Chapter 1

Quantum Mechanics without "The Observer"

Karl R. Popper

Department of Philosophy, L.S.E., University of London, Great Britain

This is an attempt to exorcize the ghost called "consciousness" or "the observer" from quantum mechanics, and to show that quantum mechanics is as "objective" a theory as, say, classical statistical mechanics. My thesis is that the observer, or better, the experimentalist, plays in quantum theory exactly the same role as in classical physics. *His task is to test the theory.*

The opposite view, usually called the *Copenhagen interpretation of quantum mechanics*, is almost universally accepted. In brief it says that *"objective reality has evaporated", and that quantum mechanics does not represent particles, but rather our knowledge, our observations, or our consciousness, of particles.* (Cp. [*28*], p. 100.)

If a mere philosopher like myself opposes a ruling dogma such as this, he must expect not only retaliation, but even derision and contempt. He may well be browbeaten (though I am happy to remember how kindly and patiently I was treated by NIELS BOHR) with the assertion that *all competent physicists know that the Copenhagen interpretation is correct* (since it has been "proved by experiment").

It seems therefore necessary to point out that this assertion is historically mistaken, by referring to physicists who like EINSTEIN, PLANCK, VON LAUE, or SCHRÖDINGER, are as competent as any, and who (unlike EINSTEIN, PLANCK, VON LAUE, and SCHRÖDINGER) were even at one time fully convinced adherents to the Copenhagen interpretation, but who do not now "regard the new interpretation as conclusive or convincing" as HEISENBERG puts it (in [*27*], p. 16).

There is, first, LOUIS DE BROGLIE, a one-time adherent to the Copenhagen interpretation; and his former pupil, JEAN-PIERRE VIGIER.

There is, next, ALFRED LANDÉ, also one of the great founders of quantum theory in the years 1921 to 1924 who later (1937 and 1951) wrote two textbooks on quantum mechanics entirely in the Copenhagen spirit, but who has more recently ([*36, 37, 38*]) become one of the leading opponents of the Copenhagen interpretation.

There is DAVID BOHM who published in 1951 a textbook, *Quantum Theory* [*1*], which was not only orthodox in the Copenhagen sense but one of the clearest and fullest, most penetrating and critical presentations of the Copenhagen point of view ever published. Shortly afterwards, under the influence of EINSTEIN, he tried new ways, and arrived in 1952 [*2*] at a tentative theory (revised in [*2a*]) whose logical consistency proved the falsity of the constantly repeated dogma (due to VON NEUMANN [*46*]) that the quantum theory is *"complete"* in the sense that it must prove incompatible with any more detailed theory.

There is MARIO BUNGE who in 1955 published a paper, "Strife about Complementarity" [*12*].

There is the German physicist, FRITZ BOPP, who explicitly subscribes to the Copenhagen interpretation, in an epistemological paragraph of a most interesting paper; thus he writes, for example, "Naturally our considerations do not mean any alteration of the mathematical concept of complementarity." (Cp. [*8*], pp. 147f.) Yet he develops there (and in previous publications) a theory with which EINSTEIN would hardly have had any quarrel since, on lines not dissimilar to EINSTEIN'S (cp. [*19*], pp. 671 f.), BOPP interprets the quantum theoretical formalism as an extension of classical statistical mechanics; that is, as a theory of ensembles.

I have given this brief and of course quite incomplete list of dissenters merely to combat the historical myth that only philosophers (and totally incompetent or senile physicists) can doubt the Copenhagen interpretation. But before proceeding to criticize this interpretation in some detail, I should like to discuss two points.

(a) In a very important sense, which to my knowledge has been usually overlooked, the Copenhagen interpretation ceased to exist long ago.

(b) Most physicists who quite honestly believe in it do not pay any attention to it in actual practice.

As to point (a), we must not forget that "the new quantum theory" or "quantum mechanics" was, to start with, and until at least 1935, simply another name for *"the new electromagnetic theory of matter"*.

In order to realize fully how the theory of the atom, and therefore the theory of matter, were identified with the theory of the electromagnetic field, we may for example turn to EINSTEIN, who said in 1920: "... according to our present conceptions the *elementary particles are ... nothing but condensations of the electromagnetic field* .... Our ... view of the universe presents two realities ..., namely, gravitational ether and electromagnetic field, or — as they might also be called — space and matter." (Cp. [*17*], p. 22. The italics are mine.)

Quantum mechanics was regarded by its adherents as *the final form of this electromagnetic theory of matter*. That is to say, the formalism was regarded, first of all, as *the theory of electrons and protons* and thereby as *the theory of the constitution of atoms: of the periodic system of elements and their physical properties: and of the chemical bond, and thus of the physical and chemical properties of matter.*

A very impressive statement of the view held by almost all physicists at least up to the discovery of the positron in 1932 is due to ROBERT A. MILLIKAN:

"Indeed, nothing more beautifully simplifying has ever happened in the history of science than the whole series of discoveries culminating about 1914 which finally brought practically universal acceptance to the theory that the material world contains but two fundamental entities, namely, positive and negative electrons, exactly alike in charge, but differing widely in mass, the positive electron — now usually called a *proton* — being 1850 times heavier than the negative, now usually called simply the *electron*." ([*44*], p. 46; the italics are mine. Cp. also [*43*], p. 377.)

In fact until at least 1935 some of the greatest physicists (cp. EDDINGTON'S [*16*]) believed that, with the advent of quantum mechanics, *the electromagnetic theory had entered into its final state*, and that the results of quantum mechanics strongly confirmed that *all matter consisted of electrons and protons*. (Neutrons and neutrinos had also been admitted, somewhat grudgingly, but it was thought that neutrons were protons + electrons; and that neutrinos might not be much more than a mathematical fiction; while positrons were regarded as "holes" in the sea of electrons.)

This theory that matter consists of protons and electrons died long ago. Its ailment (though it first remained hidden) started with the discovery of the neutron and also of the positron (which the Copenhagen authorities refused to believe in at first): and it received its final blow with the discovery of the sharply distinct *levels of interaction*, of which the electromagnetic forces constitute just one among at least four:

1. Nuclear forces.
2. Electromagnetic forces.
3. Weak decay interactions.
4. Gravitational forces.

Moreover, the hope of solving within quantum mechanics such classical problems of the electromagnetic theory as the explanation of the electronic charge has been practically abandoned.

In the light of this situation, we may now look back upon the titanic struggle between EINSTEIN and BOHR. The problem posed by EINSTEIN was whether quantum mechanics was "*complete*". EINSTEIN said no. (Cp. [*21*].) BOHR said yes.

I have no doubt that EINSTEIN was right. But even today we can read that it was BOHR who won that famous battle. This view persists largely because EINSTEIN'S attack upon BOHR'S assertion of the *completeness* of quantum mechanics was interpreted by the Copenhagen school as an attack upon *quantum mechanics itself and its "soundness" or consistency.* But this entails that we accept (i) the identification of the Copenhagen interpretation with the quantum theory, and (ii) BOHR'S shift of the problem from *completeness* to *soundness* (= *freedom from contradiction*). Yet as EINSTEIN had offered his own (statistical) interpretation of quantum theory, he clearly accepted its consistency.

As to point (b), that is, as to my assertion that most physicists who honestly believe in the Copenhagen interpretation do not pay any attention to it in actual practice, an excellent example is FRITZ BOPP [*8*], since he believes (as do EINSTEIN, PODOLSKY, and ROSEN) that particles possess both sharp positions and momenta at the same time, while the Copenhagen school believes this to be false, or "meaningless", or "unphysical". To quote a formulation of LANDÉ'S of 1951 (before he turned against the Copenhagen interpretation): "The classical idea of particles breaks down under the impact of the uncertainty relations. It is unphysical to accept the idea that there *are* particles possessing definite positions and momenta at any given time, and then to concede that these data can never be confirmed experimentally, as though by a malicious whim of nature." ([*39*], p. 42. LANDÉ continues by quoting NIELS BOHR [*6*].) But what I have mainly in mind in connection with my point (b) is this. Admittedly, the formalism of quantum mechanics is still applied by physicists to the old problems, and its methods are, with many modifications, partly used in connection with the many new problems of nuclear theory and elementary particle theory. This is certainly a great credit to its power. Yet at the same time, most *experimentalists*, though much concerned with the limits of precision of their results, do not seem to be more worried about the role of the observer or about interfering with their results than they are in connection with sensitive classical experiments; and most *theorists* are quite clear that a new and much more general theory is needed: they all seem to be in search of *a really revolutionary new theory.*

In spite of all this, it still seems necessary *to discuss the Copenhagen interpretation;* that is, more precisely, the claim that, in atomic theory, we have to regard *"the observer"* or *"the subject"* as particularly important, because atomic theory takes its peculiar character largely from *the interference of the subject or the observer (and his "measuring agencies") with the physical object under investigation.* To quote a typical statement of BOHR'S: "Indeed, the *finite interaction between object and measuring agencies* ... entails the necessity of a final renunciation of the classical ideal ... and a radical revision of our attitude towards the problem of physical reality." (Cp. [*4*], pp. 232f.)

Similarly HEISENBERG: "... the traditional requirement of science ... permits a division of the world into subject and object (observer and observed) .... This assumption is not permissible in atomic physics; the interaction between observer and object causes uncontrollable large changes in the system [that is] being observed, because of the discontinuous changes characteristic of the atomic processes." (Cp. [*26*], pp. 2f.) Accordingly, HEISENBERG suggests that "it is now profitable to review the fundamental discussion, so important for epistemology, of the difficulty of separating the subjective and the objective aspects of the world". (Cp. [*26*], p. 65; see also [*46*], pp. 418–421.)

As opposed to all this I suggest that, in practice, physicists do their measurements and experiments today fundamentally in the same way as they did them before 1925. If there is an important difference, then it is that the degree of indirectness of measurements has increased as well as the degree of "objectivity": where 30 or 40 years ago physicists used to look through a microscope to take a "reading", there are now photographic films, or automatic counters, which do the "reading". And although a photographic film has to be "interpreted" (in the light of a theory), it is in no way physically "interfered with" or "influenced" by this interpretation. Admittedly, many experimental tests have now largely a statistical character, but this makes them no less "objective": their statistical character (often processed automatically by counters and computers) has nothing to do with the alleged intrusion of the observer, or of the subject, or of consciousness, into physics, although the preparation or setting up of an experiment obviously has: *it depends on theory*.

Our *theories* which guide us in setting up our experiments have of course always been our inventions: they are inventions or products of our "consciousness". But that has nothing to do with the scientific status of our theories which depends on factors such as their simplicity, symmetry, and explanatory power, and the way they have stood up to critical discussion and to crucial experimental tests; and on their truth (correspondence to reality), or nearness to truth. (Cp. [*49*], ch. 10.)

Perhaps this is the best place to insert a few logical remarks on the *distinction between theories and concepts;* remarks which, although what follows does not depend on them, may yet help to remove some obstacles that block the way to a critical understanding of the situation in quantum theory.

What we are seeking, in science, are *true theories* — true statements, true descriptions of certain structural properties of the world we live in. These theories or systems of statements may have their instrumental use;

yet what we are seeking in science is not so much usefulness as *truth; approximation to truth; and understanding*.

Thus theories are described wrongly if they are described as being *nothing but* instruments (for example, instruments of prediction), though they are as a rule, among other things, also useful instruments. But infinitely more important for the scientist than the question of the usefulness of theories is that of their *objective truth*, or their nearness to the truth, and the kind of *understanding* of the world, and of its problems, which they may open up for us. The view that theories are *nothing but* instruments, or calculating devices (cp. [*49*], chapter 3), has become fashionable among quantum theorists, owing to the Copenhagen doctrine that quantum theory is *intrinsically ununderstandable* because we can understand only *classical "pictures"*, such as "particle pictures" or "wave pictures". I think this is a mistaken and even a vicious doctrine.

Theories are also described quite wrongly as "conceptual systems" or "conceptual frameworks". It is true that we cannot construct theories without using words or, if the term is preferred, "concepts". But it is most important to distinguish between statements and words, and between theories and concepts. And it is important to realize that it is a mistake to think that a theory T_1 is *bound* to use a certain conceptual system C_1: *one* theory T_1 may be formulated in many ways, and may use many different conceptual systems, say C_1 and C_2. Or to put it another way: two theories, T_1 and T_2, should be regarded as one if they are logically equivalent, even though they may use two totally different "conceptual systems" (C_1 and C_2) or are conceived in totally different "conceptual frameworks". I do not happen to believe that SCHRