16

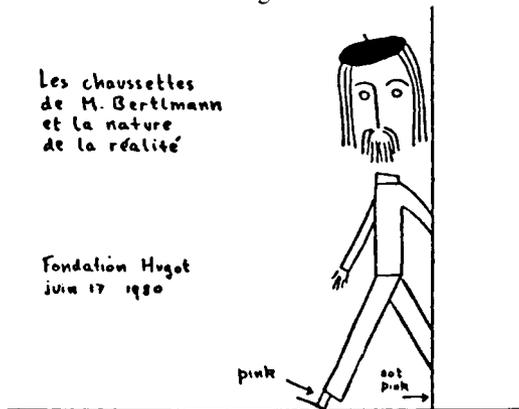## Bertlmann's socks and the nature of reality

Introduction

The philosopher in the street, who has not suffered a course in quantum mechanics, is quite unimpressed by Einstein–Podolsky–Rosen correlations[1]. He can point to many examples of similar correlations in everyday life. The case of Bertlmann's socks is often cited. Dr. Bertlmann likes to wear two socks of different colours. Which colour he will have on a given foot on a given day is quite unpredictable. But when you see (Fig. 1) that the first sock is pink you can be already sure that the second sock will not be pink. Observation of the first, and experience of Bertlmann, gives immediate information about the second. There is no accounting for tastes, but apart from that there is no mystery here. And is not the EPR business just the same?

Consider for example the particular EPR gedanken experiment of Bohm[2] (Fig. 2). Two suitable particles, suitably prepared (in the 'singlet spin state'), are directed from a common source towards two widely separated magnets followed by detecting screens. Each time the experiment is performed each of the two particles is deflected either up or down at

Fig. 1.

Les chaussettes
de M. Bertlmann
et la nature
de la réalité

Fondation Hugot
juin 17 1980

pink

not
pink

the corresponding magnet. Whether either particle separately goes up or down on a given occasion is quite unpredictable. But when one particle goes up the other always goes down and vice-versa. After a little experience it is enough to look at one side to know also about the other.

So what? Do we not simply infer that the particles have properties of some kind, detected somehow by the magnets, chosen à la Bertlmann by the source – differently for the two particles? Is it possible to see this simple business as obscure and mysterious? We must try.

To this end it is useful to know how physicists tend to think intuitively of particles with 'spin', for it is with such particles that we are concerned. In a crude classical picture it is envisaged that some internal motion gives the particle an angular momentum about some axis, and at the same time generates a magnetization along that axis. The particle is then like a little spinning magnet with north and south poles lying on the axis of rotation. When a magnetic field is applied to a magnet the north pole is pulled one way and the south pole is pulled the other way. If the field is uniform the net force on the magnet is zero. But in a non-uniform field one pole is pulled more than the other and the magnet as a whole is pulled in the corresponding direction. The experiment in question involves such non-

Fig. 2. Einstein–Podolsky–Rosen–Bohm gedanken experiment with two spin $\frac{1}{2}$ particles and two Stern–Gerlach magnets.
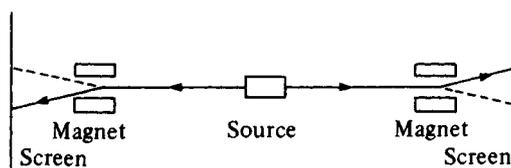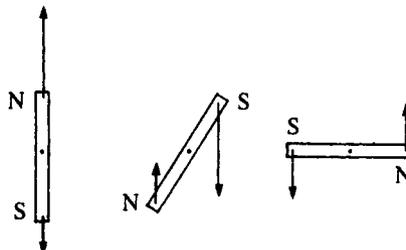

Fig. 3. Forces on magnet in non-uniform magnetic field. The field points towards the top of the page and increases in strength in that direction.

uniform fields – set up by so-called 'Stern–Gerlach' magnets. Suppose that the magnetic field points up, and that the strength of the field increases in the upward direction. Then a particle with south–north axis pointing up would be pulled up (Fig. 3). One with axis pointing down would be pulled down. One with axis perpendicular to the field would pass through the field without deflection. And one oriented at an intermediate angle would be deflected to an intermediate degree. (All this is for a particle of zero electric charge; when a charged particle moves in a magnetic field there is an additional force which complicates the situation.)

A particle of given species is supposed to have a given magnetization. But because of the variable angle between particle axis and field there would still be a range of deflections possible in a given Stern–Gerlach magnet. It could be expected then that a succession of particles would make a pattern something like Fig. 4 on a detecting screen. But what is observed in the simplest case is more like Fig. 5, with two distinct groups of deflections (i.e., up or down) rather than a more or less continuous band. (This simplest case, with just two groups of deflections, is that of so-called 'spin-$\frac{1}{2}$' particles; for 'spin-j' particles there are $(2j + 1)$ groups).

The pattern of Fig. 5 is very hard to understand in naïve classical terms. It might be supposed for example that the magnetic field first pulls the little magnets into alignment with itself, like compass needles. But even if this were dynamically sound it would account for only one group of deflections. To account for the second group would require 'compass-needles' pointing in the wrong direction. And anyway it is not dynamically sound. The internal angular momentum, by gyroscopic action, should stabilize the angle between particle axis and magnetic field. Well then, could it not be that the source for some reason delivers particles with axes pointing just one way or the other and not in between? But this is

Fig. 4. Naïve classical expectation for pattern on detecting screen behind Stern–Gerlach magnet.

