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# DOES GOD PLAY DICE? A DISCUSSION OF SOME INTERPRETATIONS OF QUANTUM MECHANICS

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# DOES GOD PLAY DICE?

## A Discussion of some Interpretations of Quantum Mechanics†

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ABSTRACT

Three interpretations of quantum mechanics are re-examined: the orthodox or "Copenhagen" interpretation, the expanding worlds interpretation, and the interpretation that is generally named statistical. It is concluded that the first two interpretations introduce unnecessary conceptual difficulties, and that it is consequently preferable to accept the statistical interpretation.

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"One must always tell what one sees.
Above all, which is more difficult,
one must always see what one sees."

Charles Péguey

## I. INTRODUCTION

The topic of this paper is still controversial; its conclusions are in disagreement with those of quite a number of physicists, mathematicians, and possibly, philosophers. Yet the subject can be discussed in a reasonably intelligible manner without using many equations. Consequently, the topic (though not necessarily this paper) ought to provoke wider interest than more detailed applications of quantum mechanics; but correspondingly there are special dangers inherent in such a discussion. Neither quantum mechanics nor special relativity are "common-sense." Thus one leaves the stepping stones of mathematics at the risk of landing in the slough of despond or the bog of folly. I must, therefore,

emphasize that, although I shall say nothing that does not owe a direct debt to previous discussions, my mistakes are my own. My hope is to stir up a little further discussion on a topic which I consider both important and considerably misunderstood. To accomplish this I shall try to make my arguments as concrete and down to earth as possible.

Possibly some reader did not catch the meaning of the title. It is not intended to be gratuitously blasphemous, but is based on a line from a famous letter written by Albert Einstein to Max Born.[1] In this letter he says that he finds it difficult to believe that God plays dice with the world. He is referring to the, at least seeming, indeterminism of the microscopic world of quantum mechanics. This paper will attempt to describe, in an abbreviated form, the various interpretations of quantum mechanics. Here the very fame of Einstein's remark has militated against a serious appraisal of Einstein's criticism of some standard views of quantum mechanics. So also has the knowledge that Einstein kept on producing gedanken experiments which appeared at first sight to reveal inconsistencies in quantum mechanics, but none of which succeeded in revealing any such inconsistencies. However, I shall not discuss any of these, since at least one of his gedanken experiments is usually described in a standard course on quantum mechanics. What I do want to discuss are the criticisms that Einstein,[2-3] and others,[4] have made of the "orthodox," sometimes called the "Copenhagen,"[5-7] interpretation of quantum mechanics. This is the view of quantum mechanics

seen by such distinguished persons as, for example, Bohr,[8-12] Dirac,[13] Wigner,[14-17] and Von Neuman.[18] Though the views of these authors are not quite identical, and not unchanging over time,[19-20] they do have pronounced similarities.

## II. DICE PLAYING

First, let us be clear what we mean by playing dice. In a dice game we know the mathematical probability that, for example, a particular sequence will occur, or that a certain number will show a certain number of times in a given number of throws. On the other hand, we do *not* know what will happen in any actual experiment. The more often we repeat the experiment, however, the closer we expect the distribution of results to approach the probabilities. If they do not, we will very likely suspect that the dice are loaded, though, of course, they are not necessarily so.

What has this to do with quantum mechanics?

In the Schrödinger representation of quantum mechanics there appears the wave function "$\Psi$" that is in some way supposed to describe the behaviour of a particle. This wave function developes in time in a rigorously well-defined way according to Schrödinger's equation. The actual form of Schrödinger's equation will depend on the assumed interactions appropriate to a particular physical experiment, and the solution for "$\Psi$" will also depend on the boundary conditions; that is, the preparations we make before turning on, and

perhaps during, the experiment. In practice, physicists generally agree not only on the computations involved (although the actual form of Schrödingers equation -- that is, the postulated physical interactions -- may be under discussion) but, also on the resultant prediction for a sequence of measurements. These predictions are, in general, probabalistic. Thus for a specified actual experimental setup the predicted results will be dice-like.

The question that arises in interpretative discussion of quantum mechanics is what meaning, or reality, we may or may not associate with "$\Psi$" or with "$\Psi^*\Psi$" and other combinations of symbols such as expectation values.

Before I attempt to discuss this further, I wish to dispose of two small points which frequently obfuscate a discussion of quantum mechanics; this should save time later.

## III. FALLACIES

### A. The Uncertainty Principle

A frequent statement, even in good texts,[21-23] is that it is impossible to measure the momentum $p_x$ and the position x of a particle *simultaneously* with an accuracy greater than

$$\Delta p_x \Delta x = \hbar/2 \tag{1}$$

where $\Delta p_x$ is the uncertainty in $p_x$ and $\Delta x$ that in x.

The first point to make is that quantum mechanics does *not* include in its formulation any prediction for the simultaneous measurement of two non-commuting variables. Rather, measurement of one variable affects the predicted subsequent measurement of another variable (unless it commutes with the first) in a way described by the uncertainty principle. Attempts have been made to produce a formalism that would encompass such simultaneous measurements[24, 25] (though it is difficult to envisage, for example, the simultaneous measurement of the spin of a particle in two orthogonal directions -- see Appendix B). None have been entirely successful, and I shall not discuss them here.[4, 26]

The second point to make is that in some cases one uses a counter to find the position of a particle and deduces what its momentum *was* before the counter interfered. The product of the uncertainty in this position measurement and the uncertainty in the deduced measurement of momentum does not obey the uncertainty principle of Equation (1) but can be measured (in principle) with far greater accuracies. If these are considered simultaneous measurements, then Equation (1) does not apply.

Consider a single slit diffraction pattern where particles with momentum p in the z-direction may pass through a slit of width $a$ (or be stopped by the screen). $\delta x$ is shown in Fig. I and is the uncertainty in the position measurement. It is roughly the size of the receiving end of the counter. $p_x$, the x-component of the momentum

of one particular particle is approximately $p\sin\Theta$, where $\tan\Theta = \frac{x}{p}$. The uncertainty $\delta p_x$ in this is then given by

$$\delta p_x = (a + \delta x)\cdot p\cdot\frac{\cos^3\Theta}{d} \qquad (2)$$

Thus $\delta x\cdot\delta p_x$ can be made as small as desired, simply by making the screen more distant. Note that we have actually made only *one* measurement: the arrival of a particle at a particular counter.

