± 1 valued) random variables is twice the probability that they are equal, minus 1:

$$\widehat{\rho}_{ab} = \frac{N_{ab}^{=} - (N_{ab} - N_{ab}^{=})}{N_{ab}} = 2\frac{N_{ab}^{=}}{N_{ab}} - 1.$$

Define

$$\widehat{p}_{ab}^{=} = N_{ab}^{=}/N_{ab}.$$

Luigi's aim is to have

$$(\widehat{S} - 2)/2 \;=\; \widehat{p_{12}^{=}} - \widehat{p_{11}^{=}} - \widehat{p_{21}^{=}} - \widehat{p_{22}^{=}}$$

close to $\sqrt{2} - 1$, my claim is that it won't be much larger than 0. Now multiply $(\widehat{S} - 2)/2$ by $N/4$ and note that the four denominators N_{ab} in the formulas for the $\widehat{p_{ab}^{=}}$ will all be pretty close to the same value, $N/4$. I propose to focus on the quantity $Z \approx N(\widehat{S} - 2)/8$ obtained by cancelling the four denominators against $N/4$:

$$Z \;=\; N_{12}^{=} - N_{11}^{=} - N_{21}^{=} - N_{22}^{=}.$$

Luigi's aim is to have this quantity close to $N(\sqrt{2} - 1)/4 \approx N/10$, or at least, significantly larger than 0, while I do not expect it to be larger than 0 by several multiples of $\sqrt{N}$.

He will not succeed. It is a theorem that *whatever Luigi's programs and stored data*, and *whatever communication between them at intermediate steps*,

$$\Pr\left\{ Z \geq k\sqrt{N} \right\} \;\leq\; \exp\left(-\tfrac{1}{2}k^2\right),$$

where $k \geq 0$ is arbitrary. For instance, with $N = 15\,000$, and $k = 12.25$, one finds that $k\sqrt{N} \approx N/10$ while $\exp(-\tfrac{1}{2}12.25^2) \leq 10^{-32}$.

In fact I can improve this result—as if improvement were necessary!—replacing k in the *right hand side* by a number one and three quarters times as large, by a technique called random time change, which I shall explain later. But I *cannot* get any further improvement, in particular, I cannot reach $\exp(-\tfrac{1}{2}30^2)$, corresponding to Weihs et al.'s (1998) thirty standard deviations. Why? Because their calculation (with $N \approx 15\,000$) was done assuming independent and identically distributed trials, and assuming probabilities equal to the observed relative frequencies, very close to those predicted by quantum mechanics; whereas my calculation is done *assuming local realism*, under the most favourable conditions possible under local realism, and assuming no further randomness than the independent fair coin tosses of the randomizers.

If you are unhappy about my move from correlations to counts, let me just say that I can make similar statements about the original $\widehat{S}$, by combining the probability inequality for Z with similar but easier probability inequalities for the N_{ab}.

3. Martingales

Let me give a sketch of the proof. I capitalize the symbols for the settings and outcomes because I am thinking of them as random variables. Write each of the counts in the expression for Z as a sum over the N trials of an indicator variable (a zero/one valued random variable) indicating whether or not the event to be counted occured on the nth trial. A difference of sums is a sum of differences. Consequently, if we define

$$
\begin{aligned}
\Delta_{ab}^{(n)} &= \mathbb{1}\{X^{(n)} = Y^{(n)}, \, (A^{(n)}, B^{(n)}) = (a, b)\}, \\
\Delta^{(n)} &= \Delta_{12}^{(n)} - \Delta_{11}^{(n)} - \Delta_{21}^{(n)} - \Delta_{22}^{(n)}, \\
Z^{(n)} &= \sum_{m=1}^{n} \Delta^{(m)},
\end{aligned}
$$

then $Z = Z^{(N)}$. Now I will show in a moment, using a variant of Bell's 1964 argument, that for each n, conditional on the past history of the first $n-1$ trials, the expected value of $\Delta^{(n)}$ does not exceed 0, whatever that history might be. Moreover, $\Delta^{(n)}$ can only take on the values -1, 0 and 1, in particular, its maximum minus its minimum possible value (its range) is less than or equal to 2. This makes the stochastic process $Z^{(0)}, Z^{(1)}, \ldots, Z^{(n)}, \ldots, Z^{(N)}$ a *supermartingale* with *increments in a bounded range*, and with initial value $Z^{(0)} = 0$. The definition of a supermartingale is precisely the property that the increments $\Delta^{(n)}$ have nonpositive conditional expectation given the past, for each n. A supermartingale is a generalisation of a random walk with zero or negative drift. Think for instance of the amount of money in your pocket as you play successive turns at a roulette table, where the roulette wheel is perfect, but the presence of a `0` and `00` means that on average, whatever amount you stake, and whatever you bet on, you lose $1/19$ of your stake at each turn. You may be using some complex or even randomized strategy whereby the amount of your stake, and what you bet on (a specific number, or red versus black, or whatever) depends on your past experience and on auxiliary random inputs but still you lose on average, conditional on the past at each time point, whatever the past. The capital of the bank is a submartingale—nonnegative drift. If there would be no `0` and `00`, both capitals would be martingales—zero drift. In a real roulette game there will be a maximum stake and hence a maximum payoff. Your capital changes by an amount between the maximal payoff and minus the maximal stake. Thus your capital while playing roulette develops in time as a supermartingale with increments of bounded range (maximal payoff plus maximal stake). If you cannot play more than N turns, with whatever strategy you like (includ-

ing stopping early), it can only very rarely happen that your capital increases by more than a few times $\sqrt{N}$ times half the range, as we shall now see.

According to Hoeffding's 1963 inequality, if a supermartingale $(Z^{(n)} : n = 0, 1, \ldots, N)$ is zero at time $n = 0$, and the range of its increments is bounded by 2, then

$$\Pr\left\{\max_{n \leq N} Z^{(n)} \geq k\sqrt{N}\right\} \leq \exp\left(-\tfrac{1}{2}k^2\right).$$

Note that if the increments of the supermartingale were actually independent and identically distributed, with range bounded by 2, then the maximum variance of $Z^{(N)}$ is precisely equal to N, achieved when the increments are equal to ± 1 with equal probability $\frac{1}{2}$. The Chebyshev inequality (sometimes known as Markov inequality) would then tell us that $Z^{(N)}$ exceeds $k\sqrt{N}$ with probability smaller than $1/k^2$. Hoeffding has improved this in two ways: an exponentially instead of geometrically decreasing probability, and a maximal inequality instead of a pointwise inequality. One cannot do much better than this inequality: in the most favourable case, just described, for large N we would have that $Z^{(N)}$ is approximately normally distributed with variance N, and the probability of large deviations of a normal variate behaves up to a constant and a lower order (logarithmic) term precisely like $\exp(-\tfrac{1}{2}k^2)$.

The proof of Hoeffding's inequality can be found in the better elementary probability textbooks and uses Markov's inequality, together with a random time change argument, and finally some elementary calculus. This gives a clue to how I can improve the result: consider the random process only at the times when $\Delta^{(n)} \neq 0$. In other words, thin out the time points $n = 0, 1, \ldots$ in a random way, only look at the process at the time points which are left. By Doob's optional stopping theorem it is *still* a supermartingale when only looked at intermittently, even when we only look at random time points, provided that we never need to look ahead to select these time points. The increments of the thinned process still have a range bounded by 2. Hence Hoeffding's inequality still applies. However, time is now running faster, thus the value of N in the inequality as stated for the new process corresponds to cN in the old, with $c > 1$. In fact, in the actual experiment we only see a ± 1 in a fraction $0.325 = \frac{1}{4}\sum_{a,b} p_{a,b}^{=}$ of all trials, hence we can improve the k on the right hand side by a factor $1/\sqrt{0.325} = 1.75$, hence 12.25 can be increased to 21.5.

It remains to prove the supermartingale property. Consider the quantity $\Delta^{(n)}$. Condition on everything which happened in the first $n-1$ trials, and also on whatever new information Luigi placed on his computers between

the $n-1$st and nth trial. Consider the situation just after Luigi's computers $\mathscr{X}$ and $\mathscr{Y}$ have received their information from $\mathscr{O}$, just before they receive the settings from $\mathbb{A}$ and $\mathbb{B}$. Under my conditioning, the state of Luigi's computers is fixed (non random). Clone Luigi's computers (this is only a thought experiment). Give the first copy of computer $\mathscr{X}$ the input 1, as if this value came from $\mathbb{A}$, and give the second copy the input 2; do the same in the other wing of the experiment. Let's drop the upper index (n), and denote by x_1 and x_2 the outputs of the two clones of $\mathscr{X}$, denote by y_1 and y_2 the outputs of the two clones of $\mathscr{Y}$. Because we are conditioning on the past up till the generation of the settings in the nth trial, everything is deterministic except the two random setting labels, denoted by A and B. The actual output from the actual (uncloned) computer $\mathscr{X}$ is $X = x_A$, similarly in the other wing of the experiment. We find

$$\begin{aligned}
\Delta_{ab}^{(n)} &= \mathbb{1}\{X = Y, (A, B) = (a, b)\} \\
&= \mathbb{1}\{x_a = y_b\}\mathbb{1}\{(A, B) = (a, b)\}.
\end{aligned}$$

The (conditional) expectation of this quantity is $\mathbb{1}\{x_a = y_b\}/4$, since the randomizers still produce independent fair coin tosses given the past, and given whatever further modifications Luigi has made. Hence the expectation of $\Delta^{(n)}$ given the past up to the start of the nth trial equals one quarter times $\mathbb{1}\{x_1 = y_2\} - \mathbb{1}\{x_1 = y_1\} - \mathbb{1}\{x_2 = y_1\} - \mathbb{1}\{x_2 = y_2\}$. Now since the x_a and y_b only take the values ± 1, it follows that $(x_1 y_2)(x_1 y_1)(x_2 y_1)(x_2 y_2) = +1$. The value of a product of two ± 1 valued variables encodes their equality or inequality. We see that the number of equalities within the four pairs involved is even. It is not difficult to see that it follows from this, that the value of $\mathbb{1}\{x_1 = y_2\} - \mathbb{1}\{x_1 = y_1\} - \mathbb{1}\{x_2 = y_1\} - \mathbb{1}\{x_2 = y_2\}$ can only be 0 or -2, so is always less than or equal to 0. We have proved the required property of the conditional expectation of $\Delta^{(n)}$ conditioning not only on the past $n-1$ trials but also on what happens between $n-1$st and nth trial. Average over all possible inter-trial happenings, to obtain the result we want. The theorem is proved.

As I remarked before, computers $\mathscr{X}$, $\mathscr{O}$ and $\mathscr{Y}$ are allowed to communicate in anyway they like between trials, and Luigi is even allowed to intervene between trials, changing their programs or data as he likes, even in a random way if he likes. He can make use in all his computers of the outcomes of the randomizers at all previous trials. It does not help. No assumption has been made of any kind of long run stability of the outcomes of his computers, or stationarity of probability distributions. The only requirement has been on my side, that I am allowed to choose setting labels at random, again and again. Only this randomness drives my conclusion. You

may see my theorem as a combinatorial statement, referring to the fraction of results obtained under all the 4^N different combinations of values of all $a^{(n)}$ and $b^{(n)}$.