Fig. 5. Quantum mechanical pattern on screen, with vertical Stern–Gerlach magnet.

easily tested by turning the Stern–Gerlach magnet. What we get (Fig. 6) is just the same split pattern as before, but turned around with the Stern–Gerlach magnet. To blame the absence of intermediate deflections on the source we would have to imagine that it anticipated somehow the orientation of the Stern–Gerlach magnet.

Phenomena of this kind[3] made physicists despair of finding any consistent space-time picture of what goes on on the atomic and subatomic scale. Making a virtue of necessity, and influenced by positivistic and instrumentalist philosophies[4], many came to hold not only that it is difficult to find a coherent picture but that it is wrong to look for one – if not actually immoral then certainly unprofessional. Going further still, some asserted that atomic and subatomic particles do not *have* any definite properties in advance of observation. There is nothing, that is to say, in the particles approaching the magnet, to distinguish those subsequently deflected up from those subsequently deflected down. Indeed even the particles are not really there.

For example[5], 'Bohr once declared when asked whether the quantum mechanical algorithm could be considered as somehow mirroring an underlying quantum reality: "There is no quantum world. There is only an abstract quantum mechanical description. It is wrong to think that the task of physics is to find out how Nature *is*. Physics concerns what we can say about Nature" '.

And for Heisenberg[6] '... in the experiments about atomic events we have to do with things and facts, with phenomena that are just as real as any phenomena in daily life. But the atoms or the elementary particles are not as real; they form a world of potentialities or possibilities rather than one of things or facts'.

And[7] 'Jordan declared, with emphasis, that observations not only *disturb* what has to be measured, they *produce* it. In a measurement of position, for example, as performed with the gamma ray microscope, "the electron is forced to a decision. We compel it *to assume a definite position*; previously it was, in general, neither here nor there; it had not yet made its decision for a definite position... If by another experiment the *velocity* of the electron is being measured, this means: the electron is compelled to decide itself for some exactly defined value of the velocity... we ourselves produce the results of measurement" '.

Fig. 6. Quantum mechanical pattern with rotated Stern–Gerlach magnet.

It is in the context of ideas like these that one must envisage the discussion of the Einstein–Podolsky–Rosen correlations. Then it is a little less unintelligible that the EPR paper caused such a fuss, and that the dust has not settled even now. It is as if we had come to deny the reality of Bertlmann's socks, or at least of their colours, when not looked at. And as if a child has asked: How come they always choose different colours when they *are* looked at? How does the second sock know what the first has done?

Paradox indeed! But for the others, not for EPR. EPR did not use the word 'paradox'. They were with the man in the street in this business. For them these correlations simply showed that the quantum theorists had been hasty in dismissing the reality of the microscopic world. In particular Jordan had been wrong in supposing that nothing was real or fixed in that world before observation. For after observing only one particle the result of subsequently observing the other (possibly at a very remote place) is immediately predictable. Could it be that the first observation somehow fixes what was unfixed, or makes real what was unreal, not only for the near particle but also for the remote one? For EPR that would be an unthinkable 'spooky action at a distance'[8]. To avoid such action at a distance they have to attribute, to the space-time regions in question, *real* properties in advance of observation, correlated properties, which *predetermine* the outcomes of these particular observations. Since these real properties, fixed in advance of observation, are not contained in quantum formalism[9], that formalism for EPR is *incomplete*. It may be correct, as far as it goes, but the usual quantum formalism cannot be the whole story.

It is important to note that to the limited degree to which *determinism* plays a role in the EPR argument, it is not assumed but *inferred*. What is held sacred is the principle of 'local causality' – or 'no action at a distance'. Of course, mere *correlation* between distant events does not by itself imply action at a distance, but only correlation between the signals reaching the two places. These signals, in the idealized example of Bohm, must be sufficient to *determine* whether the particles go up or down. For any residual undeterminism could only spoil the perfect correlation.

It is remarkably difficult to get this point across, that determinism is not a *presupposition* of the analysis. There is a widespread and erroneous conviction that for Einstein[10] determinism was always *the* sacred principle. The quotability of his famous 'God does not play dice' has not helped in this respect. Among those who had great difficulty in seeing Einstein's position was Born. Pauli tried to help him[11] in a letter of 1954:

> ... I was unable to recognize Einstein whenever you talked about him in either your letter or your manuscript. It seemed to me as if you had erected some dummy Einstein for yourself, which you then knocked down with great pomp. In particular Einstein does not consider the concept of 'determinism' to be as furdamental as it is frequently held to be (as he told me emphatically many times)... he *disputes* that he uses as a criterion for the admissibility of a theory the question: 'Is it rigorously deterministic?'... he was not at all annoyed with you, but only said you were a person who will not listen.

Born had particular difficulty with the Einstein–Podolsky–Rosen argument. Here is his summing up, long afterwards, when he edited the Born–Einstein correspondence[12].

