Reprinted from *Proceedings of the Symposium on Frontier Problems in High Energy Physics*, Pisa, June 1976

Einstein-Podolsky-Rosen Experiments

J. S. Bell
CERN, Geneva

I have been invited to speak on "foundations of quantum mechanics" — and to a captive audience of high energy physicists! How can I hope to hold the attention of such serious people with philosophy? I will try to do so by concentrating on an area where some courageous experimenters have recently been putting philosophy to experimental test.

The area in question is that of Einstein, Podolsky, and Rosen.[1] Suppose for example,[2,3] that protons of a few MeV energy are incident on a hydrogen target. Occasionally one will scatter, causing a target proton to recoil. Suppose (Fig. 1) that we have counter telescopes T_1 and T_2 which register when suitable protons are going towards distant counters C_1 and C_2. With ideal arrangements registering of both T_1 and T_2 will then imply registering of both C_1 and C_2 after appropriate time decays. Suppose next that C_1 and C_2 are preceded by filters that pass only particles of given polarization, say those with spin projection $+\frac{1}{2}$ along the z axis. Then one or both of C_1 and C_2 may fail to register. Indeed for protons of suitable energy one and only one of these counters will register on almost every suitable occasion — i.e., those occasions certified as suitable by telescopes[4] T_1 and T_2. This is because proton–proton scattering at large angle and low energy, say a few MeV, goes mainly in S wave. But the antisymmetry of the final wave function then requires the antisymmetric singlet spin state. In this state, when one spin is found "up" the other is found "down". This follows formally from the quantum expectation value

$$\langle \text{singlet}|\sigma_z(1)\sigma_z(2)|\text{singlet}\rangle = -1$$

where $\frac{1}{2}\sigma_z(1)$ and $\frac{1}{2}\sigma_z(2)$ are the z component spin operators for the two particles.

Suppose now the source-counter distances are such that the proton going towards C_1 arrives there before the other proton arrives at C_2. Someone looking at counter C_1 will not know in advance whether it will or will not register. But once he has noted what happens to C_1 at the appropriate time, he immediately knows what will happen subsequently to C_2, however far away C_2 may be.

Some people find this situation[5] paradoxical. They may, for example, have come to think of quantum mechanics as fundamentally indeterministic. In particular they may have come to think of the result of a spin measurement on an unpolarized

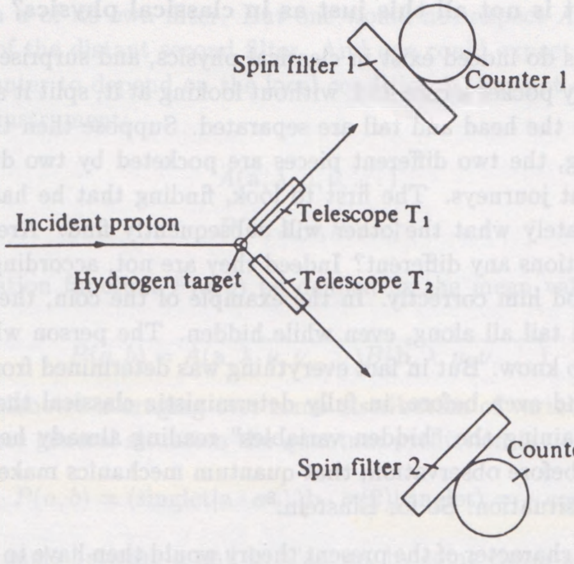

Fig. 1. Proton–proton scattering gedanken experiment.

particle (and each particle, considered separately, *is* unpolarized here) as utterly indefinite until it has happened. And yet here is a situation where the result of such a measurement is perfectly definitely known in advance. Did it only become determined at the instant when the distant particle passed the distant filter? But how could what happens a long way off change the situation here? Is it not more reasonable to assume that the result was somehow predetermined all along?

I will discuss briefly three ways of responding to this situation, which may be respectively characterized by the following three questions:

Why worry?

But is not all this just like classical physics?

But is it really true?

Why worry?

It can be argued that in trying to see behind the formal predictions of quantum theory we are just making trouble for ourselves. Was not precisely this the lesson that had to be learned before quantum mechanics could be constructed, that it is futile to try to see behind the observed phenomena? Moreover we learn again from this particular example that we must consider the experimental arrangement as a whole. We must not try to analyze it into separate pieces, with separately localized quotas of indeterminacy. By resisting the impulse to analyze and localize, mental discomfort can be avoided.