Further details are given in Gill (2003) though there I used the Bernstein rather than the Hoeffding inequality; Hoeffding turned out to give sharper results. A publication is in preparation giving more mathematical details and further results. In particular one can give similar Hoeffding bounds for the original quantity of interest $\widehat{S}$, and the unbiasedness of the two randomizers is not crucial. In fact Weihs had probabilities of heads equal to 0.48 and to 0.42 in the two wings of his experiment.

Martingales (avant la lettre) were introduced into probability theory by the great French probabilist Paul Lévy in 1935. The name martingale was given to them a few years later by his student Ville, who used them to effectively destroy Richard von Mises' programme to found probability on the notion of collectives and limiting relative frequencies. Only Andrei Nikolaevich Kolmogorov realized that this conclusion was false, and he went on to develop the notion of computational complexity based on von Mises' ideas. Later still, the Dutch mathematician Michiel van Lambalgen has shown that a totally rigorous mathematical theory of collectives can be derived if one replaces the axiom of choice (which makes mathematical existence theorems easy, a double edged sword since it creates pathologies as well as desired results) with an alternative axiom, closer to physical intuition.

The year 1935 also saw the introduction, by the great British statistician Sir Ronald Aylmer Fisher, of the notion of randomization into experimental design. He showed that randomized designs gave an experimenter total control of uncontrollable factors which could otherwise prevent any conclusions being drawn from an experiment.

4. Metaphysics

The interpretation of Bell's theorem depends on notions of what is quantum mechanics, what is local realism, and behind them, what is probability. By the way, Bell himself does not state a theorem; just shows that certain assumptions imply a certain inequality. He shows that under a conventional interpretation of quantum mechanics, this inequality could be violated. However, it has become conventional to call the statement that quantum mechanics and local realism are incompatible with one another, Bell's theorem. This is a very convenient label, all the more convenient since later authors have obtained the same conclusion through consideration of other predictions of

quantum mechanics, some of them not on the face of it involving an inequality as Bell's. Actually, Dam, Gill, and Grünwald (2003) argue elsewhere that these proofs of Bell's theorem without inequalities (Hardy, 1993), or even without probability (Greenberger, Horne, and A., 1989), do actually involve hidden probability inequalities.

On the one hand, Bell's theorem depends on an interpretation of quantum mechanics, together with an assumption that certain states and measurements, which one can consider as allowed by the mathematical framework, can also arise "in Nature", including Nature as manipulated by an experimenter in a laboratory. What I call Bell's missing fifth position, is the position that quantum mechanics itself forbids these states ever to exist. And not just the specific states and measurements corresponding to a particular proof of Bell's theorem, but any which one could use in the proof. Restricting attention to a Bell-CHSH type experimental set-up, one does not need to achieve the magic $2\sqrt{2}$, one only needs to significantly exceed the bound 2. However, let me briefly describe the calculations behind this magic number (an upper bound under quantum mechanics, according to the Cirel'son inequality), since this leads naturally to a discussion of the role of probability.

It is conventional and reasonable to take the Hilbert space corresponding to a physical system consisting of two well separated parts of space as being the tensor product of spaces corresponding to the two parts separately. To achieve $2\sqrt{2}$, we need that a state exists (can be made to exist) of the joint system, which can be written (approximately) in the form $|00\rangle + |11\rangle$ (up to normalization, and up to a tensor product with whatever else you like, pure or mixed); where as usual $|00\rangle = |0\rangle \otimes |0\rangle$, $|11\rangle = |1\rangle \otimes |1\rangle$, and $|0\rangle$ and $1\rangle$ stand for two orthonormal vectors in both the first and the second space. We need that one can simultaneously (to a good enough approximation) measure whether the first subsystem is in the state $\cos\alpha|0\rangle + \sin\alpha|1\rangle$ or in the state orthogonal to this, $\sin\alpha|0\rangle - \cos\alpha|1\rangle$; and whether the second is in the state $\cos\beta|0\rangle + \sin\beta|1\rangle$ or in $\sin\beta|0\rangle - \cos\beta|1\rangle$; where one may choose between $\alpha = \alpha_1$ or $\alpha = \alpha_2$, and between $\beta = \beta_1$ and $\beta = \beta_2$; and where a good choice of angles (settings) leading to the famous $2\sqrt{2}$ are $\alpha_1 = -\pi/4 - \pi/8$, $\alpha_2 = -\pi/8$, $\beta_1 = 0$, $\beta_2 = \pi/4$.

Conventionally it is agreed that the probability to find subsystem one in state $|\alpha\rangle = \cos\alpha|0\rangle + \sin\alpha|1\rangle$ and subsystem two in state $|\beta\rangle = \cos\beta|0\rangle + \sin\beta|1\rangle$, when prepared in $\Psi = (|00\rangle + |11\rangle)/\sqrt{2}$, is the squared length of the inner product of Ψ with $|\alpha\rangle \otimes |\beta\rangle$, which turns out to equal $\frac{1}{2}\cos^2(\alpha - \beta)$. This is the probability of the outcome $+1, +1$. The probability of $-1, -1$ turns out to be the same, while that of $+1, -1$ and of $-1, +1$ are both equal to $\frac{1}{2}\sin^2(\alpha - \beta)/2$. The marginal probabilities of ± 1 now turn out to equal $\frac{1}{2}$

and the probability of equal outcomes is $\cos^2(\alpha - \beta)$. Under the choices of angles above, one obtains $p_{12}^{=} = (1 + 1/\sqrt{2})/2 \approx 0.85$, while all the other $p_{ab}^{=} = (1 - 1/\sqrt{2})/2 \approx 0.15$. Consequently $p_{12}^{=} - p_{11}^{=} - p_{21}^{=} - p_{22}^{=} = (\sqrt{2} - 1)$.

