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Quantum Mechanics at the Macroscopic Level

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Contents

1. The principle of complementarity

It is a great honor and privilege to have been invited to speak at this symposium celebrating the centenary of the birth of Niels Bohr. The topic of my talk is essentially the foundations and interpretation of quantum mechanics, a subject on which Bohr probably thought longer and harder than anyone else in history, and as far as possible I shall therefore try to motivate what I have to say by direct reference to his own ideas. Nevertheless. I hope to persuade you that the foundations of quantum mechanics is not, as is so often thought, a topic of interest only to historians and philosophers, where ancient and hallowed ground is continually trodden and retrodden, but rather that in the last fifteen years or so it has become an *experimental* subject in a way which in Bohr's day might well have been difficult to imagine.

Throughout his life, Niels Bohr maintained a consistent point of view on the interpretation of quantum mechanics, which can be summarized in four principal theses:

(1) Microscopic entities (such as electrons and atoms) are not even to be thought of as possessing properties in the absence of specification of the macroscopic experimental arrangement.

(2) Macroscopic experimental arrangements, and the results of experiments, are to be expressed in classical, realistic terms.

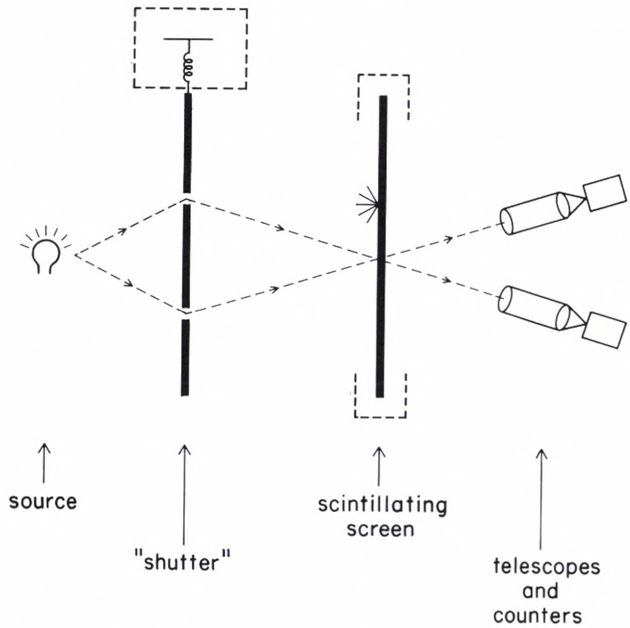


Fig. 1. The Young's slits experiment. Broken lines indicate parts of the apparatus that can be removed.

(3) There exists what Bohr repeatedly refers to as an "unanalyzable link" between the microsystem and the macroscopic measuring apparatus.

(4) The principle of complementarity: different experimental arrangements exclude one another, and the measurement of one property may therefore be "complementary" to the measurement of another. (Bohr regarded this principle as a universal feature of nature, and attempted to apply it not only to physics but to biology, psychology, etc.)

As an illustration of Bohr's point of view, let us consider the Young's slits (thought-) experiment so familiar from elementary quantum mechanics textbooks (see fig. 1). The apparatus consists of: (1) a source of electrons (or photons); (2) a "shutter", that is a screen containing two apertures which can be opened or closed at will, with the option of either bolting the shutter down or suspending it by a weak spring; (3) a removable scintillating screen; and (4) a pair of telescopes (or the equivalent device for electrons), one focussed on each of the two slits, and appropriate counters attached to each. It is evident that we cannot simultaneously arrange for the electrons (or photons) to enter the telescopes *and* for them to hit the scintillating screen. The basic "paradox" is, of course, that if we decide to employ the telescopes, i.e. (in the geometry of the figure) to remove the screen, then any one electron or photon will be registered in one counter and not in the other, i.e. it will appear to have definitely passed through one slit and not through the other; whereas, if we use the scintillating screen (so that the particles never reach the telescopes) then the accumulated statistical distribution of the particles on the screen will show diffraction effects which indicate *interference* between the two possible paths.

A number of features of this experiment are worth noting. First, it involves at least three different types of macroscopic apparatus, with quite different functions: the source, the shutter (which until further notice I shall regard as firmly bolted down) and the detection apparatus (scintillating screen *or* counters). Secondly, as pointed out by Wheeler (1978) it is, in principle at least, possible to delay the choice of which experimental arrangement to use (screen or telescope) for any single given electron until after that electron has certainly passed the shutter. Thirdly, it is possible to do a "negative-result" experiment, for example by placing a telescope and counter opposite one slit only; if the counter does *not* click, then one can infer that the microsystem passed through the other slit, without having caused (or so one would think!) any physical interaction between the microsystem and the detection apparatus.

Niels Bohr's interpretation of this situation is, of course, that the two different experimental arrangements are *complementary*: in any given experiment one is faced with the choice of measuring the path (i.e. which slit the particle passed through) or measuring the interference pattern. Contrary to what is often inferred from Heisenberg's discussion (1930) of his "γ-ray microscope" thought-experiment, there is in Bohr's view no question of an *interaction* between the system and the apparatus: the quantum-mechanical "state" of the system *is* nothing but a link between the microsystem and the macroapparatus (and hence between the various pieces of the latter), not an intrinsic property of the microsystem itself. As the philosopher Paul Feyerabend (1962) has put it, the conceptual mistake made by someone who attributed the dependence of the microsystem's behavior on the experimental apparatus used to an interaction with that apparatus would, in Bohr's view, be similar to the mistake of attributing the difference in the properties of a system observed in a moving frame of reference to an interaction with the frame. This point of view seems entirely self-consistent internally, and has been put on an even more complete logical foundation by the philosopher Hans Reichenbach (1944) in his book "The Philosophic Foundations of Quantum Mechanics". However, it is essential to remember that in the account given so far the notion of "measurement", and of "macroscopic measuring apparatus", has been taken for granted as logically unproblematical. I return to this point below.

2. *Verification of thought-experiments*

What has changed in this field since Niels Bohr's death? First, there have been dramatic verifications of some of the more counter-intuitive predictions of quantum mechanics at the atomic level—and hence, one might perhaps reasonably conclude, of the correctness of his interpretation at this level at least—in a number of experiments which in his days were merely "thought-experiments", but have now been actually carried out in the laboratory. Two classes of experiments seem to me particularly spectacular. The first is a set of experiments using the neutron interferometer, and in particular those reported by the Vienna group (Summhammer et al. 1983). The experimental setup is shown schematically in fig. 2a. The source is weak enough that only one neutron at most is normally present in the apparatus at

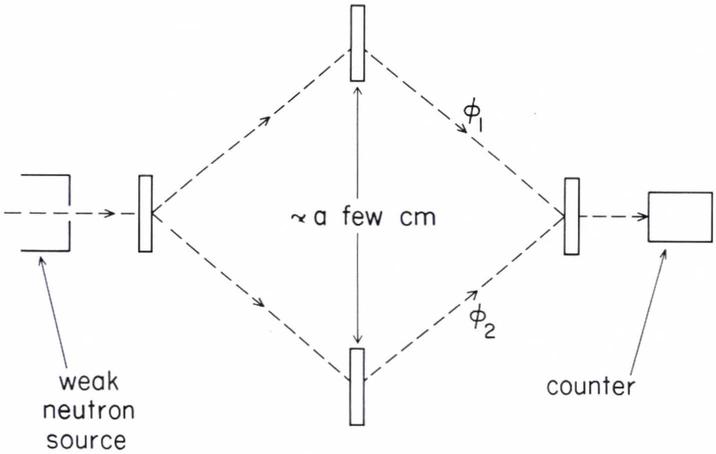


Fig. 2a. Schematic diagram of the neutron interferometer.