This is, as far as I understand it, the orthodox view, as formulated by Bohr[6] in his reply to Einstein, Podolsky, and Rosen. Many people are quite content with it.

But is not all this just as in classical physics?

Similar correlations do indeed exist in classical physics, and surprise nobody. Suppose I take from my pocket a coin and, without looking at it, split it somehow down the middle so that the head and tail are separated. Suppose then that, still without anyone looking, the two different pieces are pocketed by two different people who go on different journeys. The first to look, finding that he has head or tail, will know immediately what the other will subsequently find. Are the quantum mechanical correlations any different? Indeed they are not, according to Einstein,[7] if I have understood him correctly. In the example of the coin, the head and the tail were head and tail all along, even while hidden. The person who first looked was just the first to know. But in fact everything was determined from the handing over the pieces (and even before, in fully deterministic classical theory). It is by not explicitly containing the "hidden variables" reading already head or tail, (or "up" or "down"), before observation, that quantum mechanics makes a mystery of a perfectly simple situation. So for Einstein:[8]

> The statistical character of the present theory would then have to be a necessary consequence of the incompleteness of the description of the systems in quantum mechanics, and there would no longer exist any ground for the supposition that a future ... physics must be based upon statistics ...

That the apparent indeterminism of quantum phenomena can be simulated deterministically is well known to every experimenter. It is now quite usual, in designing an experiment, to construct a Monte Carlo computer programme to simulate the expected behaviour. The running of the digital computer is quite deterministic — even the so-called "random" numbers are determined in advance. Every such programme is effectively an *ad hoc* deterministic theory, for a particular set-up, giving the same statistical predictions as quantum mechanics.

It is interesting to follow this up a little in the above case of counter correlations. Let A be a variable which takes the values ± 1 according to whether counter 1 does or does not register. Let $B = \pm 1$ be a similar variable describing the response of counter 2. Let A and B be determined by variables $\lambda, \mu, \nu, \ldots$, some of which may be random numbers:

$$A(\lambda, \mu, \nu, \ldots)$$

$$B(\lambda, \mu, \nu, \ldots)$$

There are infinitely many ways of choosing such variables and such functions so that $B = -1$ whenever $A = +1$, and vice versa. The quantum mechanical correlations are then reproduced.

Consider, however, a variation on the experiment. Instead of having both filters pass spins pointing in the z direction, let the two filters be rotated, to pass spins pointing in some other directions. Let the filter associated with the first counter pass spins pointing along some unit vector $\mathbf{a}$, and that associated with the second counter pass spins pointing along some unit vector $\mathbf{b}$. For given values of the hidden variables λ, μ, $\nu, \ldots$ the response A of the first counter may well depend now on the orientation **a** of its own filter. But one would not expect A to depend on the orientation **b** of the distant second filter. And one could expect the response B of the second counter to depend on the local condition **b**, but not on the condition **a** of the remote instrument:

$$A(\mathbf{a}, \lambda, \mu, \nu, \dots)$$
$$B(\mathbf{b}, \lambda, \mu, \nu, \dots)$$

Let the correlation function $P(a, b)$ be defined as the mean value of the product AB:

$$P(a,b) = \overline{A(\mathbf{a}, \lambda, \mu, \nu, \dots) B(\mathbf{b}, \lambda, \mu, \nu, \dots)} \qquad (1)$$

where the bar denotes averaging over some distribution of variables $\lambda, \mu, \nu, \dots$

For this more general situation the quantum prediction is

$$P(a,b) = \langle \text{singlet} | \mathbf{a} \cdot \sigma(1) \mathbf{b} \cdot \sigma(2) | \text{singlet} \rangle = -\cos\theta \qquad (2)$$

where θ is the angle between **a** and **b**. Can we, by some clever scheme of variables $\lambda, \mu, \nu, \dots$ and functions A, B, arrange that the average (1) has the value (2)? The answer is "no".