Out of these calculations came *joint probabilities of outcomes of binary measurements* and every word here needs to be taken literally, if the argument is to proceed: there are measurements taken in both wings of the experiment, and each can only result in a ± 1. We use quantum mechanics to tell us what the probability of various combinations of outcomes is. Now there are a great many ways to try to make sense of the notion of probability, but everyone who uses the word in the context of quantum mechanics would agree that if one repeatedly measures a quantum system in the same state, in the same way, then relative frequencies of the various possible outcomes will stabilize in the long run, and they will stabilize to the probabilities, whatever that word may mean, computed by quantum mechanics. In the quantum version of our experiment, Z/N will stabilize to the value $(\sqrt{2} - 1)/4$.

My mathematical derivation of a stronger (probabilistic) version of the Bell inequalities did not hinge on any particular interpretation of probability. Someone who uses the word probability has a notion of fair coin tosses, and will not hesitate to apply probability theory to experiments involving nothing else than two times 15 000 fair coin tosses. If a certain event specified before the coins are tossed has a probability smaller than 10^{-32} one is not going to see that event happen (even though logically it *might* happen).

It seems to me that the *interpretation* of probability does not play any serious role in the ongoing controversy concerning Bell's theorem. What does play a role is that quantum mechanics is used to compute joint probabilities of outcomes of binary measurements.

Many quantum physicists will object that real physicists do not use quantum mechanics to compute probabilities, only the certain values of averages pertaining to huge collectives. Many others avoid recourse to Born's law by extending the quantum mechanical treatment to as large a part of the measurement device as possible. If probability is involved it appears to come in through an uncontroversial backdoor as statistical variation in the medium or the elements of the collective.

That may be the situation in many fields, but people in those fields do not then test Bell's theorem. The critical experiment involves binary outcomes and binary settings, committed to sequentially as I have outlined. A better objection is that in no experiment done to date, has the experimental protocal described in my computer metaphor been literally enforced. For instance, in Weihs et al.'s (1998) experiment, the only one to date where the randomization of detector settings at a sufficiently fast rate was taken seri-

ously (Aspect did his best but could only implement a poor surrogate), the $N = 15\,000$ events were post-selected from an enormously much larger collection of small time intervals in most of which there was no detection event at all in either wing of the experiment; in a small proportion there was one detection event in one wing of the experiment or the other but not both; and in a smaller proportion still, there was a detection event in both wings of the experiment. Bell's argument just does not work when the binary outcomes are derived from a post-experimental conditioning (post-selection) on values of other variables. Other experiments free of this loophole, did not (and could not) implement the delayed random choice of settings; for instance Rowe et al.'s (2001) experiments with trapped ions.

Bell was well aware of this problem. In "Bertlman's socks" he offers a resolution, whereby the source $\mathscr{O}$ may output at random time moments a signal that something is about to happen. Measurements at $\mathscr{X}$ and $\mathscr{Y}$ based on a stream of random settings from $\mathbb{A}$ and $\mathbb{B}$ take place continuously, but after the experiment has run for some time, one selects just those measurements within an appropriate time interval after a saved "alert" message from $\mathscr{O}$. It is practically extremely important that this selection may be done *after* the experiment has run its course. Post-selection is bad, but post-pre-selection is fine.

By the way, the martingale methods I outlined above are admirably suited to adaptation and extension to continuous time measurement (of discrete events). Under reasonable (but of course untestable) "unbiased detection" assumptions, one can obtain the same kind of inequalities, but now allowing detection events at random time points, and a random total number of events.

But is "local realism" adequately represented by my metaphor of a computer network? For Bell, the key property of the crucial experiment is that the measurement station $\mathscr{X}$ commits itself to a specific (binary) outcome, shortly after receiving a (binary) input from randomizer $\mathbb{A}$, before a signal from the other wing of the experiment could have arrived with information concerning the input which randomizer $\mathbb{B}$ generated in the other wing of the experiment. In the short time period between input of a and output of x, as far as the physical mechanism leading to the result x is concerned, we need only consider a bounded region of space which completely excludes the physical systems $\mathbb{B}$ and $\mathscr{Y}$. For me, "local realism" should certainly imply that a sufficiently detailed (microscopic) specification of the state in some bounded region of space would (mostly) fix the outcomes of macroscopic, discrete (for instance, binary) variables. For instance, a sufficiently detailed specification of the initial state of a coin-tossing apparatus would

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(mostly) fix the outcome. This does not prevent the outcome from being apparently random, on the contrary, but it does "explain" the apparent randomness through the variation of the initial conditions when the experiment is repeated.

This means that in a thought experiment one can clone the relevant aspects of the relevant portion of physical space, and one can carry out the thought experiment: feed into the same physical system both of the possible inputs from the randomizer and thereby fix both the possible outputs. The output you actually see is what you would have seen if you would have chosen, the input which you actually chose.

Bell (1964, 1987) used a statistical conditional independence assumption, together with an assumption that conditional probability distributions of outcomes in one wing of the experiment do not depend on settings in the other wing, rather than my "counterfactual definite" characterization of local realism. Actually it is a mathematical theorem that the two mathematical notions are equivalent to one another. Each implies the other. Note that I do not require that my counterfactual or hidden variables physically exist, whatever that might mean, but only that they can be mathematically introduced in such a way that the mathematical model with "counterfactuals" reproduces the joint probability distribution of the manifest variables.

In my opinion the present unfashionableness of counterfactual reasoning in the philosophy of science is quite misguided. We would not have ethics, justice, or science, without it.