any given time. The de Broglie wave representing the neutron is split by the first silicon crystal (shown in the figure as a rectangle) into two packets which, as remarked by Greenberger (1982), are roughly of the size and shape of a small postage stamp. Each of these two wave packets then travels freely until it reaches one of the two intermediate silicon crystals, which are a distance of a few centimeters apart. Thus, at this point the separation of the two packets is much larger than their size and they are quite distinct. The intermediate crystals deflect the wave packets in such a way that their trajectories meet again at the final silicon crystal, where the total neutron flux is measured in a way not shown explicitly in the

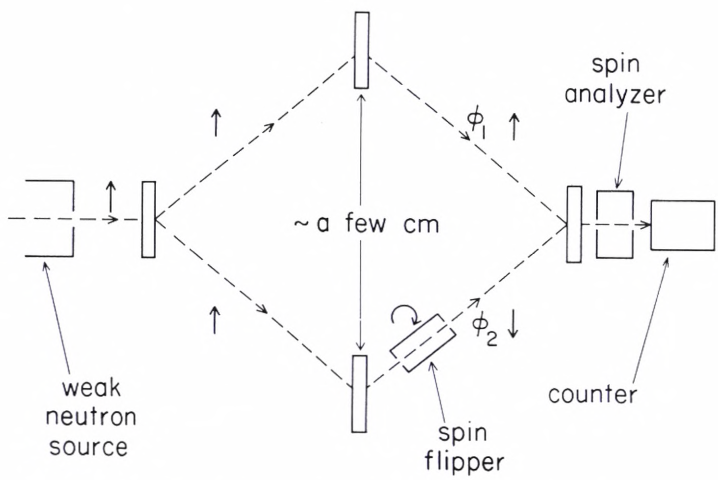


Fig. 2b. Spin-flip experiment in the neutron interferometer.

figure. * By adding suitable selection devices it is possible to measure not only the total flux, but also the flux of neutrons with spin polarization along a particular axis.

The simplest type of experiment one can carry out using this apparatus does not involve the neutron spin and is the exact analog of the classical Young's slits experiment. We simply block off beam 2 (by inserting a suitable absorber in its path) and measure the flux in beam 1, and vice versa. We then allow *both* beams to propagate freely to the detection apparatus and measure the total flux, which turns out not to be a simple sum of the measured fluxes in beams 1 and 2 separately, and, moreover, to show the quantum-mechanically expected diffraction pattern (see footnote). This already shows that the properties of the two beams combined are not the sum of those of the individual beams, even though there was only one neutron in the apparatus at any given time and the spatial separation of the two components of the wave packet was large compared to their individual extent.

The experiment just described is, of course, conceptually identical to some which had already been done with photons and even with electrons. However, by exploiting the fact that a neutron has a nonzero spin and magnetic moment one can do a good deal more. Suppose that, as shown in fig. 2b, we insert a spin analyzer in front of the detector, and moreover introduce in one beam, but not in the other, a device which rotates the neutron spin by π; for the moment, we assume that this device is a static magnetic guide which exchanges no energy with the neutron, so there is no question of any "measurement" having taken place at this point. Suppose now that the neutrons entering the apparatus are polarized along the positive z-axis. Using the notation $|\uparrow\rangle$ for such a spin state, and $|\downarrow\rangle$ for the orthogonal state of negative z-polarization, we expect that the wave function of the neutrons entering the detector will have the general form

$$\psi = \phi_1(r)\,|\uparrow\rangle + \phi_2(r)\,|\downarrow\rangle, \tag{1}$$

i.e. the neutrons "in" beam 1 will be unflipped and have spin up, whereas those "in" beam 2 will have had their spins flipped by the magnetic guide and therefore have spin down. If we now select for detection only those neutrons with spin up, this is equivalent to selecting only those in beam 1 and we should expect no diffraction pattern: similarly, if we select only those with spin down. This is confirmed experimentally. Moreover, if we measure the *total* flux (without spin selection) then we expect that the two beams, being associated with different spin states, will now add incoherently, so that again we expect no diffraction pattern: again, experiment confirms this prediction. However, suppose that we select only neutrons polarized along the x-axis (the corresponding spin state is denoted $|\rightarrow\rangle$ and the orthogonal state $|\leftarrow\rangle$). Using the fact that the state $|\rightarrow\rangle$ can be written

* In practice, rather than observing the flux as a function of position in the vertical plane, one observes the details of the interference between the two beams by inserting a wedge of material in the path of one beam, which shifts the relative phase of the two wave packets by a variable and controllable amount. Moreover, one actually observes the intensity transmitted in various directions, in general not forward as in the figure. These differences are of no importance in the present context and I shall ignore them in the ensuing discussion.

as a linear combination of $|\uparrow\rangle$ and $|\downarrow\rangle$, we see that the wave function (1) becomes on this basis:

$$\psi = 2^{-1/2}\{(\phi_1(r) + \phi_2(r))\,|\rightarrow\rangle + (\phi_1(r) - \phi_2(r))\,|\leftarrow\rangle\} \tag{2}$$

and we would expect the flux of selected neutrons to be proportional to $|\phi_1(r) + \phi_2(r)|^2$, that is, to show a diffraction pattern. Once more, this prediction is verified by the experimental results.

One might argue that this experiment too, is conceptually no different from one that can be carried out with photons (with the spin variable replaced by polarization). However, the final twist is the most amusing of all, and to the best of my knowledge has no (practicable) analog using either photons or electrons. Suppose we replace the static magnetic guide by a radiofrequency cavity, which is so tuned that a neutron entering with spin up will have its spin rotated by exactly π (as in the guide). With regard to the measurement of up-spin, down-spin or total neutron flux the situation is unchanged. The interesting question is: If we measure the flux of neutrons selected to be in the spin state $|\rightarrow\rangle$, do we or do we not expect, according to the standard formalism of quantum mechanics, to see a diffraction pattern? This question (or, more precisely, the significance of the answer for the interpretation of the formalism) has provoked a lively discussion in the recent literature [see e.g. Dewdney et al. (1984), Rauch (1983)]. At first sight, it is tempting to argue that the RF field in the cavity has transferred a finite amount of energy to the neutron, which could in principle be measured, and that we have therefore in effect measured at this point which of the paths the neutron followed (if the cavity is found to have lost energy, we know that the neutron has come via beam 2, whereas if it has not, the neutron is in beam 1). Now the standard quantum measurement axioms, at least as presented in most textbooks, tells us that measurement "collapses" the wave packet and that, if as here the result of the measurement is not inspected, the various amplitudes cannot interfere coherently (i.e. the state of the system is a mixture, not a superposition, of eigenstates of the measured quantity). This would seem to imply that, when the spin state $|\rightarrow\rangle$ is selected, the amplitudes $\phi_1(r)$ and $\phi_2(r)$ would add incoherently and we should get no diffraction pattern. Thus, prima facie we would conclude either that no diffraction pattern will occur or that, if it does indeed occur, we have refuted one of the standard measurement axioms.

I believe this argument is fallacious [see also the remarks of Rauch (1983)]. In the first place, the first conclusion is definitely incorrect: the diffraction pattern certainly is seen experimentally. * Secondly, the conclusion that the quantum measurement axioms are at fault is invalid, because by flipping the spin in the way described we have not in fact made a measurement or even provided the possibility of doing so. The crucial point is that the electromagnetic field in the RF field in a *coherent state*, and therefore its final state, after flipping the neutron spin, is virtually identical to its initial one. A correct application of quantum mechanics to the whole setup (cavity plus neutron) then predicts a diffraction pattern. That the

* Because the two interfering states have different energies, the interference is time-dependent, so that stroboscopic detection is required.

cavity has lost some energy is irrelevant, because the initial uncertainty in the energy was much larger than the amount lost, and there is therefore no way of inspecting the system to find out whether or not the loss has occurred. Note, however, that in principle (though perhaps not in practice) it would be possible to modify the experiment so that such inspection is possible. For example, if the radiation in the cavity is not in a coherent state but rather corresponds to a precise number of photons (hence a precise energy), there seems no objection in principle to determining whether or not the energy loss has occurred. In this situation, however, the final state of the cavity is orthogonal to its initial state, and application of the formalism of quantum mechanics to the combination of neutron plus cavity then predicts quite unambiguously that no diffraction pattern should be seen, whatever the spin of the neutrons selected for detection. While this particular experiment has not been done, it is conceptually similar to the experiment with a static guide in which the total neutron flux is measured: there, we could have (but did not choose to) measure the z-component of spin of the incident neutrons directly and thereby infer their path, and sure enough no interference occurred. Thus, this series of experiments, taken as a whole, provides rather spectacular evidence that we must indeed make a choice between observing the path of the neutron and observing the interference between the possible paths.

3. Aspect's experiment

The second class of experiments which is specially significant for the foundations of quantum mechanics is the series of photon polarization–correlation experiments which was initiated by Freedman and Clauser (1972) and culminated in the experiment by Aspect and co-workers (1982b). The experimental setup is shown schematically (for the Aspect experiment) in fig. 3. The source consists of a set of atoms which are pumped into an excited state from which they decay by a two-photon cascade process: the lifetime of the intermediate state is on the order of a nanosecond. According to quantum mechanics, the polarizations of the emitted photons will be correlated. In the experiment we are interested only in that subclass of processes in which the two photons are emitted approximately in the $+z$ and $-z$

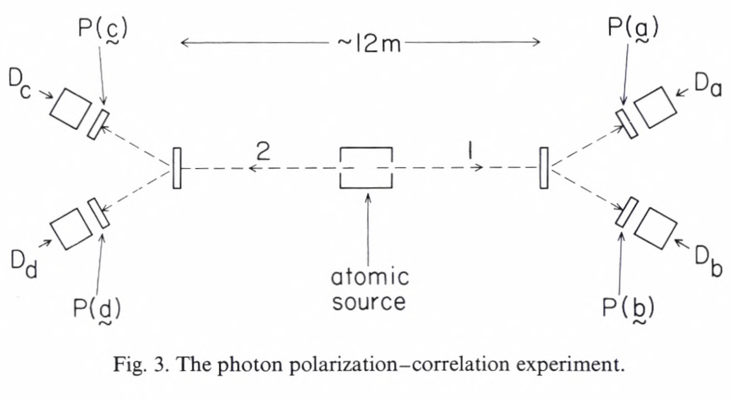


Fig. 3. The photon polarization–correlation experiment.

directions. Photon 1 (emitted in the $+z$ direction) travels a certain distance (a few meters) and enters the commutator C_1 (represented in the figure by a vertical rectangle), which can switch it into one of two distinct beams. If it is switched into the upper beam it encounters a polarizer $P_{\hat{a}}$ "set for polarization direction $\hat{\boldsymbol{a}}$", i.e. so oriented that the photon will pass if its polarization is along the direction $\hat{\boldsymbol{a}}$ (in the plane perpendicular to its wave vector) and will be rejected if the polarization is in the direction orthogonal to $\hat{\boldsymbol{a}}$ in this plane. If it passes, the photon will be detected in the photomultiplier detector $D_{\hat{a}}$: otherwise, the detector will not register. Were we being less self-conscious, we would of course describe the whole operation as a "measurement of the photon polarization along $\hat{\boldsymbol{a}}$", or more specifically a measurement of whether or not the photon is polarized along the $\hat{\boldsymbol{a}}$ direction. In an exactly similar way, if the photon is deflected into the lower beam, it enters an apparatus which will, in a similarly abbreviated description, measure the polarization along $\hat{\boldsymbol{b}}$. Exactly similar arrangements are made for photon 2, which is switched by commutator C_2 so that its polarization is measured along either the $\hat{\boldsymbol{c}}$ or the $\hat{\boldsymbol{d}}$ direction. Note that for a single given photon we can measure the polarization either along $\hat{\boldsymbol{a}}$ or along $\hat{\boldsymbol{b}}$, but not both (similarly for $\hat{\boldsymbol{c}}$ and $\hat{\boldsymbol{d}}$). In the Aspect experiment the commutators C_1 and C_2 are activated in what is (one hopes) a random manner, and the dimensions of the apparatus, and the lifetime of the intermediate state, are such that the events of photon 1 passing polarizer $P_{\hat{a}}$ or $P_{\hat{b}}$ and entering detector $D_{\hat{a}}$ or $D_{\hat{b}}$, and photon 2 doing the same with $P_{\hat{c}}$ or $P_{\hat{d}}$, etc., have spacelike separation (as do the events of passing the commutators). Thus, given the postulates of special relativity theory, there is no opportunity for transmission of information to photon 2 (or the associated apparatus) about which measurement we have made or intend to make on photon 1, nor vice versa.

To discuss this experiment it is convenient to introduce *operationally defined* quantities A, B, C, $D = \pm 1$ as follows: suppose that we "measure the polarization of photon 1 along direction $\hat{\boldsymbol{a}}$", i.e. photon 1 is switched into the upper beam. Then we assign a value $+1$ to A if the photomultiplier $D_{\hat{a}}$ clicks, and -1 if it does not *. If the photon is switched into the lower beam, so that it is the polarization along $\hat{\boldsymbol{b}}$ which is measured, then for the moment A is undefined. Similarly, if the photon indeed is switched into the lower beam, we define B to have the value $+1$ if the photomultiplier $D_{\hat{b}}$ clicks, otherwise -1; if, however, it is the polarization along $\hat{\boldsymbol{a}}$ which is measured, then B is undefined. Similar definitions are given, in the obvious way, for the variables C and D. *For any given pair of photons* one clearly measures, according to the above definitions, one and only one of the four pairs of quantities (A, C), (A, D), (B, C) and (B, D), thus one and only one of the four products AC, AD, BC and BD, which can each obviously take only the values ± 1.