$\Delta x$ is usually taken to be $a$, the width of the slit: that is, it's taken to be a measure of an attempt to localize the particle (in the x-direction) at the beginning of the experiment. $p_x$ is now taken to be a measure of $\Delta p_x$ the uncertainty introduced in the momentum because the particle has passed through the slit. $p_x$ is, of course, different for each particle arriving at the bank of counters. Each particle is *separately* registered by *a* counter. Thus $\Delta p_x$ is measured by repetition of a large number of measurements of $p_x$; and calculated as the value of $p_x$ appropriate to the first diffraction minimum. Now the source of the confusion is clear: the uncertainty principle refers to state preparation, and not to the actual particular single measurement performed during an experiment. $\Delta x\Delta p_x$ will, however, be appropriate to the histogram of a large collection of measurements of x and $p_x$ *separately* made during an experiment on the same particularly prepared wave packet. It does not in any way refer to a joint probability distribution of a classical kind.[26] The uncertainty principle applies to the predictability of the measurement

of a previously prepared pure state.[27] I have written this section almost as though $x$ and $p_x$ had retained their classical meaning intact. This is difficult to avoid, but it should be kept in mind that they do not. This is illustrated by the fact that classically one would think of measuring momentum by measuring the position of the particle at some time $t_1$ and then later it's position again at time $t_2$, with the assumption that $t_1$ and $t_2$ can be made as close together as we like. Classically, either of these, or an additional position measurement at time $(t_1 + t_2)/2$, might be considered a "simultaneous" position measurement. In quantum mechanics both the method of measurement and the corresponding interaction with the particle, and the time sequence of all the measurements would have to be kept in mind.

B. Wave-Particle Duality, or Complementarity

As we all know, in classical physics the concepts of a wave and a particle are very different. It is sometimes stated that it is not possible to see the wave and particle aspects of, for example, an electron in the same experiment. They are complementary.[9] A moment's thought will make it apparent that just the experiment that we have been discussing falsifies this statement. At least, if we mean the collection of measurements, and not that of the arrival of a single particle, this is so. Indeed, experiments showing the *wave* nature of the electron seem usually to involve counting individual electrons, with counters or photographic material (for example, in the original experiment of Davisson and Germer[28] showing the "wave-like" diffraction of electrons by a crystal, they did something equivalent. They measured the current received at a small aperture

successively placed, with respect to the crystal, at different angles).

This illustrates, also, that many of our difficulties arise because we tend to use words with classical connotations instead of either redefining them or using fresh words. It is highly comprehensible that because quantum mechanics developed so rapidly in the mid-twenties and for the next few years the conceptual difficulties presented by this rapid development were extreme. Physicists were also much influenced by Bohr who felt that there existed "the necessity of making extensive use, nevertheless, of the classical concepts upon which depends ultimately the interpretations of all experience . . . " and who spoke of "our endeavors to utilize all the classical concepts by giving them a suitable quantum-theoretical interpretation." Although it is reasonable to expect quantum theory to reproduce classical particle behaviour for billiard balls, to the required accuracy, it is not necessary that we retain classical meanings for the word particle if we choose to use that word for an electron.

So conceptual difficulties can appear later because we have not redefined words appropriately or, alternately, admitted they were inappropriate and chosen new ones. This is the reason for which I believe Einstein[29] chose to use the word "definition" in discussing simultaneity. It was not because we are permitted (except in a trivial sense that we can define any noise any way if we wish) to make a word mean whatever we want it to, but, on the contrary, that

our physical discoveries may force us either to abandon words or to define them in a precise new appropriate way, or in pairs of new ways (for example, mass and rest-mass). Einstein's ability to do this made lucid the actualities of special relativity. Bohr's preference for a form of meaning invariance still influences quantum theory.

Few would doubt that one can and does develop habits in thinking. Some seem to accord quite closely with Kant's "synthetic judgments a priori" for example, or Mach's suggestions that Euclidean space is a necessary premise for thinking. It now appears that infants, long before the age of speech (let alone reason) do indeed have many spatial concepts.[30] However, it also seems true that we are not limited in our conceptual development. The wild and generally unacceptable reasoning on one age may be the rigid, common-sense thinking of the next. For example, it seemed extraordinarily strange in Newton's day to think of "action-at-a-distance." An incomprehensible concept to the extent that Newton emphasized that one ought just to say that things behaved in a reasonable way "as if" gravitational forces acted this way. Indeed in Europe it was considered quite unacceptable because it seemed to revive the occult. Voltaire said that forty years after the publication of the Principia (at the time of Newton's death) Newton had no more than twenty followers outside England. Yet nowadays (*except* for physicists!) the intelligent layman doesn't merely accept that gravitation forces act "at a distance," he thinks in these terms without difficulty.

Thus a physicist can also think non-classically. It may still be hard to learn to do so. It may be harder than thinking of gravity as "action-at-a-distance," since invisible gravity has always been with us on the surface of the earth; and now, in addition, we live in the space age. None-

theless, there are so many problems in physics that require the use of special relativity, that physicists learn to think in these terms. Similarly for quantum mechanics: many physicists, and chemists think quantum mechanically. It is no longer necessary as in the exciting, fascinating, brilliant twenties to invent ways of making quantum theory reconcileable with classical concepts.

## IV. WHAT IS A THEORY?

To discuss interpretations of any theory, it is necessary to be as clear as possible what we mean by a theory. This may sound a bit odd to some physicists, especially experimentalists; although it would probably not seem at all so to a philosopher! I shall not be so brave as to attempt to satisfy a philosopher, but content myself with a brief, and necessarily crude, outline.

In the physical sciences, a theory usually consists of:

(a) A mathematical formalism,

(b) Input and output: interpreted in terms of experimental (or situational) input, and predicated, measurable output.

The minimum this can mean is that the algebraic symbols, or some of them, or some combination of some of them, have a physical meaning. This must necessarily remain looser than the postulated, deductive and mathematical part -- but, I contend that there must be a clear description in clear (or clearly defined) words how the formalism is to be applied to

measurement. We may have a delightful and mathematically perfect formalism, which has no known physical significance. It is also frequently true that the same formalism may be useful in quite different and unconnected physical theories. This certainly seems to indicate that (b) is essential. However, as we shall see later, there are some authors[31-35] who disagree.