The original EPR argument also gives support for these counterfactuals: we know that if one measures with the same settings in the two wings of the experiment, one would obtain the same outcomes. Hence a local realist (like Einstein) quite reasonably considers the outcome which one would find under a given setting in one wing of the experiment, as deterministically encoded in the physical state of that part of the physical system, just before it is measured, independently of how it is actually measured.

In my opinion the stylized computer network metaphor for a good Bell-CHSH type experiment is precisely what Bell himself was getting at. One cannot attack Bell on the grounds that this experiment has never been done yet. One might attack him on the grounds that it never can be done. One will need good reasons for this. His argument does not require photons, nor this particular state and these particular measurements. Again, showing that a particular experimental set-up using a particular kind of physical system is unfeasible, does not show that all experimental set-ups are unfeasible.

5. A Miscellany of Anti-Bellist Views

Bell's Four Positions

Bell offered four quite different positions which one might like to take compatible with his mathematical results. They were:

1. Quantum mechanics is wrong.

2. Predetermination.

3. Nature is non-local.

4. Don't care (Bohr) .

In my opinion he missed an intriging fifth position:

5. A decisive experiment *cannot* be done.

I would like to discuss a number of recent works in the light of these possibilities and the results I have described above.

Accardi and the Chameleon Effect

In numerous works L. Accardi claims that Bell's arguments are fundamentally flawed, because Bell could only think of randomness in a classical way: pulling coloured balls out of urns, where the colour you get to see was the colour which was already painted on the ball you happended to pick. If however you select a chameleon out of a cage, where some chameleons are mutant, and you place the chameleon on a leaf, it might turn green, or it might turn brown, but it certainly did not have that colour in advance.

This is certainly a colourful metaphor but I do not think that chameleons are that different from coloured billiard balls: according to Accardi's own story whether or not a chameleon is mutant is determined by its genes, which certainly did not get changed by picking up one chameleon or another; and a mutant chameleon always turns brown when placed on a green leaf.

The metaphor is also supposed to carry the idea that the measurement outcome is not a preexisting property of the object, but is a result of an evolution of measurement apparatus and measured object together. It seems to me that this is precisely Bohr's Copenhagen interpretation: one cannot see measurement outcomes separate from the total physical context in which they appear. Bohr's answer to EPR was to apply this idea also rigorously even when two parts of the measurement apparatus and two parts of the object being measured are light-years apart. This philosophy certainly abolishes the EPR paradox but to my mind hardly explains it.

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Accardi does provide some mathematics (of the quantum probability kind) which is supposed to provide a local realistic model of the EPR phenomenon. Naturally a good quantum theoretician is able to replace the von Neumann measurement of one photon by a Schrödinger evolution of a photon in interaction with a measurement device in such a way that though particle and apparatus together are still in a pure state at the end of the evolution, the reduced state of the measurement apparatus is a mixed state over two macroscopically distinct possibilities. One can do this for the two particles simultaneously and arrive at a mathematical model which reproduces the EPR correlations in a local way, in a sense that the various items in the model can be ascribed to separate parts of reality. I don't think it qualifies as a local realistic model.

However Accardi believes it is a local realistic model in the sense that he could have computer programs running on a network of computers which would simulate the EPR correlations, while implementing his mathematical theory. These computer programs have run through several versions but presently Accardi's web site does not seem to be accessible. Unfortunately none of the versions I have been able to test allowed me the sort of control over the protocol of the experiment, to which I am entitled under. In particular, I was not able to see the raw data, only correlations. However by setting $N = 1$ one can get some idea what is going on inside the blackbox. Surprisingly with $N = 1$ it was possible to observe a correlation of ± 1.4. Has the chameleon multiplied the outcome ± 1 in one wing of the experiment by $\sqrt{2}$? A later version of the program also allowed the outcome "no detection" and though the author still claims categorically that Bell was wrong, the main thrust of the paper seems now to be to model actual experiments, which as is abundantly known suffer seriously from the detection loophole.

The martingale results which I have outlined above were derived in order to determine how large N should be, so that I would have no danger of losing a public bet with Accardi, that his computer programs could not violate the Bell-CHSH inequalities in an Aspect-type experiment, which is to say an experiment with repeated random choice of settings. Since he was to be totally free in what he put on his computers I could not use standard statistical methods to determine a safe sample size. Fortunately the martingale came to my rescue.

Hess and Philipp and non-elements-of-reality

I first became aware of the contributions of Hess and Philipp through an article in the science supplement of a reliable Dutch newspaper. Einstein

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was right after all. It intrigued me to discover that there was a fatal time-loophole in Bell's theorem, when I had just succeeded in fixing this loophole myself in order to make a safe bet with Accardi.

The first publications by these authors appeared in print in somewhat mangled form, since the journal had requested that the paper be reduced and cut in two pieces. Some notational confusions and mismatches made it very difficult to follow the arguments. On the one hand the papers contained a long verbal critique of Bell, on the lines that correlations at a distance can easily be caused by synchronous systematic variation in other factors. This is Bell's own story of the frequency of heart attacks in distant French cities. On the other hand the papers contained a highly complex mathematical model which was supposed to represent a local realistic reproduction of the singlet correlations. Unfortunately the authors chose only to verify some necessary conditions for the locality of their model. Hidden variables which in the model were supposed to "belong" to one measurement station or the other were shown to be statistically independent of one another.

In the latest publication Hess and Philipp have given a more transparent specification of their model, and in particular have recognised the important role played by one variable which in their earlier work was either treated as a mere index or even suppressed from the notation altogether. This variable is supposed to represent some kind of micro-time variable which is resident in both wings of the experiment. It turns out to have a probability distribution which depends on the measurement settings in both wings of the experiment. The authors implicitly recognise that it is non-local but christen it a "non-element-of-reality". Thus non-local hidden variables are fine, we just should not think of them as being *real*. They wisely point out that it seems to be a very difficult problem to decide which variables are elements or reality and which are not. In Appendix 2, I give a simplified version of their model.