> The root of the difference between Einstein and me was the axiom that events which happen in different places A and B are independent of one another, in the sense that an observation on the state of affairs at B cannot teach us anything about the state of affairs at A.

Misunderstanding could hardly be more complete. Einstein had no difficulty accepting that affairs in different places could be correlated. What he could not accept was that an intervention at one place could *influence*, immediately, affairs at the other.

These references to Born are not meant to diminish one of the towering figures of modern physics. They are meant to illustrate the difficulty of putting aside preconceptions and listening to what is actually being said. They are meant to encourage *you*, dear listener, to listen a little harder.

Here, finally, is a summing-up by Einstein himself[13]:

> If one asks what, irrespective of quantum mechanics, is characteristic of the world of ideas of physics, one is first of all struck by the following: the concepts of physics relate to a real outside world.... It is further characteristic of these physical objects that they are thought of as arranged in a space-time continuum. An essential aspect of this arrangement of things in physics is that they lay claim, at a certain time, to an existence independent of one another, provided these objects 'are situated in different parts of space'.
>
> The following idea characterizes the relative independence of objects far apart in space (A and B): external influence on A has no direct influence on B...
>
> There seems to me no doubt that those physicists who regard the descriptive methods of quantum mechanics as definitive in principle

would react to this line of thought in the following way: they would drop the requirement... for the independent existence of the physical reality present in different parts of space; they would be justified in pointing out that the quantum theory nowhere makes explicit use of this requirement.

I admit this, but would point out: when I consider the physical phenomena known to me, and especially those which are being so successfully encompassed by quantum mechanics, I still cannot find any fact anywhere which would make it appear likely that (that) requirement will have to be abandoned.

I am therefore inclined to believe that the description of quantum mechanics... has to be regarded as an incomplete and indirect description of reality, to be replaced at some later date by a more complete and direct one.

2 Illustration

Let us illustrate the *possibility* of what Einstein had in mind in the context of the particular quantum mechanical predictions already cited for the EPRB gedanken experiment. These predictions make it hard to believe in the completeness of quantum formalism. But of course outside that formalism they make no difficulty whatever for the notion of local causality. To show this explicitly we exhibit a trivial *ad hoc* space-time picture of what might go on. It is a modification of the naive classical picture already described. Certainly something must be modified in that, to reproduce the quantum phenomena. Previously, we implicitly assumed for the net force in the direction of the field gradient (which we always take to be in the same direction as the field) a form

$$F \cos \theta \qquad (1)$$

where θ is the angle between magnetic field (and field gradient) and particle axis. We change this to

$$F \cos \theta / |\cos \theta|. \qquad (2)$$

Whereas previously the force varied over a continuous range with θ, it takes now just two values, $\pm F$, the sign being determined by whether the magnetic axis of the particle points more nearly in the direction of the field or in the opposite direction. No attempt is made to explain this change in the force law. It is just an *ad hoc* attempt to account for the observations. And of course it accounts immediately for the appearance of just two groups of particles, deflected either in the direction of the magnetic field or in the opposite direction. To account then for the

Einstein–Podolsky–Rosen–Bohm correlations we have only to assume that the two particles emitted by the source have oppositely directed magnetic axes. Then if the magnetic axis of one particle is more nearly along (than against) one Stern–Gerlach field, the magnetic axes of the other particle will be more nearly against (than along) a parallel Stern–Gerlach field. So when one particle is deflected up, the other is deflected down, and vice versa. There is nothing whatever problematic or mind-boggling about these correlations, with parallel Stern–Gerlach analyzers, from the Einsteinian point of view.

So far so good. But now go a little further than before, and consider *non*-parallel Stern–Gerlach magnets. Let the first be rotated away from some standard position, about the particle line of flight, by an angle a. Let the second be rotated likewise by an angle b. Then if the magnetic axis of either particle separately is randomly oriented, but if the axes of the particles of a given pair are always oppositely oriented, a short calculation gives for the probabilities of the various possible results, in the *ad hoc* model,

$$\left. \begin{aligned} P(\text{up, up}) = P(\text{down, down}) = \frac{|a - b|}{2\pi} \\[2mm] P(\text{up, down}) = P(\text{down, up}) = \frac{1}{2} - \frac{|a - b|}{2\pi} \end{aligned} \right\} \tag{3}$$

where 'up' and 'down' are defined with respect to the magnetic fields of the two magnets. However, a quantum mechanical calculation gives

$$\left. \begin{aligned} P(\text{up, up}) = P(\text{down, down}) = \frac{1}{2}\left(\sin\frac{a - b}{2} \right)^2 \\[3mm] P(\text{up, down}) = P(\text{down, up}) = \frac{1}{2} - \frac{1}{2}\left(\sin\frac{a - b}{2} \right)^2 \end{aligned} \right\} \tag{4}$$

Thus the *ad hoc* model does what is required of it (i.e., reproduces quantum mechanical results) only at $(a - b) = 0$, $(a - b) = \pi/2$ and $(a - b) = \pi$, but not at intermediate angles.

Of course this trivial model was just the first one we thought of, and it worked up to a point. Could we not be a little more clever, and devise a model which reproduces the quantum formulae completely? No. It cannot be done, so long as action at a distance is excluded. This point was realized only subsequently. Neither EPR nor their contemporary opponents were aware of it. Indeed the discussion was for long entirely concentrated on the points $|a - b| = 0$, $\pi/2$, and π.

3 Difficulty with locality

To explain this dénouement without mathematics I cannot do better than follow d'Espagnat[14,15]. Let us return to socks for a moment. One of the most important questions about a sock is 'will it wash'? A consumer research organization might make the question more precise: could the sock survive one thousand washing cycles at 45 °C? Or at 90 °C? Or at 0 °C? Then an adaptation of the Wigner–d'Espagnat inequality[16] applies. For any collection of new socks:

$$\left.\begin{array}{c}\text{(the number that could pass at } 0° \text{ and not at } 45°)\\ \text{plus}\\ \text{(the number that could pass at } 45° \text{ and not at } 90°)\\ \text{is not less than}\\ \text{(the number that could pass at } 0° \text{ and not at } 90°)\end{array}\right\} \qquad (5)$$

This is trivial, for each member of the third group either could survive at 45°, and so is also in the second group, or could not survive at 45°, and so is also in the first group.

But trivialities like this, you will exclaim, are of no interest in consumer research! You are right; we are straining here a little the analogy between consumer research and quantum philosophy. Moreover, you will insist, the statement has no empirical content. There is no way of deciding that a given sock could survive at one temperature and not at another. If it did not survive the first test it would not be available for the second, and even if it did survive the first test it would no longer be new, and subsequent tests would not have the original significance.

Suppose, however, that the socks come in pairs. And suppose that we know by experience that there is little variation between the members of a pair, in that if one member passes a given test then the other also passes that same test *if* it is performed. Then from d' Espagnat's inequality we can infer the following:

$$\left.\begin{array}{c}\text{(the number of pairs in which one could pass at } 0° \text{ and}\\ \text{the other not at } 45°)\\ \text{plus}\\ \text{(the number of pairs in which one could pass at } 45° \text{ and}\\ \text{the other not at } 90°)\\ \text{is not less than}\\ \text{(the number of pairs in which one could pass at } 0° \text{ and}\\ \text{the other not at } 90°)\end{array}\right\} \qquad (6)$$

This is not yet empirically testable, for although the two tests in each

bracket are now on different socks, the different brackets involve different tests on the same sock. But we now add the random sampling hypothesis: if the sample of pairs is sufficiently large and if we choose at random a big enough subsample to suffer a given pair of tests, then the pass/fail fractions of the subsample can be extended to the whole sample with high probability. Identifying such fractions with *probabilities* in a thoroughly conventional way, we now have

$$
\left.
\begin{array}{c}
\text{(the probability of one sock passing at } 0° \text{ and} \\
\text{the other not at } 45°) \\
\text{plus} \\
\text{(the probability of one sock passing at } 45° \text{ and} \\
\text{the other not at } 90°) \\
\text{is not less than} \\
\text{(the probability of one sock passing at } 0° \text{ and} \\
\text{the other not at } 90°)
\end{array}
\right\} \quad (7)
$$

Moreover this is empirically meaningful is so far as probabilities can be determined by random sampling.

We formulated these considerations first for pairs of socks, moving with considerable confidence in those familiar objects. But why not reason similarly for the pairs of particles of the EPRB experiment? By blocking off the 'down' channels in the Stern–Gerlach magnets, allowing only particles deflected 'up' to pass, we effectively subject the particles to tests which they either pass or do not. Instead of temperatures we now have angles *a* and *b* at which the Stern–Gerlach magnets are set. The essential difference, a trivial one, is that the particles are paired à la Bertlmann – if one were to pass a given test the other would be sure to fail it. To allow for this we simply take the converse of the second term in each bracket:

$$
\left.
\begin{array}{c}
\text{(the probability of one particle passing at } 0° \text{ and} \\
\text{the other at } 45°) \\
\text{plus} \\
\text{(the probability of one particle passing at } 45° \text{ and} \\
\text{the other at } 90°) \\
\text{is not less than} \\
\text{(the probability of one particle passing at } 0° \text{ and} \\
\text{the other at } 90°)
\end{array}
\right\} \quad (8)
$$

In case any one finds the detour by socks a little long, let us look directly at this final result and see how trivial it is. We are assuming that particles have properties which dictate their ability to pass certain tests – whether or

not these tests are in fact made. To account for the perfect anticorrelation when identical tests (parallel Stern–Gerlach magnets) are applied to the two members of a pair, we have to admit that the pairing is generalized à la Bertlmann – when one has the ability to pass a certain test, the other has not. Then the above assertion about pairs is equivalent to the following assertion about either member:

$$
\left.
\begin{array}{c}
\text{(the probability of being able to pass at } 0° \text{ and} \\
\text{not able at } 45°) \\
\\
\text{plus} \\
\text{(the probability of being able to pass at } 45° \text{ and} \\
\text{not able at } 90°) \\
\text{is not less than} \\
\text{(the probability of being able to pass at } 0° \text{ and} \\
\text{not able at } 90°)
\end{array}
\right\} \qquad (9)
$$

And this is indeed trivial. For a particle able to pass at $0°$ and not at $90°$ (and so contributing to the third probability in (9)) is either able to pass at $45°$ (and so contributes to the second probability) or not able to pass at $45°$ (and so contributes to the first probability).

However, trivial as it is, the inequality is not respected by quantum mechanical probabilities. From (4) the quantum mechanical probability for one particle to pass a magnet with orientation a and the other to pass a magnet with orientation b (called P (up, up)) in (4) is

$$
\frac{1}{2}\left(\sin \frac{a-b}{2} \right)^2
$$

Inequality (9) would then require

$$
\tfrac{1}{2}(\sin 22.5°)^2 + \tfrac{1}{2}(\sin 22.5°)^2 \geqslant \tfrac{1}{2}(\sin 45°)^2
$$

or

$$
0.1464 \geqslant 0.2500
$$

which is not true.

Let us summarize once again the logic that leads to the impasse. The EPRB correlations are such that the result of the experiment on one side immediately foretells that on the other, whenever the analyzers happen to be parallel. If we do not accept the intervention on one side as a causal influence on the other, we seem obliged to admit that the results on both sides are determined in advance anyway, independently of the intervention on the other side, by signals from the source and by the local magnet setting. But this has implications for non-parallel settings which conflict with those

of quantum mechanics. So we *cannot* dismiss intervention on one side as a causal influence on the other.

It would be wrong to say 'Bohr wins again' (Appendix 1); the argument was not known to the opponents of Einstein, Podolsky and Rosen. But certainly Einstein could no longer write so easily, speaking of local causality '... I still cannot find any fact anywhere which would make it appear likely that that requirement will have to be abandoned'.

4 General argument

So far the presentation aimed at simplicity. Now the aim will be generality[17]. Let us first list some aspects of the simple presentation which are not essential and will be avoided.

The above argument relies very much on the perfection of the correlation (or rather anticorrelation) when the two magnets are aligned ($a = b$) and other conditions also are ideal. Although one could hope to approach this situation closely in practice, one could not hope to realize it completely. Some residual imperfection of the set-up would spoil the perfect anticorrelation, so that occasionally both particles would be deflected down, or both up. So in the more sophisticated argument we will avoid any hypothesis of perfection.

It was only in the context of perfect correlation (or anticorrelation) that *determinism* could be inferred for the relation of observation results to preexisting particle properties (for any indeterminism would have spoiled the correlation). Despite my insistence that the determinism was inferred rather than assumed, you might still suspect somehow that it is a preoccupation with determinism that creates the problem. Note well then that the following argument makes no mention whatever of determinism.

You might suspect that there is something specially peculiar about spin-$\frac{1}{2}$ particles. In fact there are many other ways of creating the troublesome correlations. So the following argument makes no reference to spin-$\frac{1}{2}$ particles, or any other particular particles.

Finally you might suspect that the very notion of particle, and particle orbit, freely used above in introducing the problem, has somehow led us astray. Indeed did not Einstein think that fields rather than particles are at the bottom of everything? So the following argument will not mention particles, nor indeed fields, nor any other particular picture of what goes on at the microscopic level. Nor will it involve any use of the words 'quantum mechanical system', which can have an unfortunate effect on the discussion. The difficulty is not created by any such picture or any such terminology. It is created by the predictions about the correlations in the visible outputs of certain conceivable experimental set-ups.

Consider the general experimental set-up of Fig. 7. To avoid inessential details it is represented just as a long box of unspecified equipment, with three inputs and three outputs. The outputs, above in the figure, can be three pieces of paper, each with either 'yes' or 'no' printed on it. The central input is just a 'go' signal which sets the experiment off at time t_1. Shortly after that the central output says 'yes' or 'no'. We are only interested in the 'yes's, which confirm that everything has got off to a good start (e.g., there are no 'particles' going in the wrong directions, and so on). At time $t_1 + T$ the other outputs appear, each with 'yes' or 'no' (depending for example on whether or not a signal has appeared on the 'up' side of a detecting screen behind a local Stern–Gerlach magnet). The apparatus then rests and recovers internally in preparation for a subsequent repetition of the experiment. But just before time $t_1 + T$, say at time $t_1 + T - \delta$, signals a and b are injected at the two ends. (They might for example dictate that Stern–Gerlach magnets be rotated by angles a and b away from some standard position). We can arrange that $c\delta \ll L$, where c is the velocity of light and L the length of the box; we would not then expect the signal at one end to have any influence on the output at the other, for lack of time, whatever hidden connections there might be between the two ends.

Sufficiently many repetitions of the experiment will allow tests of hypotheses about the joint conditional probability distribution
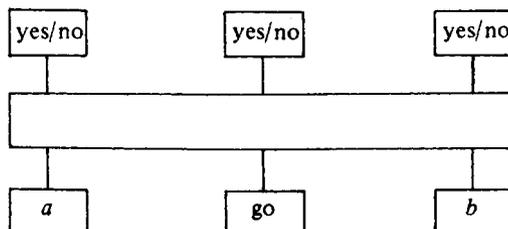
$$P(A, B | a, b)$$

for results A and B at the two ends for given signals a and b.