At the opposite extreme are those who feel that every mathematical step should be accompanied by a corresponding physical picture; in other words, every step of a calculation should be interpreted, or interpretable. For didactic purposes, this would obviously be ideal; but desirability does not necessarily make it so. Indeed, I believe that philosophically this constitutes too strong a demand. A theory must, of course, be internally consistent. It must also predict the results (even if incorrectly?) of any experiment that it is possible to design within its framework. Thus, if we demand a picture of what is "really" happening at some intermediate stage during an experiment, then it follows that we must be able, at least in principle, to measure this intermediate stage without appreciably disturbing the subsequent measurements. If we want to slice the formalism, we must, according to this argument, slice the experiment too. If the theory itself says that this "slicing" alters the later measurements, then it seems to me to be verging on the ridiculous to ask what "really" is happening at this point, or to demand a physical picture of it. If I seem to be stressing the obvious, the reason will, I trust, become apparent when

we discuss the interpretations of the wave function in more detail.

## V. BOHR-COPENHAGEN SCHOOL*

We have already discussed complementarity (wave-particle duality), and a little about measurement. It is now time to venture to discuss the interpretation of the wave function. I shall, for simplicity, stick as much as possible to the Schrödinger representation, but anything I say could equally well apply to any representation, or be stated abstractly.

According to the "orthodox" view, after we have prepared a system so that it is in a pure state (that is, so that there are no ordinary statistical uncertainties about its composition, and correspondingly, no necessity to introduce the density matrix into our discussion), then the wave function of the system is a complete description of that system. Complete is a word that should now be defined. However, I will return to this in the discussion of Einstein, Podolsky and Rosen's paper.[2] A similar statement, which will serve us better for the present is that the wave function describes the individual system. For example, let us consider an atom which can decay by emission of some particle P, and let us further suppose that this decay is isotropic. Then the spatial part of the

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* This interpretation is sometimes called the "orthodox" interpretation.

wave function describing P will also be isotropic. If we take the position that the wave function describes an individual system we have P equally present, or absent, at all points equidistant from the atom. Now we all know that when we actually measure the arrival of P at some counter, it does not register at all other counters that are placed at the same distance from the atom. On the contrary, if we really knew that we started with only one radioactive atom -- which is probably an unrealizable situation -- after one counter had registered we might well turn off the experiment. It should be mentioned at this point that some authors[36, 37] (see Appendix A) feel (at least if P is a photon) that we should test this. In the orthodox approach, then clearly something special happens when we make a measurement. In some way the wave function "collapses." In Dirac's terminology, the measurement of any "physical observable" -- which in the formalism is represented by an Hermitian operator -- results in a "jump" such that we always have as a result of the measurement an eigenvalue of the operator. In addition, Dirac claims that a subsequent measurement of the same observable will result in the same eigenvalue. The second statement is not, I believe, an essential part of the orthodox interpretation. It is also misleading. This may be seen immediately by considering *any* measurement that disturbs the system in question. For example, a position measurement automatically introduces an uncertainty in subsequent position measurements. It may be argued that Dirac's statement should only be applied to observables with discrete eigen-

values. However, even so it is, at best, ambiguous; for consider the decay of an excited state observed by detection of an emitted photon. Here we find out that the atom was in an excited state only at the cost of that energy eigenvalue being altered. Of course, we can find cases where once we have measured an eigenvalue a subsequent measurement yields the same value. An example that springs to mind is a Stern-Gerlach spin measurement.

Let us consider the "collapse" of the wave function a little further. I emphasize that the necessity for this collapse arises because of the demand that the wave function describes the individual system. Thus in measuring our wandering particle P, we "collapse" its wave function to one localized in position. The final remarks in the preceeding paragraph may help to point up the oddness of this. Had we prepared a completely polarized atomic beam, no subsequent Stern-Gerlach measurement in the same direction will collapse the spin wave function. So, "it ain't necessarily so."

## VI. EINSTEIN, PODOLSKY AND ROSEN REVISITED

In 1935 Einstein, Podolsky and Rosen wrote their famous paper[2] entitled "Can Quantum Mechanical Description of Reality Be Complete?" This paper (which I shall refer to as EPR) has been referred to as a paradox, and by Rosenfeld,[38-41] almost unbelievably, as a fallacy! I contend that, in fact, it is neither a paradox nor a fallacy, but simply a valid criticism of the interpretation of

quantum mechanics proposed by Bohr.

First, I will quote their definitions of what they mean by complete, and what by physical reality.

(a) They define a complete theory as one in which "every element of physical reality must have a counterpart in theory . . ."

(b) They identify "physical reality" in the following way: "If, without in any way disturbing the system we can predict with certainty (i.e., with probability unity) the value of a physical quantity, then there exists an element of physical reality corresponding to it."

(c) My contention will be that EPR does indeed prove that the "Copenhagen" Quantum Mechanics is not a complete theory with the above definitions of complete and real.

Note that this is a logical contention[42] and places no obligations on nature. Nature is under no necessity to produce a complete theory but logic necessitates that Proposition (a) and proposition (b) imply proposition (c).

Let us follow the gist of their arguments once more in the form proposed by Bohm.[22] We start with two like particles of spin 1/2 initially existing in a singlet (spin zero) state.

Let us assume the decay occurs (almost) at rest and is

reasonably well localized near point A. By the EPR prescription, the spin of either one of the particles -- arbitrarily called particle (1), say, is real in two mutually perpendicular directions. This follows because we may set up a Stern-Gerlach magnet in any direction we choose, and so measure the spin of the other particle -- particle (2), say. Thereafter we shall know the spin of particle (1) in our chosen direction which could have been in either of the two orthogonal directions. No spin eigen-vector for particle (1) can define a situation in which definite spin is assigned to it in two mutually orthogonal directions since the spin operater in two orthogonal directions are non-commuting. Hence both these elements of physical reality cannot be described by an eigenvector. Thus their proof. The wave function does not furnish a complete description of the individual system.

Let us analyse this setup a little further. We place a Stern-Gerlach magnet to make a spin measurement (in the x-direction) in a position a long way from the origin of the decay, A. If a particle enters our magnet we can measure whether its spin is up or down with respect to the x-direction. We now know unambiguously whether the spin of the other, unmeasured, particle is up or down. This follows since symbolically we can represent the spin wave function of the combined system by:

$$\uparrow(1)\ \downarrow(2) - \downarrow(1)\ \uparrow(2)$$

Thus when we measure the spin of one particle to be, for example, "up" then the formalism gives zero probability for the spin of the other to be up, and unit probability for it to be down.

If we interpret this to mean that the wave function has collapsed, then this collapse must have travelled instantaneously over the whole wave function. Such a suggestion must dismay us (let alone the creator of special relativity?) unless we can somehow argue that it is not a real signal. Thus the following two statements are not compatible.[43]

> (1) The wave function provides a complete (or exhaustive) physical description of an individual system.
>
> (2a) The result of a real physical measurement on one object does not affect the result of a measurement on another object *spatially separated from it, and non-interacting.*

The proposition (2a) will be renamed (2b) if the italicized words are replaced by "widely spatially separated from it," or by (2c) if we substitute "at a space-time location which is space-like with respect to the first." It should be remembered that for any ordinary postulated interaction (2b) does approach (2a) very rapidly. It should be emphasized that (1) conflicts with (2c) as much as with (2a) and therefore conflicts with the special theory of relativity.

When Inglis[44] says, referring to EPR, that "Their query focuses attention on the meaning of separating a system into two parts," he appears to be confusing meaning and physical feasibility. The assumption that it *is* physically possible to achieve effective physical separations is implicit in many, probably all, experimental set-ups. Thus I think the meaning is clear although, of course, the

assumption may be wrong. In the section on hidden variable theories this will be considered again.

There is no difficulty if we give up (1). Instead, let us assume that the wave function describes an ensemble of similarly prepared systems. In effect, let us go back to playing dice. There can be no objection in a statistical interpretation to a situation in which a particular result has a non-zero and non-unit probability, but in which we know that if we obtain *in fact* some particular value we shall know the value absolutely of some other quantity. Such a situation occurs each time we throw a die. If the top face of the die shows a six, for example, then without looking we know that on the bottom face is a one. An observer under the glass table on which the die lands will so observe it. In general, if the upper face shows y spots, then the hidden face has (7-y) spots. Thus Inglis' assertion that in measuring one spin "This correlation is disturbed by the measurement on which the prediction is based" is not appropriate. The correlation is not disturbed. To carry our analogy just one step further, we also (as with the components of the spins in other directions) do not know what number of spots there may be most nearly facing North, if we can only look from above. We need not, and probably should not, carry the analogy further.

Thus the EPR paper may simply be taken as a criticism of the orthodox *interpretations* of quantum mechanics, and *not* of quantum mechanics itself.

If, then, we insist on (2) as either (2a), (2b) or (2c) -- and

I, for one, would be most reluctant to accept any theory for which the final one did not hold -- we must give up the orthodox interpretation. We must accept, also, that Copenhagen quantum mechanics is *not* a complete theory by the EPR definition. The theory is complete only if the wave function does not describe an individual system.

Thus quantum mechanics as a *theory* can only be considered complete by assuming that the wave function is an incomplete description of an individual physical system. It certainly seems possible then that quantum mechanics while not "complete" in the second sense, cannot be completed: that it is all we can aspire to. I shall return later to later attempts to make quantum mechanics determinate by hidden variable theories, and indicate why Von Neumann's theorem is inapplicable.

## VII. SCHRÖDINGER'S CAT

"So, naturalists observe, a flea
Hath smaller fleas that on him prey;
And these have small fleas to bite 'em,
And so proceed *ad infinitum.*" Jonathon Swift

To elucidate further the difficulties of interpretation let us resurrect Schrödinger's[45] poor cat once more. The set-up is as follows: there is one radioactive atom surrounded by counters. The half-life of the atom is one hour. When any counter records the

decay, it also acts as a trigger so that a hammer falls and smashes a flask containing hydrogen cyanide. Surrounding this somewhat unusual experiment is an air-tight glass box (to protect the physicist and keep the vapor concentrated) which also contains a cat. After one hour, the wave function describing the radioactive atom will be 50% undecayed and 50% decayed. Logically there is nothing to preclude us from describing the macroscopic system that includes the cat by a wave function. As far as the wave function is concerned, the cat is 1/2 alive and 1/2 dead. Does, then, the wave function describe the individual system? If it does, then it seems reasonable to ask whether the cat is dead or alive since, neglecting recent problems in medical ethics, cats are generally considered to be either alive or dead. The phrase "half dead" is not usually taken literally. How shall we collapse the wave function so that the poor cat is out of its agony? Clearly (?) we must *measure* the deadness of the cat and settle the matter one way or the other. Let us put a spring under the box that will register when the cat falls down, and let us connect it to a dial. The pointer on the face of the dial will turn when the cat dies. But wait a moment: why should we not also describe this macroscopic addition by a wave function? For this wave function, the pointer is half in the initial position and half in the turned position. It is obvious that no matter how many complications we introduce we remain in the same difficulties. We can have infinite regression of counters and dials, etc.[46, 47]

No suggestions seem to lead us satisfactorily out of this

dilemma. For example, it does not seem reasonable to suppose that there is something mystically different about measurement that makes it unlike other physical processes. Attempts have been made, however, to introduce non-linearities in measurement.[48] I am not aware of any that have succeeded. But it does not seem reasonable that they should, unless they also alter our understanding of those simple processes that quantum mechanics describes.

## VIII. THE BUCK STOPS HERE

Harry S. Truman

Wigner[16] has the most mystical explanation: he suggests that the regression stops at human consciousness. This should surely be an exciting thought to serious students of ESP. It is powerfully reminiscent of the deliberations of the idealist philosophers on the Cartesian doubt of the existence of objects outside one's own consciousness. Knox's witty comment seems relevant:

"There was once a man who said "God
Must think it exceedingly odd
If he finds that this tree
Continues to be
When there's no one about in the Quad."

Ronald Arbuthnot Knox

Before leaving this fascinating solution of Wigner's, I will

only make one last brief comment. Or rather, I would like to ask a question. At what point in evolution did human consciousness acquire this quality? Perhaps, after all the *cat's* consciousness might do the trick? Cat lovers who have, no doubt, been seething at our treatment of the cat surely will object to our neglect of his consciousness also. Bishop Berkeley, of course, would stop at the consciousness of God. His argument is succintly, if irreverently, summarized by the famous author Anon., who said:

> Dear Sir, Your astonishment's odd:
> *I* am always about in the Quad.
> And that's why the tree
> Will continue to be,
> Since observed by Yours Faithfully God."

In the statistical interpretation there is no problem. The wave function brutally represents a vast array of similarly placed cats. In any particular experiment about half the cats are dead after an hour, and about half are alive. In a sequence of such experiments there will, as usual, be statistical fluctuations.

This last paragraph has been criticized on the grounds that this represents a mixture and not a pure state, and that Wigner[16] has shown that such transitions are strictly forbidden. But it is not a mixture. Nor is there any, other than accidental, reason to suppose this.

It is an interpretation of a pure wave function (cat 50% dead) and *not* a transition to a mixed state. The wave function developes in time according to Schrödinger's equation which enables us to calculate mathematic probabilities for the occurence of certain events, and to make appropriate predictions. Occasionally an event may have probability unity, but usually it will not. We can, in general, using the density matrix formulation, separate the predications associated with a pure state from those associated with a mixed state. If for some particular type of measurement we can set up a pure state that predicts certain *results* to be the same as some of those of a mixed one, this is not in itself a criticism of the "statistical interpretation." [Though possibly "probabilistic interpretations" might be a verbal improvment, less misleading or confusing than the former.] Certainly, in the cat case, we can set up a macroscopic situation in which we break the flasks in some suitably designed random way with the right probabilities to duplicate the results of cat mortality (with similar statistical variations).

To illustrate the possible confusion between a pure and mixed state, consider a beam of particles that travel in the z-direction and have spins polarized in the x-direction, the "up" direction. The experimenter has a Stern-Gerlach magnet A which separates the particles according to their spins in the y-direction. After magnet A is placed, magnet B which is somewhat similar to A, except that the field is reversed, and its strength is so adjusted that the separated beams re-combine. He then uses a further magnet

C and counters D to show (insofar as his vacuum is good, etc.) that he has a pure state -- particles with spin "up" in the x-direction. However, if unknown to him I sneak some material between A and B so that one of the beams is stopped and the other is not, his results will be that (approximately, of course) 50% of his particles are spin up and 50% spin down. He may interpret this as an unpolarized beam -- that is, not a pure state. But you and I know that it is polarized in the y-direction. It is in a pure state. So also our cats.

Incidentally, it might be interesting if instead of a block in one channel we could set up an apparatus E to check by which channel the particle has traveled, and to do this in such a way that no depolarization occur -- i.e., a position not a spin measurement -- and do it without stopping the particle. Then, presumably, an experiment at C and D would no longer show particles with x-direction spin "up." It could be used to prove or disprove Wigner's "consciousness" theory since reading or not reading E should not affect this x depolarization, though, of course, it would affect our understanding, and also the spread in the beam. Such sequential measurements, though in a sense intrinsic to the theory, seem almost impossible in practice. Nor do I know of one accomplished.

## IX. EXPANDING WORLDS[31-35] -- ANOTHER INTERPRETATION

There is another interpretation which has been seriously

proposed by several authors. They assume that:

(a) Quantum mechanics generates its own interpretations.

(b) Wave function *never* collapses.

(c) All the possible worlds potentially described by the wave function exist simultaneously.

Thus after an hour, in one world the cat is alive, the pointer has not moved, etc., etc., -- and the man sees the cat alive. In another world the cat is dead. Every quantum transition in another galaxy is splitting our world into myriad of almost duplicate worlds -- although some of them will become incredibly different in time. The laws of quantum mechanics prevent us from feeling it -- fortunately. My mind boggles. There will presumably be maverick worlds where kettles of water boil when placed on blocks of ice, where the cyanide flask breaks but all the gas collects in one corner of the box, leaving the cat happily alive . . . However, I must say that I would, if I were about to go down with the Titanic, find little comfort in thinking of the worlds in which it might have avoided that iceburg!

On page10, I argued that a theory ought to consist of both a mathematical formalism and an interpretation. If this is so then it would be contradictory (or tautological) to speak of a theory generating its own interpretation. (See also Tarski[49] and Gödel[50]) To discuss this possibility we must relax our definition of theory.

Is it then reasonable to expect a mathematical formalism to suggest its own interpretation? Historically, it appears that it is people who make the interpretation. For example, the Lorentz transformations did not suggest their own interpretation. Einstein did,[29, 51] after a careful discussion of space and time measurements. One might also recall the misidentification of the energy and momentum of an electron.[52] This led to an extra unwanted contribution to the rest mass; a non-relativistic energy-momentum vector, and a lot of wasted paper. Even now this error still plagues some texts on classical electromagnetic theory. So also the case of the uniformly accelerating electron! But the trouble is surely due to people, and not to the theory generating a mistaken interpretation. Though perhaps left to itself the theory would have generated a correct interpretation?

The Klein-Gordon equation for a mass zero particle is the same in appearance as the usual equation for sound waves (in the first approximation). Both look similar to the equation for the electrostatic potential in a vacuum, in the appropriate guage. There really is, of course, a relationship between the latter and the Klein-Gordon equation, though there are considerable differences between the interpretation (at least that I would make) of the Klein-Gordon equation for one particle, and that of a classical (many photon) electromagnetic field (see Appendix A).

Thus I repeat that the *same* mathematical formalism may be part of different theories: that a theory must include a "meta" physics; that is, an interpretation.

If the expanding worlds are unmeasureable in principle, this interpretation is unnecessary. It reduces in practice to the statistical interpretation. The physicists usual choice would, therefore, be (Ockham's razor) to dispense with it. However, we will do well to be wary of this type of argument since it has also been advanced in the past against other theories; against Avogadro's atomic theory of matter; against Pauli's neutrino hypothesis. Dewitt[28] has suggested that this interpretation of quantum mechanics could have cosmological implications. Should this be the case, then obviously it would be of interest. For the present I shall leave it to the cosmologists and the mind expanders.

## X. THE COLLAPSING WAVE FUNCTION

It would appear, therefore, that I disagree with those (whom I will call C's) who interpret a measurement as causing, or in some manner necessitating, a collapse in the wave function, and, at the same time, with those (whom I will call E's) who, in contrast, wish never to collapse it. This apparent contradiction must be squarely faced. It must be explicated thoroughly in what sense I do *not* consider that a measurement requires one to collapse the wave function, although collapsing it does not lead to contradictory assessments in actual experiments (even though it may lead to contradictory ideas).

C's and E's are not considering the same type of situation.

E's are working on a totally unrestricted scale: on an all-inclusive program. This is largely alien to scientific procedure[53] and philosophy. It is not just a matter of scale: cosmology is not outside the scientists view. Nor is it that scientists do not try to combine various disciplines and branches of knowledge: for example, attempts such as Dirac's, to quantize General Relativity, or Einstein's to combine General Relativity and Electromagnetic Theory into a Unified Field Theory. It is that even the more wide-ranging (and difficult!) theoretical attempts are limited in the aspects of the world which they attempt to mirror conceptually. In experiments the restrictions are even more evident. Not only do scientists try to isolate some particular objects and interactions and exclude as much (disturbance!) as possible, but also that any experiment is restricted in space and time. The E's, therefore, face their own ontological and teleological difficulties which cannot be identified with any *finite* experimental set-up, no matter how complex or massive.

It is in the restricted context that it is possible to make clear that there is no necessity to consider that the wave function collapses when a measurement (an observation) is made.

Let us first consider an extremely simple set up. A system is prepared on which two measurements are to be made. One measurement is in location $L_1$ (of some small extension in space), the other is in location $L_2$. We will label the time coordinate at $L_1$ by t and at $L_2$ by T. If we say a measurement occurs at a particular time t, we shall mean within some (specified) time interval

$t - \Delta t$, $t + \Delta t$. Similarly, for a particular T. A measurement at $(L_1, t)$ will be recorded as either A or not-A. For example, the arrival, or the reverse, of an electron at $L_1$, the appearance of a bubble in a bubble chamber or the reverse, etc. Similarly, at $(L_2, T)$ it will be B or not-B. Let us now imagine we do a quantum mechanical calculation for these four possible results of the experiment

$$P_{11} = \text{Prob.} \quad (A,t \; ; \; B,T) \tag{W1}$$

$$P_{12} = \text{Prob.} \quad (A,t \; ; \; \text{not-}B,T) \tag{W2}$$

$$P_{21} = \text{Prob.} \quad (\text{not-}A,t \; ; \; B,T) \tag{W3}$$

$$P_{22} = \text{Prob.} \quad (\text{not-}A,t \; ; \; \text{not-}B,T) \tag{W4}$$

Then it follows that

$$\text{Prob. } (B,T) = P_{11} + P_{21} \tag{W5}$$

$$\text{Prob. } (A,t) = P_{11} + P_{12} \tag{W6}$$

and also that

$$\text{Prob. } (BT, \text{ given } A,t) = \frac{P_{11}}{P_{11} + P_{12}} \tag{W7}$$

Supposing that $P_{22}$ is very much larger than $P_{11}$, $P_{12}$, $P_{21}$ (a very likely assumption) and the latter are the same order of magnitude as each other, there is nothing abstruse about the fact that (W7) gives higher probability for the

occurence of (B,T) than does (W5), or that if $P_{12} = 0$, (W 7) gives (B,T) as "certain." Nor does it matter whether we are predicting (that is, that $(L_2,T)$ lies within the light cone of $(L_1,t)$), or retrodicting, or if $(L_1,t)$, $(L_2T)$ are space-like with respect to each other. Nor does it alter the argument if in our original calculation we included in the Hamiltonian terms for the interaction that might be produced by the measurement at $L_1$ and $L_2$. We are only formulating our calculated probabilities in different ways, there is no need to "collapse" the wave function. (W7) justifies practical calculations using a collapsed wave function but it is not required that we do so. This is nicely illustrated by the calculation originally done by Mott[54] and to be found in Schiff[55] (Section 30) where he calculates a cloud chamber track *without* considering a collapse in the wave function.

The E.P.R. experiment may be similarly discussed where A, not-A are replaced by particle with spin-up, particle with spin-down, or the non-arrival of any particle. In general, a similar analysis without any wave-function reduction may be made of experiments involving many measuring locations, times and complex interactions *if* a finite time allows us to record all the results. Only when we repeat this sequence again and again will the *probabilities* assume any appearance of reality. Nearly all authors sound as if they mean the same thing when they initially discuss calculating quantum mechanical probabilities; it is only in ascribing some sort of intermediate reality to the wave function that controversial differences

become apparent. Actually, if the measurements in an experiment do not have some sort of objective validity it seems difficult to ascribe any meaning at all to precedently predicted probabilities for their occurence.

Jauch[56] resolves the EPR paradox (and the cat paradox *without* consciousness involved, after changing it to a mechanical situation) by saying that it "originates in our habit of thinking that the states of two sub-systems determine uniquely the state of the composite system." Now there is no fault in this, but I don't think it illuminates the problem of comparing state preparation and measurement. Jauch also specifically excludes anything that involves human consciousness because "a characterization, and analysis of 'consciousness' in physical terms" is "a task which seems to transcend the present limitations of physics." This seems excessive (and no answer to Wigner's dilemma) in the sense that, whereas I prefer an interpretation that does not explicitly depend on human consciousness,[57] yet without human consciousness around all these deliberations are pointless. That science without scientists is meaningless is not a solipsist position.

## XI. THE STATISTICAL INTERPRETATION

This is a perfectly comfortable and non-contradictory interpretation which so far appears to fit all experiments. It also matches the actual way we approach experiments which involve quantum mechanics.

However, it does leave open, as Einstein intended, the

possibility that there are "hidden" variables, and that the world is really deterministic.

For a long while this appeared to be countered by a theorem of Von Neumann[18] (1932) which seemed to indicate that no hidden variable theory could be consistent with the statistical predictions of quantum mechanics. In 1952 this was shown to be false by Bohm.[58] He actually constructed a theory which did just that. It was not until 1966 that this contradiction was satisfactorily analysed.[59] There was nothing wrong with Von Neumann's mathematics. It is simply that one of his assumptions can never be satisfied by a dispersion-free state. (See Appendix B). Inglis' argument that "Quantum mechanics is so broadly successful and convincing that the quest (for hidden variables) does not seem hopeful," seems, therefore, to have less substance; and it might still seem reasonable to consider hidden variable theories.

Hidden Variable Theories

From the inception of quantum mechanics to the present day there have been suggestions that the probabilistic predictions of quantum mechanics are not the end of the story but the superficial appearance of an underlying deterministic situation: that there are hidden variables that, if we but knew them, would enable us to predict the exact result of single experiments; and that the quantum mechanical predictions for a sequence of measurements come from averaging over these unknown variables. In 1926 De Broglie[60] and, separately, Madelung[61] proposed this kind of interpretation of quantum

mechanics. De Broglie, partly because of critical objections at the Solvay Conference in 1927, gave up his attempts for a long time. As already mentioned, in 1952 Bohm proposed a hidden variable theory in agreement with quantum mechanics. This may have had less impact simply *because* it agreed with quantum mechanics and, therefore, could be ignored as adding no substance until or unless it would predict some different experimental result. However, if one theory could agree with quantum mechanics, it removed the apparent roadblock of Von Neumann's theorem. Other proposals,[62, 63] including another by de Broglie,[64] were made subsequently.

In 1964 Bell[65] proved a very interesting theorem which affects the outlook for hidden variable theories. He showed that any theory which does satisfy the localizability assumption of Section VI (that is, Proposition 2) will predict some *experimental* consequences that are *different* from those of quantum mechanics.

In his original paper, he considers the decay of a singlet into two like particles of spin 1/2. As in the Bohm version of EPR, there are two magnets A and B far from each other and from the approximate position of the decay. A is to measure the spin of one particle in direction $\vec{a}$, and B of the other in direction $\vec{b}$. If $\vec{\sigma}_1$ and $\vec{\sigma}_2$ are the Pauli spin matrices for the two particles then we can express the quantum mechanical prediction as

$$\left\langle \vec{\sigma}_1 \cdot \vec{a}\, \vec{\sigma}_2 \cdot \vec{b} \right\rangle = - \vec{a} \cdot \vec{b} \tag{3}$$

That is, though a measurement of $\vec{\sigma}_1 \cdot \vec{a}$ will be either +1 or -1 and so will that of $\vec{\sigma}_2 \cdot \vec{b}$, there will be a statistical correlation between these two measurements (each, of course, made on the same decay) varying from total if $\vec{a}$ and $\vec{b}$ are in the same direction to zero at right angles. Bell showed that no theory that satisfied the localizability assumption could duplicate this correlation at all angles. If $\overline{P}(\vec{a},\vec{b})$ is the average value of the product of the measurements performed when

$$|\overline{\vec{a} \cdot \vec{b}} - \vec{a} \cdot \vec{b}| \leqslant \delta \tag{4}$$

and $\overline{P}(\vec{a},\vec{b})$ differs from the quantum mechanical value (3) by less than $\varepsilon$ it will follow that

$$4(\varepsilon + \delta) \geqslant \sqrt{2} - 1 \tag{5}$$

for some orientation.

Clauser, et al., showed that Bells theorem could be extended to more realizable experimental situations such as the two photon correlation predicted in the decay of a singlet → triplet → singlet. S. J. Freedman's[67, 68] results showed that the quantum mechanical prediction held to sufficient accuracy to exclude hidden variable theories satisfying the localizability condition. (Although Holt's[69] experiment seems in disagreement with this.)

## XII. CONCLUSION

Where does this leave us? If we accept the statistical interpretation of quantum mechanics, then quantum mechanics is a very strong position. Empirically, it works and, ironically, the very arguments that Einstein used in the hope of making quantum mechanics less tenable (i.e., that two different measurements that are physically separated do not affect each other) appears to make disprovable a physically reasonable set of hidden variable theories. Before the final sentences, I must re-emphasize that, from the arguments in this paper, events and measurements do not seem to be fundamentally separated. In addition, for re-emphasis, there are two types of "events" we do not measure. The first type I would label as "non-events." These "non-events" are of the sort that, if we tried to measure them, our whole physical set-up would be altered. For example, in a two-slit diffraction pattern, if we try to measure which slit the particle passed through, we find the diffraction pattern is destroyed. The other class of events are ones that might, in principle, be measureable (without additional disturbance), but which we do not actually measure for practical reasons. For example, a beam in a less than perfect vacuum makes material encounters. If these are unmeasured, we consider them random and introduce ordinary statistics (thus, use the density matrix) to allow for their average, general-statistical effect. In "pure" quantum mechanics, every event is equivalent in principle to a measurement. I mean that it is *logical*

in this interpretation not to separate, in a fundamental way, events and measurements, though it may not necessarily prove correct not to make this separation.

Put thus explicitly, this position makes also more explicit our lack of understanding of transitions from the microscopic to the macroscopic, but I do not think it need detract from our admiration for the successes of quantum mechanics. Nor does it seem necessary to me that because we give up the question, "What was objectively there before I made the measurement?" that it then follows that we should give up *all* our ideas of what is "real" or objective.

It remains open, naturally, that some theoretician may think up, or have accidentally thrust upon them, a completely new theory for which quantum mechanics is an approximation, or some sort of limit; perhaps in the way that classical mechanics appears to be for quantum mechanics. Quantum mechanics may yet be shown to make some objectively false predictions.

# APPENDIX A

## The Semiclassical Approximation

Classical electromagnetic field theory is usually used in macroscopic calculations. That is, in cases where we are describing a state, or rather a mixture of a lot of states (except in laser experiments) with a large number of photons present and these states are, consequently, not much changed by the presence or absence of a single photon. In the semiclassical situation, we are faced with a very different problem: typically a system consisting of a single electron, or atom, and one, or no, photon. A reasonable (though not logically necessary) attitude would then be to treat the interpretation of an electromagnetic wave in the same manner as the interpretation of the wave equation for an electron.

The classical equivalent of an electron as a particle does not exist for the photon. In using a wave formulation (Maxwell's equations), our Newtonian photon has already been "first quantized." We don't know it, only because $\hbar$ does not appear in the relativistic quantum mechanics equation for a "particle" with zero rest mass, and, there is no non-relativistic equation for such a particle. Thus, the "particle" nature of light appears only in "second quantization" -- that is, in quantum field theory in which also reappears the "particle" nature of the electron. The above also indicates clearly that, in order to explain an experiment involving an atom (or electron) and a photon which produce a non-classical result (i.e., one in which $\hbar$

is involved as well as other fundamental constants), either we must "first quantize" the electron or "second quantize" the photon. In a particular effect it may, in practice, be simpler to choose one than it is to choose the other.