't Hooft and predetermination

't Hooft notes that at the Planck scale experimenters will not have much freedom to choose settings on a measurement apparatus. Thus Bell's position 2 gives license to search for a classical, local, deterministic theory behind the quantum mechanical theory of the world at that level. So far so good.

However, presumably the quantum mechanical theory of the world at the Planck scale is the foundation from which one can derive the quantum mechanical theory of the world at levels closer to our everyday experience. Thus, his classical, local and deterministic theory for physics at the Planck scale is a classical, local and deterministic theory for physics at the level of

present day laboratory experiments testing Bell's theorem. It seems to me that there are now two positions to take. The first one is that there is, also at our level, no free choice. The experimenter thinks he is freely choosing setting label number 2 in Alice's wing of the experimenter, but actually the photons arriving simultaneously in the other wing of the experiment, or the stuff of the measurement apparatus there, "know" this in advance and capitalize on it in a very clever way: they produce deviations from the Bell inequality, though not larger than Cirel'sons quantum bound of $2\sqrt{2}$ (they are, after all, bound by quantum mechanics). But we have no way of seeing that our "random" coin tosses are not random at all, but are powerfully correlated with forever hidden variables in measurement apparatus far away. I find it inconceivable that there is such powerful coordination between such totally different physical systems (the brain of the experimenter, the electrons in the photodetector, the choice of a particular number as seed of a pseudo-random number generator in a particular computer program) that Bell's inequality can be resoundingly violated in the quantum optics laboratory, but nature as a whole appears "local", and randomizers appear random.

Now "free choice" is a notion belonging to philosophy and I would prefer not to argue about physics by invoking a physicist's apparently free choice. It is a fact that one can create in a laboratory something which looks very like randomness. One can run totally automated Bell-type experiments in which measurement settings are determined by results of a chain of separate physical systems (quantum optics, mechanical coin tossing, computer pseudo-random number generators). The point is that if we could carry out a perfect and succesful Bell-type experiment, then if local realism is true an exquisite coordination persists throughout this complex of physical systems delivering precisely the right measurement settings at the two locations to violate Bell's inequalities, while hidden from us in all other ways.

There is another position, position 5: the perfect Bell-type experiment cannot be made. Precisely because there is a local realistic hidden layer to the deepest layer of quantum mechanics, when we separate quantum-entangled physical systems far enough from one another in order to do separate and randomly chosen measurements on each, the entanglement will have decayed so far that the observed correlations have a classical explanation. Loopholes are unavoidable and the singlet state is an illusion.

Khrennikov and exotic probability theories

In a number of publications Khrennikov constrasts a classical probability view which he associates with Kolmogorov, with a so-called contextualist

viewpoint. He also contrasts the Kolmogorov point of view and the von Mises (frequentist). Furthermore, he has suggested that the resolution of Bell's paradox might be found in some non-standard probability theory, for instance p-adic. A rationale for this might be that stabilization of relative frequencies might not be a fact at the micro-level, hence no classical probability theory can be applied there.

Let me first make some remarks on the question of whether an exotic probability theory might explain away the Bell paradox. Though there is no direct relation, I am reminded of an earlier attempt by Pitowsky (1989) to resolve all paradoxes through adopting a mathematically very sophisticated and non-standard version of probability theory, in that case, by allowing non-measurable random variables and events. If events are not measurable, and moreover have lower and upper probabilities equal to zero and one respectively, then relative frequencies do not converge, but can have all values between 0 and 1 as points of accumulation. This allows Pitowsky (1989) to wriggle out of the constraint of Bell's inequality. Each probability concerning hidden variables can take any value.

Now experimentalists know that relative frequencies of macroscopic outcomes do tend to converge under many repetitions of a carefully controlled experiment, whether in quantum mechanics or not. The proof of Bell's theorem as I give it does not require stabilization of relative frequencies of some further unspecified micro-variables, but of joint relative frequencies of macroscopic variables, both "what was actually measured" and of "what might have been measured". Moreover it assumes that the stabilized values respect, by showing statistical independence, the physical independence which follows from locality. The results of a coin toss on one side of Innsbruck campus is not correlated with a photon measurement on the other side. In the case of Pitowsky (1989), exotic probability does not "explain" at all; what is called an explanation is sleight-of-hand hidden under impressive (but very specialistic) mathematics. At best, the explanation would imply a physics which is even more weird than quantum mechanics.

I have yet to study the case for p-adic probability carefully, but a priori I am highly sceptical.

Regarding Kolmogorov and von Mises I have already remarked that I do not see any opposition between alternative views of probability here. Komogorov merely describes probability, von Mises tries to explain it. Komogorov's theory is mere accountancy. The underlying variable ω of a Komogorovian probability space is not a physical cause, a hidden variable, it is merely a label of a possible outcome. Naturally, in classical physical systems, there is a many-to-one correspondence between initial conditions and

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distinguishable final conditions, so one *could* think of ω as being an element of a big list of initial configurations. But this is not obligatory and, outside of physics, it is not usual. See Kolmogorov (1933) for very clear descriptions of what ω is supposed to stand for and how probability can be interpreted. I think you will find that Kolmogorov was definitely a contextualist.

Kracklauer and the bombs under Bell's theory

According to Kracklauer, one counter-example is enough to explode a theorem. Not content with one bomb he has come up with local realistic explanations of a large number of celebrated experiments in quantum mechanics. Unfortunately, showing that a long list of historical experiments did not *prove* what various experimenters and interpreters claim, does not prove a certain theory, which inspired those experiments, wrong.