Now of course it would be no surprise to find that the two results A and B are correlated, i.e., that P does not split into a product of independent factors:

$$P(A, B | a, b) \neq P_1(A | a) P_2(B | b)$$

But we will argue that certain particular correlations, realizable according

Fig. 7. General EPR set-up, with three inputs below and three outputs above.

to quantum mechanics, are *locally inexplicable*. They cannot be explained, that is to say, without action at a distance.

To explain the 'inexplicable' we explain 'explicable'. For example the statistics of heart attacks in Lille and Lyons show strong correlations. The probability of M cases in Lyons and N in Lille, on a randomly chosen day, does not separate:

$$P(M, N) \neq P_1(M)P_2(N)$$

In fact when M is above average N also tends to be above average. You might shrug your shoulders and say 'coincidences happen all the time', or 'that's life'. Such an attitude is indeed sometimes advocated by otherwise serious people in the context of quantum philosophy. But outside that peculiar context, such an attitude would be dismissed as unscientific. The scientific attitude is that correlations cry out for explanation. And of course in the given example explanations are soon found. The weather is much the same in the two towns, and hot days are bad for heart attacks. The day of the week is exactly the same in the two towns, and Sundays are especially bad because of family quarrels and too much to eat. And so on. It seems reasonable to expect that if sufficiently many such causal factors can be identified and held fixed, the *residual* fluctuations will be independent, i.e.,

$$P(M, N | a, b, \lambda) = P_1(M | a, \lambda)P_2(N | b, \lambda) \qquad (10)$$

where a and b are temperatures in Lyons and Lille respectively, λ denotes any number of other variables that might be relevant, and $P(M, N | a, b, \lambda)$ is the conditional probability of M cases in Lyons and N in Lille for *given* (a, b, λ). Note well that we already incorporate in (10) a hypothesis of 'local causality' or 'no action at a distance'. For we do not allow the first factor to depend on b, nor the second on a. That is, we do not admit the temperature in Lyons as a causal influence in Lille, and vice versa.

Let us suppose then that the correlations between A and B in the EPR experiment are likewise 'locally explicable'. That is to say we suppose that there are variables λ, which, if only we knew them, would allow decoupling of the fluctuations:

$$P(A, B | a, b, \lambda) = P_1(A | a, \lambda)P_2(B | b, \lambda) \qquad (11)$$

We have to consider then some probability distribution $f(\lambda)$ over these complementary variables, and it is for the averaged probability

$$P(A, B | a, b) = \int d\lambda f(\lambda)P(A, B | a, b, \lambda) \qquad (12)$$

that we have quantum mechanical predictions.

But not just any function $p(A, B | a, b)$ can be represented in the form (12). To see this it is useful to introduce the combination

$$E(a, b) = \begin{pmatrix} P(\text{yes}, \text{yes} | a, b) + P(\text{no}, \text{no} | a, b) \\ - P(\text{yes}, \text{no} | a, b) - P(\text{no}, \text{yes} | a, b) \end{pmatrix} \tag{13}$$

Then it is easy to show (Appendix 1) that if (12) holds, with however many variables λ and whatever distribution $\rho(\lambda)$, then follows the Clauser–Holt–Horne–Shimony[18] inequality

$$|E(a, b) + E(a, b') + E(a', b) - E(a', b')| \leqslant 2 \tag{14}$$

According to quantum mechanics, however, for example with some practical approximation to the EPRB gedanken set-up, we can have approximately (from (4))

$$E(a, b) = \left(\sin \frac{a - b}{2} \right)^2 - \left(\cos \frac{a - b}{2} \right)^2 = - \cos (a - b) \tag{15}$$

Taking for example

$$a = 0°, \quad a' = 90°, \quad b = 45°, \quad b' = -45° \tag{16}$$

We have from (15)

$$E(a, b) + E(a, b') + E(a', b) - E(a', b')$$
$$= -3 \cos 45° + \cos 135° = -2\sqrt{2} \tag{17}$$

This is in contradiction with (14). Note that for such a contradiction it is not necessary to realize (15) accurately. A sufficiently close approximation is enough, for between (14) and (17) there is a factor of $\sqrt{2}$.

So the quantum correlations are locally inexplicable. To avoid the inequality we could allow P_1 in (11) to depend on b or P_2 to depend on a. That is to say we could admit the input at one end as a causal influence at the other end. For the set-up described this would be not only a mysterious long range influence – a non-locality or action at a distance in the loose sense – but one propagating faster than light (because $c\delta \ll L$) – a non-locality in the stricter and more indigestible sense.

It is notable that in this argument nothing is said about the locality, or even localizability, of the variable λ. These variables could well include, for example, quantum mechanical state vectors, which have no particular localization in ordinary space-time. It is assumed only that the outputs A and B, and the particular inputs a and b, are well localized.

5 Envoi

By way of conclusion I will comment on four possible positions that might be taken on this business – without pretending that they are the only possibilities.