There are several reasons why we tend not to equate the two: 1) one can be treated non-relativistically, and one cannot, 2) an electron is a fermion and a photon is a boson, and 3) perhaps most important is the fact that in the Lagrangian of the quantum field theoretical formulation, the interaction term between the two is quadratic in the electron field, but linear in the photon field. This means that in first order perturbation theory we do not need to create or to annihilate the electron, but must create or annhilate a photon.

I conclude, therefore, that we may sometimes find it more convenient to "second quantize" the photon, yet only "first quantize" the electron, but that, in considering the decay of one atom, we should use a statistical quantum mechanical *interpretation* of the "emitted" electromagnetic wave.

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0 0 0 0 4 1 0 8 0 2 5



## APPENDIX B

Dispensionless States?

Let $|a\rangle$ represent the spin-vector for a spin 1/2 particle. The assumption, let us number it(B1), says:

$$\langle a|(\sigma_x + \sigma)|a\rangle = \langle a|\sigma_x|a\rangle + \langle a|\sigma_z|a\rangle \qquad \text{(B1)}$$

but, if $|a\rangle$ is associated with an individual physical system, there are conceptual difficulties. For simplicity, let $|a\rangle$ represent a particle with spin up in the z-direction. $|a\rangle = |z\uparrow\rangle$

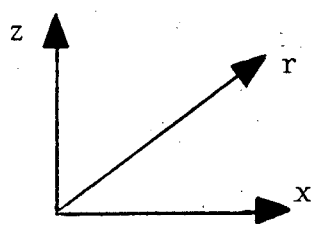


Fig. II

The r-direction is as shown in Fig. II. If we set up the magnet in the z-direction, we measure $\sigma_z$ to be + 1. If, instead, we set up a magnet in the x-direction, we would measure $\sigma_x$ to be either + 1 or - 1 (with equal probabilities). If, instead, we set up a magnet in the r-direction, we would measure $(\sigma_x + \sigma_y)$ to be $\pm\sqrt{2}$ (with unequal probabilities). There is no way we can visualize making all three

measurements on the same particle. If the measurements are made sequentially on the same particle, then our possible results depend on the order of the sequence.

Thus, this particular assumption of Von Neumann's requires that $|a\rangle$ *should not be associated with an individual physical system.*

To put it in more familiar words, (B1) equates the expectation value for a spin measurement in the r-direction to the sum of the expectation values of spin measurements in the x-direction and the z-direction. To satisfy this we are weighing the *possible* results of different measurements in *different* experiments, by a probability. We are back to a statistical interpretation.

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"But the phase suggests this is only one of several

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He seems to consider that new interpretations mean hidden variable theories:

> "Most of us would feel convinced that it would not prove possible to graft onto quantum mechanics some new kind of interpretation without changing the results, that is the observable predictions of the theory. . . . cannot make such an alternative interpretation a causal one in the deterministic sense."

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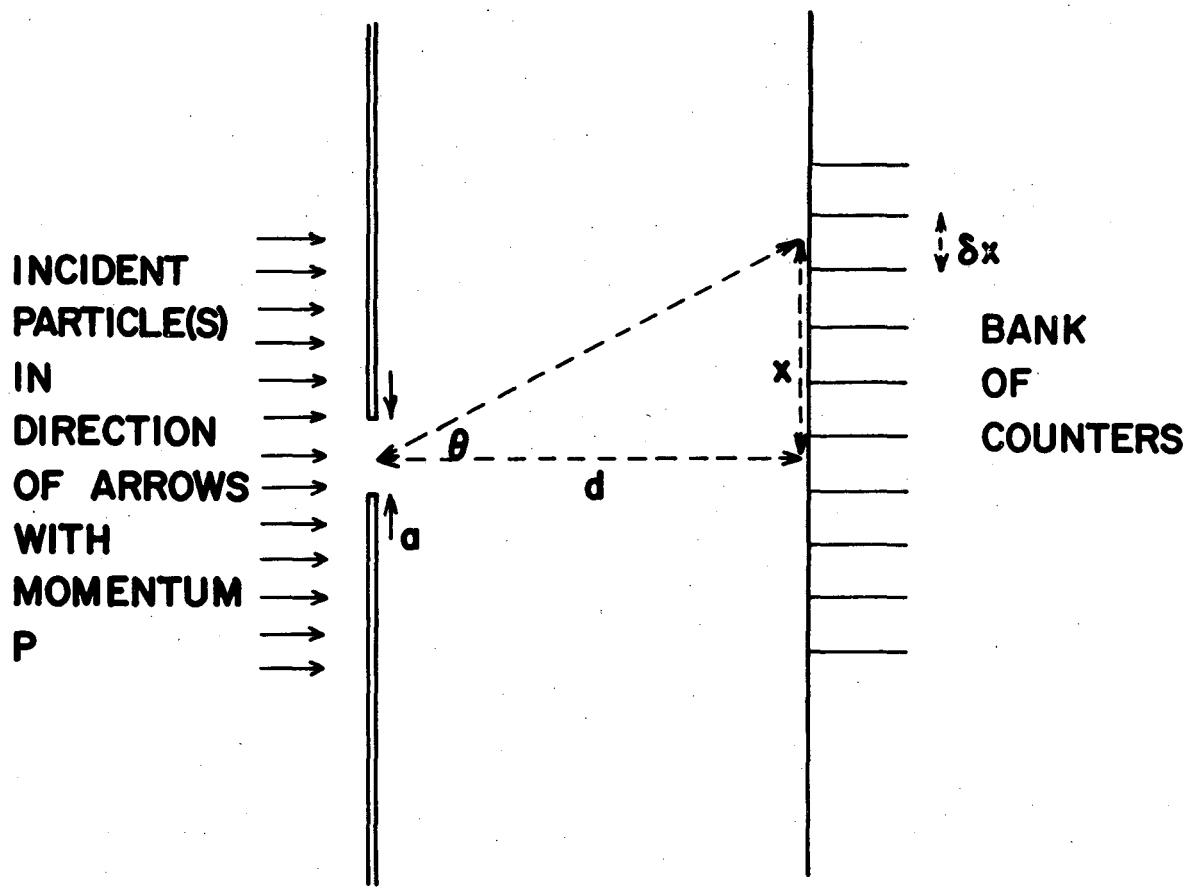


XBL 7410–7964

**FIGURE 1**

Single Slit diffraction pattern.

0 0 0 0 4 1 0 6 0 3 0

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