To analyze the significance of this experiment, I follow a line of reasoning similar (but not identical) to that given by Stapp (1985) [see also d'Espagnat (1979)]. [The

* In the setup as described, there is clearly no way of knowing in practice, in any individual event (i.e. for any individual pair of emitted photons) that a photon has been rejected by the polarizer. This lacuna could in fact be filled by ancillary apparatus: cf. the earlier experiment of Aspect and coworkers (1982a). However, it is of little importance in the present context, since with perfect detector efficiencies a very slight generalization of the postulates of locality and induction allow the relevant statistics to be inferred from a measurement of detector counting rates *without* polarizer P_a in place.

original and classic paper on this subject is of course the famous paper of Bell (1964).] Let us make three assumptions which at first sight seem completely natural and obvious, namely the assumptions of *locality*, *counterfactual definiteness* and *induction*. For example, suppose that on a particular pair of photons we measured A and C, i.e. photon 1 was directed into $P_{\hat{a}}$ and photon 2 into $P_{\hat{c}}$. Then we assume that had we decided to measure A and D rather than A and C, that is, to direct photon 2 into $P_{\hat{d}}$ rather than $P_{\hat{c}}$, then we would still have obtained the result $A = +1$ (locality). Moreover, we assume that had we decided to measure B rather than A, and either C or D, then the result we *would have* got for B is definite, though of course unknown, (counterfactual definiteness) and moreover is independent of whether it was C or D which was simultaneously measured (locality). Similar assumptions are made for the quantities C and D. Note that the assumption of counterfactual definiteness is weaker than that of hidden variables or even of local objectivity at the microscopic level (it says nothing about any "properties" of the microscopic entities as such, only about the macroscopic events which we "would have" observed). Finally we assume that the statistical properties of the subset of events in which we actually measure a given pair of the quantities A, B, C, D are representative of the statistical properties of the whole ensemble of events. *

With the assumptions of counterfactual definiteness and locality, we can assign to any definite event (associated with emission of a particular pair of photons) definite values of all four of the quantities A, B, C, D. For example, suppose that for this event we measure A and C. Then the values of A and C are, trivially, the values we actually get, i.e. are definite and known, while B and D are now defined and have the values which we "would have" got, i.e. are definite but unknown. In the words of Einstein et al. (1935) we can regard all four of A, B, C, D as "elements of reality". Since by their definitions A, B, C and D can each take only the values ± 1, it is obvious (if necessary by exhaustion of the 16 possibilities!) that we have for each individual event the relation

$$AC + AD + BC - BD \leqslant 2, \tag{3}$$

and hence that for the ensemble of events as a whole we must have

$$\langle AC \rangle_{\text{ens}} + \langle AD \rangle_{\text{ens}} + \langle BC \rangle_{\text{ens}} - \langle BD \rangle_{\text{ens}} \leqslant 2, \tag{4}$$

which is the generalization by Clauser et al. (1969) of the celebrated inequality originally proved by Bell (1964). Finally, by the assumption of induction the expectation value of e.g. $\langle AC \rangle$ for the ensemble may be replaced by the average of the values obtained in that subset of events in which A and C were actually measured, i.e. by the "experimental" value. Thus we have a clear prediction, which follows from our three general assumptions above, about the results of the experimental measurements. The punch-line is, of course, that the quantum-mechanical predictions for the quantities $\langle AC \rangle$ etc., *violate* the inequality, and that the

* The rather clumsy language of this formulation is used to emphasize the point that the argument does *not* require the ascription of definite properties to the photons themselves.

experiments find agreement with quantum mechanics. Because of various technical problems associated with imperfectly efficient detectors etc. [see the experimental papers, and the review paper by Clauser and Shimony (1978)], a few subsidiary assumptions are necessary before we can claim that the experiments conclusively refute all theories having the three properties assumed above. Most workers in the area [though not all—cf. e.g. Marshall (1983)] would regard these subsidiary assumptions as so plausible that the loopholes left by them are of little interest, and if one takes this point of view one can say that any theory which makes simultaneously the assumptions of locality, counterfactual definiteness and induction is conclusively refuted by the experiments.

The class of theories so excluded certainly contains, as a subclass, not only all theories in which the microscopic entities (photons) possess properties which are independent of the experimental arrangement, but also all theories which ascribe to them properties which depend only on the *local* experimental arrangement: if we wish to talk about "properties" of the individual photons at all, then the experiment shows that these properties must be a function of the *global* experimental arrangement, including events (such as the switching of the "distant" photon into one beam or the other) which, within the assumptions of special relativity, could not have physically influenced the microsystem in question. It is therefore certainly extremely difficult, if not impossible, to interpret the outcome in terms of an *interaction* between the system and the apparatus—at least within the framework of very basic "common-sense" assumptions which most of us would be very loth to give up. Thus, the polarization-correlation experiments, and in particular the most recent experiment of Aspect and co-workers (1982b), may be regarded as a spectacular confirmation of the correctness of Bohr's point of view at the microscopic level. Whether or not these experiments tell us anything about the nature of reality at a *macroscopic* level is a question to which I shall return below.

4. Describing the measuring apparatus in plain language

Now let us turn to the main topic of this talk and explore the consequences of following Niels Bohr's views in a different direction. Bohr repeatedly stresses in his writings the distinction between the quantum system under investigation and the measuring apparatus, which must be described classically. Often, but not always (cf. below), he seems to imply that the distinction between "quantum" and "classical" coincides with the distinction between "microscopic" (or "atomic") and "macroscopic".

Bohr repeatedly stresses that

> "the description of the experimental arrangement and the recording of observations must be given in plain language, suitably refined by the usual physical terminology" [this particular quotation is from his essay on "Quantum Physics and Philosophy" (Bohr 1958)].

To the question "Why must it?" his answer is:

> "This is a simple logical demand, since by the word 'experiment' we can only mean a procedure regarding which we are able to communicate to others what we have done and what we have learned" [ibid.].

Now, when someone says that something "must" be done, he clearly believes that it *can* be done. The question now arises: Is it possible, within the framework of quantum mechanics, to assign a classical description to the measuring apparatus? This question, which has of course been a subject of endless debate for the last fifty years, poses itself for the following reason. If we consider a typical apparatus—say for example a Geiger counter—it is nothing but a complicated assembly of atoms and molecules put together in a certain way and therefore, one would think, it must obviously be *possible*, even if it is not necessary, to describe it within the framework of quantum mechanics. In particular, its macroscopic state (e.g. of being triggered or not) must in principle be describable in quantum-mechanical language. In practice, a realistic description would almost certainly require the use of a density matrix, but as Wigner (1963) showed conclusively, this fact in no way affects the point of the paradox I am in process of developing, so for simplicity of notation *only** let us assign to it a pure state. Thus, for example, let us consider the Young's slits experiment as above, with, for simplicity, a single telescope and counter set up opposite slit 2. Suppose the probability amplitude (wave function) of the (microscopic) particle is written in the general form $\psi = a\psi_1 + b\psi_2$, where ψ_1 (ψ_2) represents a wave propagating through slit 1 (slit 2) only. Consider first the case $a = 1$, $b = 0$, so that $\psi = \psi_1$: then the particle certainly propagates through slit 1 and misses the counter. The final wave function of the latter (cf. above) is then some wave function Ψ_1 which corresponds to the state of not being triggered. Conversely, if $b = 1$ and $a = 0$ ($\psi = \psi_2$) then the particle certainly propagates through slit 2 and enters the counter, which is thereby triggered. The final state of the counter is therefore described by some wave function Ψ_2, which is not only orthogonal to Ψ_1, but corresponds to macroscopically different properties. Thus, symbolically, we can write:

$$\psi_1 \to \Psi_1, \qquad \psi_2 \to \Psi_2.$$

So far, there is no particular difficulty: the quantum fluctuations of physical quantities in a macroscopic system in either of the states Ψ_1, Ψ_2 are extremely small relative to their thermodynamic average values in these states, so there is no inconsistency in describing each of the states in ordinary classical thermodynamic terms (cf. also footnote).