Suppose, for example, we arrange that (1) equals (2) for $\mathbf{a} = \mathbf{b}$, i.e., $\theta = 0$:

$$P(a,b) = -1 \quad \text{for} \quad \mathbf{a} = \mathbf{b}$$

Then A and B must have opposite signs every where in the $\lambda, \mu, \nu, \dots$ space. Consider now what happens when **a** is varied to some new value **a′**. B (which is independent of **a** by hypothesis) does not change for given $\lambda, \mu, \nu, \dots$ But A will change sign at certain points, and these points will contribute $AB = +1$ instead of $AB = -1$ in the average (1). So

$$P(a', a) - P(a, a) = 2\rho$$

where ρ is the total probability of the set of points $\lambda, \mu, \nu, \dots$ at which A changes sign. Now this set of points, at which A changes sign when **a** is varied to **a′**, in no way depends on **b**. It follows from (1), and from $B = \pm 1$, that

$$|P(a', b) - P(a, b)| \leqslant 2\rho$$

So of all values **b**, $\mathbf{b} = \mathbf{a}$ is that for which P varies most rapidly with **a**. Unlike the quantum correlation (2), which is stationary in θ at $\theta = 0$, at the hidden variable correlation (1) must have a *kink* there (Fig. 2).

One could, of course, get the quantum mechanical result from a more general hidden variable representation in which A depends on **b** as well as **a**, or B on **a** as well as **b**:

$$A(\mathbf{a}, \mathbf{b}, \lambda, \mu, \nu, \dots)$$
$$B(\mathbf{a}, \mathbf{b}, \lambda, \mu, \nu, \dots)$$

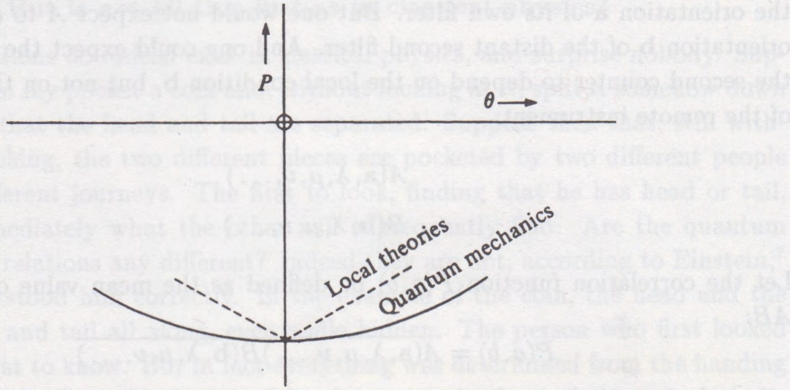

Fig. 2. Behaviour of correlation P near $\theta = 0$, $P = -1$.

But this would make the behaviour of a counter dependent on what is done at a distant place. This would seen strange enough with **a** and **b** constant, but suppose now that these settings vary with time. Then according to quantum mechanics the relevant values of **a** and **b** are those obtained when the particles pass through the corresponding filters. Suppose for example we arrange that the two passages are simultaneous. Then A (or B) would have to depend *instantaneously* on the setting **b** (or **a**) of the distant instrument. The causal dependence would have to propagate faster than light.

So all this is not at all just like classical physics. Einstein argued that the EPR correlations could be made intelligible only by completing the quantum mechanical account in a classical way. But detailed analysis shows that any classical account of these correlations has to contain just such a "spooky action at a distance"[9] as Einstein could not believe in:

> But on one supposition we should, in my opinion, absolutely hold fast: the real factual situation of the system S_2 is independent of what is done with the system S_1 which is spatially separated from the former.[10]

If nature follows quantum mechanics in these correlations, then Einstein's conception of the world is untenable.

But is it really true?

Well, *does* nature follow quantum mechanics in these matters? It might be argued that the very general and very remarkable success of quantum mechanics makes it pointless to do special experiments on these correlations. We will just find, after a lot of trouble, that quantum mechanics is again right. But it can also be argued that the great success of quantum mechanics, in so far as it differs from classical mechanics, is on the microscopic scale. Here, on the other hand, we are concerned with specifically quantum phenomena on the macroscopic scale.

The present movement to check these things experimentally started with the key paper of Clauser, Holt, Horne, and Shimony.[11] From the basic representation (1) they showed that

$$|P(a,b) - P(a,b')| + |P(a',b) + P(a',b')| \leqslant 2 \qquad (3)$$

Here P is the counting correlation already defined, a and a' are alternative settings of the first polarizer, and b and b' alternative settings of the second. It is readily seen that the quantum mechanical P, (2), for well chosen a, a', b, b', violates (3) by a factor as large as $\sqrt{2}$. It is in terms of this very practical "locality inequality" that the various experiments have been interpreted.

Unfortunately it is not at present possible to approach the conditions of the ideal critical experiment. Real counters, real polarization analyzers, and real geometrical arrangements, are together so inefficient that the quantum mechanical correlations are greatly diluted. The counters seldom say "yes, yes", usually say "no, no", and say "yes no" with a frequency only weakly dependent on the polarizer settings. In these conditions

$$P(a,b) = 1 - (\delta(a,b))^2$$

where δ is small and weakly dependent on the arguments a, b. The inequality (3) is then trivially satisfied. So it is only by allowing (in effect) for various inefficiencies in conventional ways, and so *extrapolating* from the real results to hypothetical ideal results, that the various experiments can be said to "test" the inequality. But the results are nevertheless of great interest. Compensating failures could be imagined, of the conventional quantum mechanics of spin correlations and of the conventional phenomenology of the instruments, which would make the practical experiments irrelevant. But that would seem an extraordinary conspiracy.

Of these experiments only one is concerned with the low energy *pp* scattering of the above gedanken experiment. It is that of Lamehi-Rachti and Mittig at Saclay.[12] Protons of 14 MeV lab energy are scattered at a lab angle of 45°, and spin correlation of scattered and recoil protons measured. They do not have the ideal yes–no polarization filters of the gedanken experiment. Instead they analyze polarization by secondary scattering on Carbon. Nor do they have the telescopes T_1 and T_2 to tell when there are indeed suitable particles going towards the counters. This also lengthens the extrapolation from real to ideal experiment. Nevertheless if there were some tendency for the singlet spin state to dissipate somehow with macroscopic separation of the particles, it should show up, barring conspiracy, in such an experiment. The preliminary results show no such effect. They agree with quantum mechanics and disagree (in the sense of a certain extrapolation) with the locality inequality.

All the other experiments have been done with pairs of photons rather than spin half particles. In the theory the two linear polarization states of each photon replace the two spin states of each spin $\frac{1}{2}$ particle. Suitably correlated photon pairs arise in the annihilation of slow positrons with electrons. Again there are no very efficient polarization filters. The experimenters have to resort to Compton scattering of the photons; according to quantum mechanics the polarization correlations are then translated into angular correlations. Such experiments have been done at Columbia[13] (Kasday, Ullman, and Wu) and at Catania[14] (Faraci, Gutkowski, Notarrigo, and Pennisi). The Columbia result is in agreement with quantum mechanics, and (in the extrapolated sense) in significant disagreement with the inequality. The reverse is the case for the Catania experiment. The reasons for this discrepancy between the two experiments are not known, as far as I can tell.

For optical photons, in contrast with the energetic photons of positron annihilation, efficient polarization filters *are* available — namely birefringent crystals and "piles-of-plates". Moreover suitably correlated photon pairs are produced in certain atomic cascades. Consider for example a two photon cascade in which initial and final atomic states have zero angular momentum. When the two photons come off back to back their helicities must be so correlated that there is no net angular momentum about their common direction of motion. There is a corresponding correlation of linear polarization states. Unfortunately the photons do not always come off back to back, for the residual atom can take up momentum. Very often then a "no" from a counter has no significance for polarization, but just means that no photon has gone that way. This problem could be eliminated in principle by suitable telescopes T to veto the uninteresting cases. But this has not been possible in practice. The significance of "no" from a counter is further diminished in these experiments by the very low efficiencies of the photon counters. So there is no question of actually realizing a system which violates the locality inequality. But such experiments do test whether the quantum polarization correlations persist over macroscopic distances. Experiments have been done by Clauser and Freedman,[15] on a cascade in Calcium, by Holt and Pipkin[16] and by Clauser[17] on a cascade in Mercury, and by Fry[18] on another cascade in Mercury. Three of these four experiments confirm quantum mechanics very nicely and (in the sense of some extrapolation) disagree significantly with the locality inequality. But for Holt and Pipkin the reverse is true. It is not understood why this experiment disagrees with the very similar one of Clauser.

Now these experiments do not test at all what was said to be the most striking feature of the quantum correlations. This was their dependence only on the instantaneous settings, during the passage of the particles, of the polarization filters. It is therefore of very great interest that an atomic cascade experiment is now under way in which *the settings of the polarizers are changed while the photons are in flight.* Clauser[19] suggested that this might be done by the use of something like Kerr cells. But according to Aspect[20] such cells heat up too quickly and are of too low transmission to be useful in practice. His idea is to replace each filter–counter combination by a pair of such combinations with differently oriented filters. He thinks that he can bring one or other orientation into play by a switching device that can rapidly redirect the incident photon from one filter to the other. He believes that such switching can be effected by the generation of ultrasonic standing waves on which the photon undergoes Bragg reflection. If this experiment gives the expected result it will be a confirmation of what is, to my mind, in the light of the locality analysis,[21] one of the most extraordinary predictions of quantum theory.