On the theoretical side he also has a large number of arguments, but in my opinion none is persuasive. One is that in real experiments there are not binary outcomes but there is macroscopic photoelectric current. But one can convert a continuous current to a binary outcome (does it exceed a given threshold or not). Bell's argument just requires that binary outcomes are output and analysed; any intermediate steps are irrelevant.

Another argument is that photons do not actually exist. This certainly is a serious point regarding Bell-type experiments in quantum optics, and is connected to the Fifth Position, to which I will return. As a mathematician I have to admit that the word "photon" is perhaps no more than just a word. What we call a photon is associated with certain mathematical objects in certain theories of "electro-magnetic radiation" and associated with point-like events which one can identify in various experiments involving "light". Mathematics itself is just a game of logical manipulations of distinct symbols on pieces of paper. Bell was careful to describe his decisive experiment in terms of macroscopic every-day laboratory objects, and avoided any use of words like "particle" which only have a meaning within an existing theory.

Another argument is that the mathematics of spin does not involve Planck's constant hence does not involve quantum mechanics. The transfer of EPR to the realm of spin half or of photons is lethal. However, it seems to me that quantum mechanics is as much about incompatible observables as about Planck's constant.

Finally, Kracklauer enlists the support of the Jaynes (1989), who claimed to have resolved all probability paradoxes in physics by proper use of probability theory. According to E.T. Jaynes, Bell's factorization was an improper

use of the chain rule for conditional probability. Apparently Jaynes did not recognise an uncontroversial use of the notion of conditional independence. Suppose I have a large collection of pairs of dice. The two dice in each pair are identical. However half the pairs have two 1's, two 2's and two 3's on their faces, the half have two 4's, two 5's and two 6's. Call these Type 1 and Type 2 dice. Naturally if many times in succession, we take a random pair of dice, send one to Amsterdam and the other to Bagdad, and toss each dice once, there will be a strong correlation between the outcomes in the two locations. Denote by X and Y the outcomes at the two locations, and by T the type of the dice. Suppose moreover that the dice-throwing apparatus in Amsterdam and Bagdad each depend on a setting, called a and b, which is chosen by a technician in each laboratory. (The result of the setting is to bias the outcome in a way which I will not further specify here.) Bell calculates as follows:

$$
\begin{aligned}
\mathrm{E}_{ab}\{XY\} &= \mathrm{E}\{\,\mathrm{E}_{ab}\{XY \mid T\}\,\} \\
&= \Pr\{T = 1\}\,\mathrm{E}_{ab}\{XY \mid T = 1\} \;+\; \Pr\{T = 2\}\,\mathrm{E}_{ab}\{XY \mid T = 2\} \\
&= \Pr\{T = 1\}\,\mathrm{E}_a\{X \mid T = 1\}\,\mathrm{E}_b\{Y \mid T = 1\} \\
&\quad +\; \Pr\{T = 2\}\,\mathrm{E}_a\{X \mid T = 2\}\,\mathrm{E}_b\{Y \mid T = 2\}.
\end{aligned}
$$

Jaynes prefers to consider probabilities than expectations, that is fine. He points out that the mere fact that our probability of seeing a particular value for X is immediately changed when we are told the outcome of Y, does not mean any spooky action at a distance (as Bell also many times explained). He is also willing to apply the definition of conditional probability to write

$$
\begin{aligned}
\Pr_{ab}\{X = x, Y = y \mid T = t\} \\
= \Pr_{ab}\{X = x \mid Y = y, T = t\}\,\Pr_{ab}\{Y = y \mid T = t\}
\end{aligned}
$$

but then refuses to admit

$$
\begin{aligned}
\Pr_{ab}\{X = x \mid Y = y, T = t\} &= \Pr_a\{X = x \mid T = t\}, \\
\Pr_{ab}\{Y = y \mid T = t\} &= \Pr_b\{Y = y \mid T = t\},
\end{aligned}
$$

going on to say that Bell's theorem only prohibits Bell's kind of local hidden variable models, not all. He does not make any attempt to specify what he understands by a local model, and expresses great surprise at very new results of Steve Gull, presented at the same conference as Jaynes' own paper, in which a computer network metaphor is introduced and where it is shown that the singlet correlations cannot be simulated on such a network! (Steve

Gull faxed me his two pages of notes on this, which he likes to use an examination exercise. His proof uses Fourier analysis). Jaynes thought that it would take another 30 years to understand Gull's work, just as it had taken the world 20 years to understand Bell's (the decisive understanding having just come from E.T.). I am not impressed.

Bell's use of probability language was in 1964 still a bit clumsy. Jaynes' work led him to a strong sense that any probability paradox in physics is most likely the result of muddled thinking. I suspect that Jaynes was so confident of this general rule that he made no attempt to understand Bell's argument and consequently completely missed the point.

Volovich and the fifth position

Volovich's recent work shows that in an EPR type context of the state of two entangled particles propagating in three-dimensional space, quantum mechanics itself would prohibit a loophole free test of local realism. Basically, particles will be lost with a too large probability, and the detection loophole is present.

In my opinion it would be interesting to find out if this is generic. However one must bear in mind that Bell's theorem is not dependent on a particular kind of physical scenario (for instance, polarization of entangled photons). The mathematical analysis must be carried out at a much more fundamental level in order to show that no physical system consisting of two well separated subsystems can evolve into a sufficiently entangled state by any means whatsover.

I would rather expect progress here to come from 't Hooft's programme: show that quantum mechanics at the Planck scale has a local realistic explanation, show that quantum mechanics at our scale is a consequence, and hence that it too is constrained by local realism.

Alternatively progress will come from experiment: someone does carry out a loophole free Bell-CHSH type experiment, or does factor large integers in no time at all using a quantum computer.

6. Last Word

Tossing a coin, shuffling a pack or cards, picking a ball from an urn, are classical paradigms of randomness. Moreover all these experiments are well understood both from a physical and from a mathematical point of view. We understand perfectly well how small variations in initial conditions are mag-

nified exponentially and result in quite unpredictable macroscopic results. On the basis of physical symmetries we can propose uniform probability distributions over initial conditions, when listed appropriately, and can use this to predict the probabilities of macroscopic outcomes, for instance of biased roulette wheels. We understand moreover that the probabilities of the macroscopic outcomes are remarkably robust to the probability distribution of initial conditions. Finally, the probability conclusions are quite independent of the flavour of probability interpretation.

Actually, generating a pseudo-random number on a computer is no different, except that the fine control which we can impose on initial conditions and on each intermediate step means that the result is exactly reproducible. But one can also buy a coin-tossing apparatus which so precisely fixes the initial velocity and angular momentum (among other factors) of the coin being tossed, that (unless one is unfortunate and chooses initial conditions close to the boundary between "heads" and "tails" that the coin falls the same way, (almost) every time.

That statistical independence holds when well separated physical systems are each used to generate randomness, is not harder to understand. An extraordinarily exquisite coordination between the number of times a pack of cards is shuffled, and between the force used to spin a coin into the air, could produce any degree of correlation in their outcomes.

These considerations mean that for me, that Bell's theorem has more or less nothing to do with interpretations of probability. Classical physical randomness, and classical physical independence, are what are at stake. My conclusion (excluding the fifth position) is that quantum mechanics is definitely non-classical.

In order to establish that quantum mechanics is non classical, we had to assume that physical independence between randomization devices at separate locations in space is possible. We had to assume a degree of control on the amount of information passing from one physical system to another: when we press the button labelled "1" on one of the measurement devices, only the fact that it was that button and not the other is important for the subsequent physics, even though actually we exert more or less pressure, for a longer or shorter time, and thereby could unbeknown to us be introducing information from other locations and from the distant past into the apparatus. Bell's conditional independence assumption is a way to express the physical intuition, that even though this might introduce more statistical variation into the outcome, it cannot carry information from the other wing of the experiment, concerning the randomization outcome there.

I find it fascinating that in order to prove that quantum mechanics is

intrinsically probabilistic (the outcomes cannot be traced back to variation in initial conditions) we must assume that we can ourselves generate randomness. And in order to demonstrate the kind of non-separatbility implied by entanglement, we have to assume control and separation of the physical systems which we use in our experiments.

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Appendix 1: Weihs' data

		$b=1$	$b=1$	$b=2$	$b=2$
		$y=+1$	$y=-1$	$y=+1$	$y=-1$
$a=1$	$x=+1$	313	1728	1636	179
$a=1$	$x=-1$	1978	351	294	1143
$a=2$	$x=+1$	418	1683	269	1100
$a=2$	$x=-1$	1578	361	1386	156

The table show the numbers of occurrences of each of the 16 possible values of (a, b, x, y), see Weihs' 1999 thesis, page 113, available from his personal web pages at `www.quantum.at`. The grand total is $N = 14\,573$.

Appendix 2: A local model of the singlet correlations

I present a caricature of the Hess-Philipp model, `quant-ph/0212085`. The caricature has all those properties, on the basis of which Hess and

Philipp claimed its locality. However, the caricature is blatantly non local. This makes it clear that Hess and Philipp are only checking necessary conditions, not sufficient conditions, for locality. In my construction I will only consider planar settings (orientiations), and measure angles as fractions of 2π, thus settings a, b become points in the unit interval $[0, 1]$ with endpoints identified. I am going to construct random variables R, Λ^*, Λ^{**}, Λ whose joint probability distribution is allowed by Hess and Philipp to depend on a and b. Actually, my R will be a 2-vector. R is supposed to be some kind of microscopic (i.e., hidden to the experimenter) time variable. Λ^* and Λ^{**} are station variables. Λ is a source variable, transmitted to both stations.

Let a and b be given. Let Λ^*, Λ^{**}, and Λ be independent random variables, each uniformly distributed on $[0, 1]$. Define $R = (R_1, R_2)$ as follows:

$$R_1 \;=\; (\Lambda^{**} + a) \bmod 1, \tag{1}$$

$$R_2 \;=\; (\Lambda^* + b) \bmod 1, \tag{2}$$

As required by HP, conditional on R, the pair $(\Lambda^*, \Lambda^{**})$ is independent of Λ. All further independence properties desired by HP are trivially satisfied. However,

$$b \;=\; (R_2 - \Lambda^*) \bmod 1, \tag{3}$$

$$a \;=\; (R_1 - \Lambda^{**}) \bmod 1. \tag{4}$$

Consequently, given R and Λ^* one can reconstruct b; given R and Λ^{**} one can reconstruct a and Λ^*.

Finally, let $A = A(\Lambda^*, \Lambda, R, a)$ and $B = B(\Lambda^*, \Lambda, R, b)$ be functions taking values in $\{-1, +1\}$. From the given arguments to A and B, the missing station setting b and a can be reconstructed. From a, b and Λ one can construct a pair of binary random variables with joint probability distribution depending in any way one likes on a and b. In particular one can arrange to reproduce the singlet correlations.

To prove that both the HP model and this caricature are non-local, it suffices to observe that they reproduce the singlet correlations in a realistic fashion, and therefore by Bell's theorem cannot be local-realistic. However, according to Hess and Philipp this conclusion is short-sighted. Obviously, R is not an element of reality! The only elements of reality in my model are Λ, Λ^* and Λ^{**}. They are evidently local, so my model is local, after all.