The problem arises when the initial state of the microsystem is a linear combination of the states ψ_1 and ψ_2 (as we know it must be in the Young's slits experiment if we are ever to produce a diffraction pattern): $\psi = a\psi_1 + b\psi_2$. In that case, by the linearity of the quantum formalism, it follows directly from the above equations that we have

$$a\psi_1 + b\psi_2 \to a\Psi_1 + b\Psi_2,$$

that is, the correct quantum-mechanical description of the final state of the counter

* This point needs some emphasis, since those physicists who deny the existence of a "quantum measurement paradox" frequently seem to labor under the illusion that those who assert it are unaware of the necessity of a density-matrix description (cf. below).

is a superposition of *macroscopically distinct* states. Such a state does not appear, prima facie, to correspond to *any* classical description (the "Schrödinger's Cat" paradox). It is all very well to say "we *must* describe the apparatus by classical mechanics, because otherwise we cannot communicate with one another", but in the light of the above considerations that begins to sound rather as if I should say: "My old car *must* be able to do 45 mph on the interstate freeway". If, after inspecting it sceptically, you say: "I doubt if it can; why are you so sure it must be able to?" it is hardly an adequate answer for me to say: "because that is what the law requires"!

It seems to me that Niels Bohr never gave a quite explicit response on this point. However, in his controversy with Einstein he did comment on a rather different point, namely that in the two-slit experiment it would in principle be possible to let the shutter be suspended freely and, by measuring its recoil, determine which slit the photon passed through. His comment is:

> "It is not relevant that experiments involving an accurate control of the momentum or energy transfer from atomic particles to heavy bodies like diaphragms and shutters would be very difficult to perform, if practicable at all. It is only decisive that, in contrast to the proper measuring instruments, these bodies together with the particles would in such a case constitute the system to which the quantum-mechanical formalism has to be applied."

From this it is possible to infer his probable response to the problem posed above. As the historian of quantum mechanics, Max Jammer, puts it in his book "Philosophy of Quantum Mechanics" (1974):

> "This statement, and similar passages, suggest that Bohr, recognizing the insufficiency of the phenomenalistic position, regarded the measuring instrument as being describable both classically and quantum mechanically. By concluding that the macrophysical object has objective existence and intrinsic properties in one set of circumstances (e.g. when used for the purpose of measuring) and has properties relative to the observer in another set of circumstances, or, in other words, by extending complementarity on a new level to macrophysics, Bohr avoided committing himself either to idealism or to realism. Summarizing, we may say that for Bohr the very issue between realism and positivism (or between realism and idealism) was a matter subject to complementarity."

Were he alive today, and had he read the voluminous literature of the last two decades on the quantum measurement paradox, I suspect Bohr might have made his argument explicit as follows: No doubt there are or could be circumstances (e.g. if the voltage across the counter were such that it could be ionized but not triggered) in which it is not excluded a priori that we could see interference between macroscopically different states of the counter. However, if we wish the counter to work *as a measuring apparatus*, then we must introduce a considerable degree of irreversibility, simply to stabilize the result of the measurement. Then we can argue (step 1) that there is now no possibility of exhibiting any interference between the macroscopically distinct states, and (step 2) that there is "hence" no inconsistency in assigning a definite macrostate to the counter, that is, in giving a classical description. Now I want to stress that in that (small) subset of the physics community which worries about the quantum measurement paradox, step 1 is not usually regarded as controversial or in need of further emphasis: Those who feel that there is a problem do *not* feel it because they believe that in practice, or perhaps even in principle, one can demonstrate interference between the different final states of the counter (and by the same token will not be reassured by further demonstrations that this is impossible). What worries them is step 2. It worries them because they feel that in the final analysis, physics cannot forever refuse to give an account of *how* it is that we obtain definite results whenever we do a particular measurement. This problem should not be confused with the question of why we get the particular result we do: the present issue is how it is possible for us to get a definite result at all. For those who believe there is a problem, this is perhaps *the* basic question in modern physics, and it has of course been subjected to endless debate in the literature: see, for example, part 4 of d'Espagnat's book "Conceptual Foundations of Quantum Mechanics" (1976).

However that may be, it is in any case interesting to raise the question: Is it possible, in practice, to prepare a macroscopic system (not necessarily something which is itself suitable to be a measuring apparatus) which can be in one of two or more *macroscopically distinct* states, and then present ourselves with the choice of either measuring which of the two states it is in, or of observing the interference between the two possibilities? Further, could we then demonstrate that in the second case it *could not have been* in a definite macrostate? If the answer to these questions is yes, and if the experiment is done and comes out in favor of the quantum-mechanical predictions, then this would be a spectacular confirmation of Bohr's viewpoint as interpreted by Jammer, i.e. of the extension of the idea of complementarity to the macroscopic scale. The whole reason that I am here giving this talk is that I believe that the answer to the above two questions is probably yes.

Before embarking on the discussion of this, let me digress for a moment to correct what seems to be a quite widespread misconception: the Aspect experiment, fundamental as it is for the interpretation of the quantum formalism at a microscopic level, sheds no direct light on the above questions. The main reason is that there is no question, in this experiment, of observing any interference between macroscopically distinct states of the apparatus. Perhaps the easiest way to see this is to note that all the experimental results are completely compatible with a theory in which each of the macroscopic counters is always in a definite macrostate, but that state produces a nonlocal effect on the distant photon. Needless to say any such theory would be incompatible with the universality of the quantum formalism and would involve some obvious difficulties, e.g. connected with the Lorentz-noninvariance of the time order of spacelike-separated events, but it is not clear that they would be any worse in principle than in the usual interpretation. In any case my point here is not the plausibility or otherwise of such theories, but merely that they are obviously not excluded by the experimental results, which therefore cannot be used as evidence for the superposition of different macrostates. To put it another way, the Aspect experiment forces us to sacrifice *either* counterfactual definiteness or locality (or of course induction), not necessarily both. The standard extrapolation of quantum mechanics to the macrolevel in effect sacrifices counterfactual definiteness, a choice which is exactly in the spirit of Bohr's thinking: My point is that at the macrolevel, even if not necessarily at the microlevel, the sacrifice of locality is not only not logically excluded but may arguably be no more unattractive.

5. *Interference between macrostates*

Returning now to my main theme, let me imagine that I had been able to put the above idea, of observing interference between macrostates and checking the complementarity principle at the macroscopic level, to Niels Bohr. I suspect he would have commented that the experiment, while certainly interesting in principle, would be in practice impossible. This, indeed, is the dogma which has developed in five decades of the literature on the quantum measurement problem. Probably, Bohr would have given two reasons. First, as he often emphasized, quantum effects are important only when the action S is on the order of the quantum of action $\hbar$. If E is the energy of the system in question and $\tau_{\mathrm{cl}} \sim \omega_{\mathrm{cl}}^{-1}$ is the order of magnitude of its classical period of motion, then in a typical experiment we have $S \gtrsim E\tau_{\mathrm{cl}}$, and so the condition $S \lesssim \hbar$ implies $E \lesssim \hbar\omega_{\mathrm{cl}}$; since for a macroscopic system the relevant classical frequencies are certainly not greater than, say, 10^{16} s^{-1} (and are often much less), this in turn implies that the characteristic energy scale for the macroscopic motion can be no greater than a few eV, i.e. on the order of the ionization energy of a single atom. It seems at first sight totally out of the question that any macroscopic variable should be associated with so tiny an energy.

What has changed the situation dramatically here is an effect which Bohr did not quite live to see, namely the effect predicted theoretically by Josephson (1962) and bearing his name. The really exciting thing about the Josephson effect is that, through it, the motion of a *macroscopic* variable (such as the current or trapped flux in a bulk superconducting ring, see below) can be controlled by a *microscopic* energy, on the order of the thermal energy of an atom at room temperature or even smaller. Although the Josephson effect itself is *not* an example of superposition of macrostates, it lays the basis for an attempt to build such a superposition (other systems, such as the charge density waves in a one-dimensional metal, or the photon field in a ring laser, may also be candidates, but for technical reasons I have myself tended to concentrate on Josephson devices and will therefore take my concrete examples from this area).

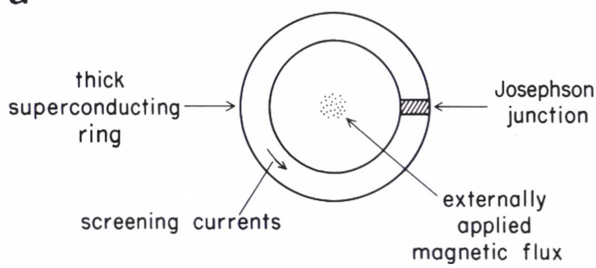
The second objection to the proposal to observe interference at the macrolevel, which has become a standard theme of the literature on the quantum theory of measurement, is that macroscopic systems, by their nature, always interact so strongly and so dissipatively with their environments that the quantum phase coherence between the different macrostates is inevitably washed out. The only way to combat this objection is to try to formulate as realistic a model as possible of (for instance) a specific type of superconducting device, incorporating from the beginning its interactions with an open environment, and to actually carry out a fully quantum-mechanical calculation of its behavior. [For example, in the case of a superconducting device the open environment includes the normal (unpaired) electrons, the radiation field, the phonons, the nuclear spins and so on.] There has been a considerable amount of work along these lines over the last five or six years [for a partial review, see e.g. Leggett (1986)], and the provisional conclusion is that while the conditions to see the effects of phase coherence at the macrolevel are indeed extremely stringent, it is not obvious that with modern state-of-the-art levels of cryogenics, microfabrication and noise control they could not be met in a purpose-built system. There are two principal reasons why the standard arguments to be found in the literature on quantum measurement theory fail: One is that even for a system interacting very strongly with its environment, much of the effect of the interaction may be adiabatic in nature and merely renormalize the system parameters without destroying the phase coherence (a point which seems to have been missed in much of the literature). The second is that the residual "dissipative" part of the interaction, which certainly *is* fatal to phase coherence, can be extremely small in a system such as a superconducting device because of the extremely low entropy (e.g. in a 1 cm niobium ring at 10 mK, if we assume that the standard Gibbs formulae apply, the electronic entropy of the whole macroscopic body is actually less than Boltzmann's constant!).

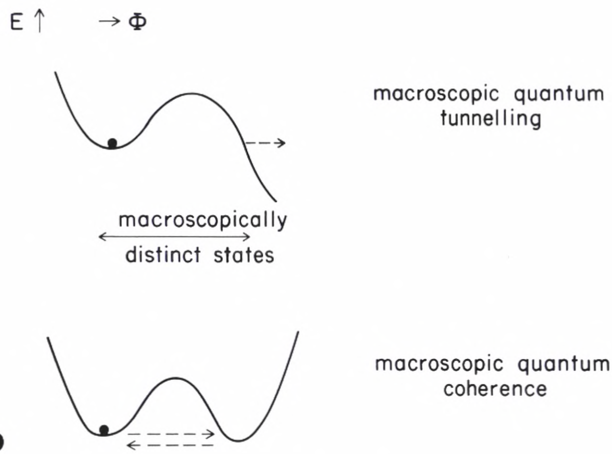
6. Macroscopic quantum tunneling and coherence

The system which I believe to be conceptually the simplest candidate for these experiments is an RF SQUID ring, that is, a bulk superconducting ring interrupted by a single Josephson junction (see fig. 4). An external magnetic flux can be imposed through the ring: we treat this provisionally as a c-number. It will induce screening currents which in turn produce their own magnetic fields, and hence the *total* flux Φ trapped through the ring, or equivalently the circulating current, is a dynamical variable. If we believe that quantum mechanics does indeed apply at the macrolevel, then the variable Φ must be treated as a quantum-mechanical operator; in fact, were we to treat this degree of freedom as totally decoupled from the rest, we could write down a wave function $\psi(\Phi)$ for it which should satisfy Schrödinger's equation, with the junction capacitance playing the role of mass. (However, in reality it is essential, as stressed above, to give a description which, while fully quantum-mechanical, adequately takes into account the effect of the environment.) By varying the parameters of the system, in particular the external flux, we can produce different forms of "potential energy" associated with this variable, and look for various specifically quantum-mechanical effects. Two kinds of behavior are of particular interest (see fig. 4): the first, which has become known in the literature as "macroscopic quantum tunnelling" (MQT) * is the macroscopic analog of the decay of a heavy nucleus by emission of an α-particle: the system starts in a metastable potential well and tunnels out through the potential barrier into what is effectively a continuum. What distinguishes this phenomenon from α-particle decay and other "microscopic" tunneling effects is that the classically accessible regions which are separated by the barrier are, by most reasonable criteria, macroscopically distinct. The second phenomenon of interest, which is generally known as "macroscopic quantum coherence" (MQC) is the macroscopic analog of the inversion resonance of the ammonia molecule; the system tunnels coherently backward and forward between two degenerate potential wells. Again, the classically accessible regions between which tunnelling takes place are macroscopically distinct.

* In restrospect "macroscopic quantum decay" might have been a better name.

a





b

Fig. 4. (a) SQUID ring; (b) macroscopic quantum tunnelling and coherence.

There are now at least six experiments in the literature on MQT, the most recent and sophisticated being those of the IBM, SUNY Stony Brook and Berkeley groups (Washburn et al. 1985, Schwartz et al. 1985, Devoret et al. 1985). * These experiments confirm that tunneling of a macrovariable does occur, and that its dependence on the parameters, including these which describe its dissipative interactions with its environment, agree with the theoretical predictions at least qualitatively. In addition, the Berkeley group have recently carried out a very pretty experiment (Martinis et al. 1985) in which the tunneling out of discrete excited states can be observed by exciting the system into these states by microwave radiation. Taken together, these experiments offer strong circumstantial evidence that, provided the interaction with the environment is adequately taken into account, the behavior of a macroscopic variable is compatible with the predictions of quantum mechanics extrapolated to the macroscopic scale. However, these experiments do not directly test the principle of complementarity at this level: for this we need an experiment of

* Most of these experiments were actually performed not on the SQUID ring described above but on a closely related system, a single junction biased by a fixed external current. The theory is however very similar.

the MQC or a similar type. Although the MQC experiment has in fact been attempted by Bol and co-workers (Bol et al. 1983) they were unable to reach the parameter regime where spectacular (i.e. oscillatory) results are predicted by the theory, and indeed none were seen. * Let us assume that in the future it will be possible to reach this regime, and examine the significance of the experiment.

The system is a reasonably (geometrically) macroscopic RF SQUID ring, in an external magnetic flux which is adjusted so that the ring has available to it two degenerate states, corresponding to a current I_0 circulating in either the clockwise or the anti-clockwise sense. The magnitude of the current is typically on the order of a few microamperes, so that by any reasonable criterion the two states are macroscopically distinct. To discuss the significance of the experiment, we introduce a variable $P(t)$ which is defined to be $+1$ if a measurement is made at time t and a current of magnitude I_0 observed with a clockwise sense, and -1 if the measurement is made and an anti-clockwise current of magnitude I_0 observed. If no measurement is made at time t, then the variable $P(t)$ is for the moment undefined.

As applied to this system, quantum mechanics makes two important predictions: (a) If the current is measured, then, except in exponentially rare cases (corresponding to finding the system "under the barrier"), the value is always found ** to be $\pm I_0$, i.e. any measurement will yield the result $P(t) = \pm 1$. (b) For an ideal (noninteracting, nondissipative) system the experimentally observed *correlation* between the values of $P(t)$ at different times (with no measurement made in the meantime) is predicted to be given by the simple formula

$$\langle P(t_i)\ P(t_j)\rangle = \cos\left[\Delta(t_j - t_i)\right], \tag{5}$$

where Δ is the characteristic resonance frequency of the system ($\hbar\Delta$ is the energy splitting between the even- and odd-parity eigenstates). Note carefully that the result (5) is a direct consequence of the existence, at times *intermediate* between t_i and t_j, of a superposition of eigenstates of P. For example, if $P(0)$ is $+1$ and we measure P after half a cycle, the prediction of eq. (5) is that P is then found with certainty to be -1. This results *only* if we take the state of the system at, say, a quarter cycle to be a *linear superposition* of $P = \pm 1$, even though we know that an observation at this time would certainly have given one of these two values or the other: had we taken the state to be a classical mixture of those eigenstates, then at half-cycle the expectation value of P would have been 0, not -1.

What this argument shows is that one cannot modify quantum mechanics piecemeal and expect to preserve the results. To that extent it is parallel to the argument given by Furry (1936) about nonlocal correlations shortly after the original paper by Einstein et al. (1935); he showed that if one replaced the pure

* Various experiments on an RF SQUID ring coupled to a tank circuit have also been interpreted as evidence for MQC-type effects (Prance et al. 1983, and earlier references cited therein; cf. also Dmitrenko et al., 1984). These experiments are so indirect, and involve so many unknowns, that I believe it would be rash to draw any firm conclusions from them in the present context.

** Strictly speaking we need to generalize the definition of $P(t)$ slightly to allow for the zero-point quantum fluctuations in each well, i.e. for small deviations of the current magnitude from I_0 (cf. Leggett and Garg 1985).

state of two separated particles by a mixture, but otherwise preserved the axioms of quantum mechanics, one would get results which violate the original quantum predictions and (as we know) also contradict experiment. Clearly, the significance of the MQC experiment would be enhanced if one could generate the analog, in this context, of the much stronger result embodied in the famous theorem of Bell (1964), i.e. prove that any experiment which agreed with quantum mechanics must ipso facto be inconsistent with a few "common-sense" assumptions. I shall now attempt to do just that, referring to a published paper by myself and Garg (Leggett and Garg 1985) for the details of the argument.

7. Macrorealism

Let me define a general class of theories about the world, which I shall call "macrorealistic", by the following postulates:

(1) Macrorealism: a macroscopic body which has available to it two or more *macroscopically distinct* states must at all times * "be" in a definite one of these states, whether or not it is observed.

(2) Noninvasive measurability at the macrolevel: by a sufficiently careful measurement (in particular, by an "ideal negative result" measurement in which we throw away the positive results) we can determine which of the above macrostates the system is in without affecting its subsequent dynamics (at least as regards these states).

(3) Induction: results obtained on the subset of an ensemble on which a given property is actually measured are representative of the properties of the ensemble as a whole.

The postulate (1) allows us to infer that $P(t)$ exists for all times t, whether or not measured, and (nearly) always takes one of the two values ± 1. A trivial adaptation of the inequality (4) then allows us to conclude that, for an ensemble of runs *in which the system is undisturbed* (e.g. by measurement) we have the inequality [where $P_i \equiv P(t_i)$]

$$\langle P_1 P_2 \rangle_{\text{ens}} + \langle P_2 P_3 \rangle_{\text{ens}} + \langle P_3 P_4 \rangle_{\text{ens}} - \langle P_1 P_4 \rangle_{\text{ens}} \leqslant 2, \tag{6}$$

and postulates (2) and (3) then allow us to apply this prediction also to the experimentally measured correlations. Finally, by taking (for instance) $t_4 - t_3 = t_3 - t_2 = t_2 - t_1 = \pi/4\Delta$, we demonstrate that the quantum-mechanical prediction (5), violates the above inequality.

The quantum-mechanical prediction (5) holds of course only for the unrealistic case of a system totally decoupled from its environment, and one might wonder whether a realistic degree of dissipative coupling would not modify the correlations enough that they satisfy the "macrorealistic" inequality (6). This question has motivated some fairly detailed calculations (Chakravarty and Leggett 1984, Leggett et al. 1986) and the upshot is that for a degree of dissipation, which may not be

* More accurately: at "nearly all" times; see the cited reference.

unrealistically low from an experimental point of view, the quantum-mechanical predictions continue to violate the inequality. Thus the experiment can in principle *discriminate unambiguously between macrorealism and complementarity at the macro-level.*

Several experimental groups are now actively studying the feasibility of this experiment, and the probability is that it will at least be attempted within the next two or three years. If it works, and confirms quantum mechanics, it will be a spectacular confirmation of the validity of extending Niels Bohr's thinking to the macroscopic level.

Now, the reaction of 99.9% of the physics community to this experiment may well be a bored shrug. We all know quantum mechanics is right, it will be said: so if the experiment comes out in favor of quantum mechanics, it only tells us what we already know, whereas, if it comes out against quantum mechanics, it is obviously a bad experiment. But is it? In the last five minutes of my talk I am going to try to throw an intellectual hand-grenade into the discussion. I start with an awful confession: If you were to watch me by day, you would see me sitting at my desk solving Schrödinger's equation and calculating Green's functions and cross-sections exactly like my colleagues. But occasionally at night, when the full moon is bright, I do what in the physics community is the intellectual equivalent of turning into a werewolf: I question whether quantum mechanics is the complete and ultimate truth about the physical universe. In particular, I question whether the superposition principle really can be extrapolated to the macroscopic level in the way required to generate the quantum measurement paradox. Worse, I am inclined to believe that at *some* point between the atom and the human brain it not only may but *must* break down. I am inclined to believe this for a simple but overwhelming reason: try as I may (and I have tried for many years) I simply cannot convince myself that any of the solutions proffered to the quantum measurement paradox is philosophically satisfactory, and to pretend otherwise, even in this place and on this occasion, would be intellectually dishonest.

But, you will say impatiently, is it not *obvious* that if atoms and molecules obey the superposition principle, and if Geiger counters, cats and even ultimately our brains are composed of atoms and molecules, then these macroscopic objects must themselves obey the principle? Indeed it is obvious—just as it was obvious, until 1957, that the laws of nature had to be the same in a right-handed system of coordinates as in a left-handed one. Just as in that case, the proper question to ask is: *Where is the evidence*? After all, whatever our theoretical prejudices, physics is supposed to be an experimental subject! Well, until quite recently there simply was no evidence on this point [despite a widespread misconception to the contrary, the so-called macroscopic quantum phenomena such as the Josephson effect itself or circulation quantization in liquid helium are quite irrelevant in this context—see e.g. Leggett (1980)]. As we have seen, recent experiments on MQT and on energy level quantization do provide strong circumstantial evidence that, as regards these phenomena, at least, quantum mechanics is still working at the level of superconducting devices. However, they do not directly test the principle of superposition of macroscopically distinct states, so that it is not a foregone conclusion that the MQC experiment, which is different in substantial ways, will come out in its favor.