First, and those of us who are inspired by Einstein would like this best, quantum mechanics may be *wrong* in sufficiently critical situations. Perhaps Nature is not so queer as quantum mechanics. But the experimental situation is not very encouraging from this point of view[19]. It is true that practical experiments fall far short of the ideal, because of counter inefficiencies, or analyzer inefficiencies, or geometrical imperfections, and so on. It is only with added assumptions, or conventional allowance for inefficiencies and extrapolation from the real to the ideal, that one can say the inequality is violated. Although there is an escape route there, it is hard for me to believe that quantum mechanics works so nicely for inefficient practical set-ups and is yet going to fail badly when sufficient refinements are made. Of more importance, in my opinion, is the complete absence of the vital *time* factor in existing experiments. The analyzers are not rotated during the flight of the particles. Even if one is obliged to admit some long range influence, it need not travel faster than light – and so would be much less indigestible. For me, then, it is of capital importance that Aspect[19,20] is engaged in an experiment in which the time factor is introduced.

Secondly, it may be that it is not permissible to regard the experimental settings a and b in the analyzers as independent variables, as we did[21]. We supposed them in particular to be independent of the supplementary variables λ, in that a and b could be changed without changing the probability distribution $\rho(\lambda)$. Now even if we have arranged that a and b are generated by apparently random radioactive devices, housed in separate boxes and thickly shielded, or by Swiss national lottery machines, or by elaborate computer programmes, or by apparently free willed experimental physicists, or by some combination of all of these, we cannot be *sure* that a and b are not significantly influenced by the same factors λ that influence A and B[21]. But this way of arranging quantum mechanical correlations would be even more mind boggling than one in which causal chains go faster than light. Apparently separate parts of the world would be deeply and conspiratorially entangled, and our apparent free will would be entangled with them.

Thirdly, it may be that we have to admit that causal influences *do* go faster than light. The role of Lorentz invariance in the completed theory would then be very problematic. An 'aether' would be the cheapest

solution[22]. But the unobservability of this aether would be disturbing. So would the impossibility of 'messages' faster than light, which follows from ordinary relativistic quantum mechanics in so far as it is unambiguous and adequate for procedures we can actually perform. The exact elucidation of concepts like 'message' and 'we', would be a formidable challenge.

Fourthly and finally, it may be that Bohr's intuition was right – in that there is no reality below some 'classical' 'macroscopic' level. Then fundamental physical theory would remain fundamentally vague, until concepts like 'macroscopic' could be made sharper than they are today.

Appendix 1 – The position of Bohr

While imagining that I understand the position of Einstein[23,24], as regards the EPR correlations, I have very little understanding of the position of his principal opponent, Bohr. Yet most contemporary theorists have the impression that Bohr got the better of Einstein in the argument and are under the impression that they themselves share Bohr's views. As an indication of those views I quote a passage[25] from his reply to Einstein, Podolsky and Rosen. It is a passage which Bohr himself seems to have regarded as definitive, quoting it himself when summing up much later[26]. Einstein, Podolsky and Rosen had assumed that '... if, without in any way disturbing a system, we can predict with certainty the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity'. Bohr replied: '... the wording of the above mentioned criterion... contains an ambiguity as regards the meaning of the expression "without in any way disturbing a system". Of course there is in a case like that just considered no question of a mechanical disturbance of the system under investigation during the last critical stage of the measuring procedure. But even at this stage there is essentially the question of *an influence on the very conditions which define the possible types of predictions regarding the future behaviour of the system*... their argumentation does not justify their conclusion that quantum mechanical description is essentially incomplete... This description may be characterized as a rational utilization of all possibilities of unambiguous interpretation of measurements, compatible with the finite and uncontrollable interaction between the objects and the measuring instruments in the field of quantum theory'.

Indeed I have very little idea what this means. I do not understand in what sense the word 'mechanical' is used, in characterizing the disturbances which Bohr does not contemplate, as distinct from those which he does. I do not know what the italicized passage means – 'an influence on the very

conditions...'. Could it mean just that different experiments on the first system give different kinds of information about the second? But this was just one of the main points of EPR, who observed that one could learn *either* the position *or* the momentum of the second system. And then I do not understand the final reference to 'uncontrollable interactions between measuring instruments and objects', it seems just to ignore the essential point of EPR that in the absence of action at a distance, only the first system could be supposed disturbed by the first measurement and yet definite predictions become possible for the second system. Is Bohr just rejecting the premise – 'no action at a distance' – rather than refuting the argument?

Appendix 2 – Clauser–Holt–Horne–Shimony inequality

From (13) and (11)

$$E(a, b) = \int d\lambda f(\lambda)\{P_1(\text{yes}|a, \lambda) - P_1(\text{no}|a, \lambda)\}, \{P_2(\text{yes}|b, \lambda) - P_2(\text{no}|b, \lambda)\}$$

$$= \int d\lambda f(\lambda)\bar{A}(a, \lambda)\bar{B}(b, \lambda) \tag{18}$$

where $\bar{A}$ and $\bar{B}$ stand for the first and second curly brackets. Note that since the *P*s are probabilities,

$$0 \leqslant P_1 \leqslant 1, \quad 0 \leqslant P_2 \leqslant 1$$

and it follows that

$$|\bar{A}(a, \lambda)| \leqslant 1, \quad |\bar{B}(b, \lambda)| \leqslant 1 \tag{19}$$