I think that future generations should be grateful to those who bring these matters out of the realm of gedanken experiment into that of real experiment. Moreover several of the real experiments are of great elegance. To hear of them (not in schematic terms from a theorist but in real terms from their authors) is, to borrow a phrase from Professor Gilberto Bernardini, a spiritual experience.

Appendix. Einstein and Hidden Variables

I had for long thought it quite conventional and uncontroversial to regard Einstein as a proponent of hidden variables, and indeed[22] as "the most profound advocate of hidden variables". And so I had on several occasions appealed to the authority of Einstein to legitimise an interest in this question. But in so doing I have been accused, by Max Jammer[5] in his very valuable book: *The Philosophy of Quantum Mechanics*, of misleading the public:

> One of the sources of erroneously listing Einstein among the proponents of hidden variables was probably J. S. Bell's widely read paper: On the Einstein–Podolsky–Rosen Paradox, *Physics* **1**, 195–200 (1964), which opened with the statement: "The paradox ... was advanced as an argument that quantum mechanics ... should be supplemented by additional variables." ... Einstein's remarks in his "Reply to Criticisms" (Ref. 4–9, p. 672), quoted by Bell in support of his thesis, are certainly no confession of the belief in the necessity of hidden variables.

The remark of Einstein which I had quoted was this:

> But on one supposition we should, in my opinion, absolutely hold fast: the real factual situation of the system S_2 is independent of what is done with the system S_1, which is spatially separated from the former.

The object of this quotation was to recall Einstein's deep commitment to realism and locality, the axioms of the EPR paper. And the quotation was not from p. 672 of Einstein's "Reply to Criticisms", but from p. 85 of his "Autobiographical Notes" in the same volume.[23] But turning to p. 672, I find the following:

> Assuming the success of efforts to accomplish a complete physical description, the statistical quantum theory would, within the framework of future physics, take an approximately analogous position to the statistical mechanics within the framework of classical mechanics. I am rather firmly convinced that the development of theoretical physics will be of this type; but the path will be lengthy and difficult.

This seems to me a rather clear commitment to what is usually meant by hidden variables.[24]

Other similarly clear statements are readily found:[25]

> I am, in fact, firmly convinced that the essentially statistical character of contemporary quantum theory is solely to be ascribed to the fact that this (theory) operates with an incomplete description of physical systems.

Moreover, the Einstein–Podolsky–Rosen paper *did* have the title: "Can Quantum Mechanical Description of Physical Reality be Considered Complete?" And it did end with:

> While we have thus shown that the wave function does not provide a complete description of the physical reality, we left open the question of whether or not such a description exists. We believe, however, that such a theory is possible.

It seems to me then beyond dispute that there was at least one Einstein, that of the EPR paper and the Schilpp volume, who was fully committed to the view that quantum mechanics was incomplete and should be completed — which is the hidden variable programme. Max Jammer seems not to have found this Einstein, but claims to have found another. As evidence he cites phrases from private letters, an oral tradition, and Einstein's well-known commitment to classical field theory.

Now the belief in classical field theory, in "Continuous functions in the four dimensional (continuum) as basic concepts of the theory[26]", in no way excludes belief in "hidden" variables. It can be seen rather as a particular conception of those variables.

The oral tradition was that Einstein expected quantum mechanics ultimately to come in conflict with experiment. But if such an expectation were to exclude him from the list of proponents of hidden variables, I doubt it anyone could be left on it. If such a list were compiled I think it would be of people concerned to reproduce the experimentally confirmed aspects of quantum mechanics but eager to find in their investigations some hint as to where a critical experiment might be sought. Indeed few would expect the ultimate vindication of quantum mechanics (on the statistical level) so strongly as Einstein himself on one occasion:[27] "The formal relations which are given in this theory — i.e., its entire mathematical formalism — will probably have to be contained, in the form of logical inferences, in every useful future theory".

The quotations from private letters are of negative reactions by Einstein to the very particular 1952 hidden variables of Bohm. This scheme reproduced completely, and rather trivially, the whole of nonrelativistic quantum mechanics. It had great value in illuminating certain features of the theory, and in putting in perspective various "proofs" of the impossibility of a hidden variable interpretation. But Bohm himself did not think of it as in any way final. Jammer could have added to his quotations the following, from a letter from Einstein to Born:[6]

> Have you noticed that Bohm believes (as de Broglie did, by the way, 25 years ago) that he is able to interpret the quantum theory in deterministic terms? That way seems too cheap to me.

On which Born comments:

> Although this theory was quite in line with his own ideas,. . .

So Born also had listed Einstein as a proponent of hidden variables. I think he was right.

Notes and References

1. A. Einstein, B. Podolsky and N. Rosen, *Phys. Rev.* **47**, 777 (1935).
2. D. Bohm, *Quantum Theory*, Englewood Cliffe, N.J. (1951).
3. A. Peres and P. Singer, *Nuovo Cimento* **15**, 907 (1960); R. Fox, *Lettere al Nuovo Cimento* **2**, 656 (1971).
4. It is assumed that these telescopes do not affect proton spin.
5. M. Jammer, *The Philosophy of Quantum Mechanics*, Wiley, N.Y. (1974). Chapters 6 and 7 give a comprehensive account of the history (and prehistory) of the EPR paradox.
6. N. Bohr, Discussions with Einstein, in Ref. 23.
7. Appendix.
8. A. Einstein, in Ref. 23, p. 87.
9. A. Einstein, in Ref. 28, p. 158.
10. A. Einstein, in Ref. 23, p. 85.
11. J. F. Clauser, R. A. Holt, M. A. Horne and A. Shimony, *Phys. Rev. Lett.* **23**, 880 (1969).
12. M. Lamehi-Rachti and W. Mittig, *Phys. Rev.* **D14**, 2543 (1976).
13. L. R. Kasday, J. D. Ullman and C. S. Wu, *Nuovo Cimento* **B25**, 633 (1975).
14. G. Faraci, D. Gutkowski, S. Notarrigo and A. R. Pennisi, *Lettere al Nuovo Cimento* **9**, 607 (1974).
15. J. F. Clauser and S. J. Freedman, *Phys. Rev. Lett.* **28**, 938 (1972).
16. F. M. Pipkin, *Adv. Atomic and Mol. Phys.* **14**, 281 (1978).
17. J. F. Clauser, *Phys. Rev. Lett.* **36**, 1223 (1976).
18. E. S. Fry and R. C. Thompson, *Phys. Rev. Lett.* **37**, 465 (1976).
19. As reported by A. Shimony, Ref. 22.
20. A. Aspect, *Phys. Lett.* **A54**, 117 (1975); *Phys. Rev.* **D14**, 1944 (1976).
21. For simplicity, in this paper we followed up the consequences of determinism, which is required by locality only in the case of ideal perfect correlations. But (3) holds in a much wider class of theories, local but indeterministic. See, for example, and references therein: J. F. Clauser and M. A. Horne, *Phys. Rev.* **D10**, 526 (1974); B. D'Espagnat, *Phys. Rev.* **D11**, 1424 (1975); and *Conceptual Foundations of Quantum Mechanics*, Benjamin, new edition (1976); J. S. Bell, *The Theory of Local Beables*, CERN, TH 2053 (1975), in GIFT (1975) Proceedings and Epistemological Letters March 1976.
22. A. Shimony, in *Foundations of Quantum Mechanics*, B. D'Espagnat, Ed. Academic Press, N.Y., London (1971), p. 192, quoted with disapproval by M. Jammer, Ref. 5.
23. P. A. Schilpp, Ed., *Albert Einstein, Philosopher-Scientist*, Tudor, N.Y. (1949).
24. The usual nomenclature, *hidden* variables, is most unfortunate. Pragmatically minded people can well ask *why bother about hidden entities that have no effect on anything?*. Of course, every time a scintillation occurs on screen, every time an observation yields one thing rather than another, the value of a *hidden* variable is revealed. Perhaps *uncontrolled* variable would have been better, for these variables, by hypothesis, for the time being, cannot be manipulated at will by us.
25. Ref. 23, p. 666. See also Einstein's introductory remarks in Louis de Broglie, *Physicien et Penseur*, Albin Michel, Paris (1953), p. 5, and letters 81, 84, 86, 88, 97, 99, 103, 106, 108, 110, 115 and 116, in Ref. 28.
26. Ref. 23, p. 675.
27. Ref. 23, p. 667.
28. M. Born, Ed., *The Born-Einstein Letters*, p. 192, Macmillan, London (1971).