8. The ultimate lesson of quantum mechanics

How might the principle break down? One logical possibility is that there is some effect (not of course accounted for in Schrödinger's equation) which is a strong function of the *number* of particles which behave differently in the two "branches" of the superposition. * Then, for experiments at the atomic level, where this number is 1 or 2, it could be totally negligible, whereas for macroscopic superpositions of the "Schrödinger's Cat"-type, where the number is $\sim 10^{23}$, it could totally dominate the behavior. This proposal involves various technical difficulties and I suspect it is in any case much too conservative. In fact, on nights when the full moon is *very* bright, I suspect that what may be needed is nothing less than a radical revision of the reductionist prejudice which has served us so well for 200 years, that is, the prejudice that *all* the properties of macroscopic systems can be explained, in principle, in terms of their constituent atoms and molecules. In fact, could it be that Bohr was right in his insistence that the macroscopic instruments "define the very conditions" under which the atomic phenomena appear, but wrong to conclude that the way in which this happens is itself unanalyzable and beyond the laws of physics? Could the ultimate "lesson of quantum mechanics" be that we eventually need to go *beyond* quantum mechanics? It would not be the first time that such a shift of viewpoint has happened in physics: for pretty well 100 years the orthodoxy was instantaneous action-at-a-distance—an axiomatic and unanalyzable concept—and the minority who felt the concept to be metaphysically objectionable were no doubt told to get back to their calculations and forget about such "philosophical" questions. In that case, as we all know, the doubters were eventually proved right and their doubts led to qualitatively new physics. Should the corresponding outcome some day occur with quantum mechanics, it would of course in no way contradict Bohr's viewpoint at the atomic level, nor would it negate his repeated insistence that the quantum theory has blocked off forever a return to our old classical common-sense picture of the world. Rather, any such theory of the future would almost certainly be a logical extension of his line of thinking, taking us even further away from classical notions in a way which at present we can hardly imagine.

Obviously, these are wild and vague speculations: but the experimental and theoretical program I have described in the main body of this talk is perhaps at least a small step towards exploring them. Of course, if you ask me to bet on the possibility that a well-conducted MQC experiment will come out against quantum mechanics, then when sober (and particularly after contemplating the results of the tunnelling and other experiments) I would probably not take odds of less than 100 to 1; I suspect that the solution of the measurement paradox, when it comes, will

* In view of some of the comments made in the discussion of this lecture, it is worth emphasizing that this is a quite different variable from anything connected with the geometrical *scale* of the phenomenon. Thus arguments of the type "We know (do we in fact?) that quantum mechanics works from the Planck length up to the atomic scale (25 orders of magnitude): what is so special about the extra ten orders of magnitude needed to get up to the human scale, that it should suddenly break down?" seem to me quite irrelevant. To the best of my knowledge neither cosmology nor particle physics offers any evidence against (or for!) the hypothesis discussed here.

come at a much deeper and more subtle level. But, after all, a journey of 10 000 miles starts with a single step. Were he alive today, Niels Bohr would surely have offered us odds of much more than 100 to 1 in favor of quantum mechanics continuing to hold at this and indeed at any level: but, as a scientist whose own life's work was based on the overthrow of what were then common-sense ideas, I believe he would at least have encouraged us to ask the questions and to do the experiments, and it is in this spirit that I dedicate this talk to his memory.

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Discussion, session chairman W. Kohn

Peierls: The experiments discussed are interesting and I hope will be done, but I would bet heavily on their confirming quantum mechanics. This is because I do not see that on the way from the atomic to the macroscopic there can be any point which makes a qualitative distinction. I do sympathize with Leggett about the difficulty of understanding how the observer can be described in terms of quantum mechanics. I believe he cannot be so described—not because he is macroscopic, but because he is alive. I believe that it is not certain that biology is a branch of physics in the sense in which chemistry is a branch of physics. This is a revolutionary thought, too difficult to be spelt out in the discussion.

Leggett: If you asked me today I would certainly bet myself that the solution would not be at the level of "inert" physical devices such as SQUIDs, but would more likely be found at the level of complexity and organization which must be necessary for biology, let alone psychology. But one has to start somewhere!

Thirring: I would like to strengthen what Rudolf Peierls has said. If one accepts the present view then the fundamental length scale where the real action takes place is the Planck length or something a little bigger, say 10^{-28} cm to be generous. In these units even an atom is an immense structure, 10^{20} times bigger, and we are another 10 powers of ten bigger. So why should the boundary between microscopic and macroscopic be just between 10^{20} and 10^{30} times the real microscopic length and the laws of nature change drastically in this region?

Leggett: I feel that this is a difficult theme. I think it is a difficulty which makes it very obvious how deeply ingrained are our reductionist prejudices, even when we try consciously to override them. I think it is not obvious that the behaviour of large collections of atoms must obviously be determined by the behavior of their individual constituents. This indeed goes against the scientific thinking of the last 200 years. But I personally think that there is a good chance that we may have to revise our ideas in this direction.

Bleuler: Underlining Walter Thirring's remark I would like to emphasize that a neutron (usually described by Schrödinger's equation) is, in fact, a most complicated bound system (valence quarks, seaquarks, gluons) having certain similarities with macroscopic systems. The same holds, in principle, for electrons (surrounded by the photons) and also for the quarks (surrounded by the gluons).

Leggett: I too would like to believe that fundamental laws of physics don't show dependence on the scale of things. But this is in the end a matter for experiment.

There is in any case a fundamental difference between the neutron and the macroscopic system in that the latter corresponds to states of large baryon number B rather than B about equal to one.

Jens Bang: I would like to recall Bohr's stressing the *concepts* and the *language*, and I would like to do it in connection with Schrödinger's cat. As we remember, it was in a superposition of being alive or dead. One must ask: Is there a meaning to this? Are concepts like life and death really describable as quantum states? Maybe they are hardly describable as classical physical states!

Leggett: Of course a proper description of the state of the cat in Schrödinger's thought-experiment, if it can be given at all, must be given in the language of density matrices rather than wave-functions. However, as I tried to emphasize, Wigner has shown, in my opinion conclusively, that doing this in no way blunts the force of the paradox. If it is the question of whether concepts like "life" and "death" can ever be adequately described, even in the language of density matrices, this doesn't seem to me a real problem since we can always replace the cat by a macroscopic but inert object such as a counter.

Weisskopf: I would personally put a bet even higher than 100 : 1, perhaps 10 000 : 1, that quantum mechanics is the only relevant theory in the macroscopic world. But still I have my doubts about those remarks that tell us that the atom is actually much nearer to man than the quark world or whatever happens at the Planck length.

When we go from atoms and molecules to us human beings, complexity, organization and disorder enters, concepts that are rarely found at the lower level. We do not yet understand too well complexity and organization and disorder, and therefore it is possible that some new unexpected fundamental principle may enter. The probability is not zero because of our limited and inappropriate knowledge of what happens in complex organizations, in particular in the brain.

Anderson: In connection with complexity we should really also discuss the problem of the purely quantum-mechanical computer. This was a deliberate omission in my talk because I am not knowledgeable enough and am also sceptical. That is really at the heart of this basically philosophical problem: Can one make a quantum computer that has essentially no dissipation? I think I can imagine a scenario that solves all of the measurement paradox difficulties, if I can say that dissipationless switching processes are taking place throughout the whole complex system. If one could actually make a device that could work as a computer, i.e. totally reversible and totally quantum-mechanical—that would disturb me very much and I am sceptical about it.

Leggett: I would agree with that statement, but perhaps not with the interpretation you put on it. In particular, I would strongly disagree that your scenario, even if attainable, would in any way solve the quantum measurement paradox.