From (18)

$$E(a, b) \pm E(a, b') \leqslant \int d\lambda f(\lambda)\bar{A}(a, \lambda)[\bar{B}(b, \lambda) \pm \bar{B}(b', \lambda)]$$

so from (19)

$$|E(a, b) \pm E(a, b')| \leqslant \int d\lambda f(\lambda)|\bar{B}(b, \lambda) \pm \bar{B}(b', \lambda)|$$

likewise

$$|E(a', b) \mp E(a', b')| \leqslant \int d\lambda f(\lambda)|\bar{B}(b, \lambda) \mp \bar{B}(b', \lambda)|$$

Using again (19),

$$|\bar{B}(b, \lambda) \pm \bar{B}(b', \lambda)| + |\bar{B}(b, \lambda) \mp \bar{B}(b', \lambda)| \leqslant 2$$

and then from

$$\int d\lambda f(\lambda) = 1$$

follows

$$|E(a,b) \pm E(a,b')| + |E(a',b) \mp E(a',b')| \leqslant 2 \qquad (20)$$

which includes (14).

Notes and references

1 A. Einstein, B. Podolsky and N. Rosen, *Phys. Rev.* **46**, 777, (1935). For an introduction see the accompanying paper of F. Laloë.

2 D. Bohm, *Quantum Theory.* Englewood Cliffe, New Jersey (1951).

3 Note, however, that these *particular* phenomena were actually inferred from other quantum phenomena in advance of observation.

4 And perhaps romanticism. See P. Forman, Weimar culture, causality and quantum theory, 1918–1927, in *Historical Studies in the Physical Sciences,* vol. 3, 1–115. R. McCormach, ed. University of Pennsylvania Press, Philadelphia (1971).

5 M. Jammer, *The Philosophy of Quantum Mechanics,* John Wiley (1974), p. 204, quoting A. Petersen, *Bulletin of the Atomic Scientist* **19**, 12 (1963).

6 M. Jammer, *ibid,* p. 205, quoting W. Heisenberg, *Physics and Philosophy,* p. 160. Allen and Unwin, London (1958).

7 M. Jammer, *ibid,* p. 161, quoting E. Zilsel, P. Jordans Versuch, den Vitalismus quanten mechanisch zu retten, *Erkenntnis* **5**, (1935) 56–64.

8 The phrase is from a 1947 letter of Einstein to Born, Ref. 11, p. 158.

9 The accompanying paper of F. Laloë gives an introduction to quantum formalism.

10 And his followers. My own first paper on this subject (*Physics* **1**, 195 (1965).) starts with a summary of the EPR argument *from locality to* deterministic hidden variables. But the commentators have almost universally reported that it begins with deterministic hidden variables.

11 M. Born (editor), *The Born–Einstein-Letters,* p. 221. (Macmillan, London (1971).

12 M. Born, *ibid,* p. 176.

13 A. Einstein, *Dialectica,* 320, (1948). Included in a letter to Born, Ref. 11. p. 168.

14 B. d'Espagnat, *Scientific American,* p. 158. November 1979.

15 B. d'Espagnat, *A la Recherche du Réel.* Gauthier-Villars, Paris (1979).

16 'The number of young women is less than or equal to the number of woman smokers plus the number of young non-smokers.' (Ref. 15, p. 27). See also E. P. Wigner, *Am. J. Phys.* **38**, 1005 (1970).

17 Other discussions with some pretension to generality are: J. F. Clauser and M. A. Horne, *Phys. Rev.* **10D** (1974) 526; J. S. Bell, CERN preprint TH-2053 (1975), reproduced in *Epistemological Letters* (Association Ferd. Gonseth, CP 1081, CH-2051, Bienne) 9 (1976) 11; H. P. Stapp, *Foundations of Physics* **9** (1979) 1. Many other references are given in the reviews of Clauser and Shimony, and Pipkin in Ref. 19.

18 J. F. Clauser, R. A. Holt, M. A. Horne and A. Shimony, *Phys. Rev. Lett.* **23**, 880 (1969).

19 The experimental situation is surveyed in the accompanying paper of A. Aspect. See also: J. F. Clauser and A. Shimony, *Rep. Prog. Phys.* **41**, 1881 (1978); F. M. Pipkin, *Ann. Rev. Nuc. Sci.* (1978).

20 A. Aspect, *Phys. Rev.* **14D**, 1944 (1976).

21 For some explicit discussion of this, see contributions of Shimony, Horne, Clauser and Bell in *Epistemological Letters* (Association Ferdinand Gonseth, CP 1081, CH-2051, Bienne) **13**, p. 1 (1976); **15**, p. 79, (1977) and **18**, p. 1 (1978). See also Clauser and Shimony in Ref. 19.

22 P. H. Eberhard, *Nuovo Cimento* **46B**, 392 (1978).

23 But Max Jammer thinks that I misrepresent Einstein (Ref. 5, p. 254). I have defended my views in Ref. 24.

24 J. S. Bell, in *Frontier Problems in High Energy Physics, in honour of Gilberto Bernardini*. Scuola Normale, Pisa (1976).

25 N. Bohr, *Phys. Rev.* **48**, 696 (1935).

26 N. Bohr, in *Albert Einstein, Philosopher–Scientist*. P. A. Schilpp, Ed., Tudor, N.Y., (1949).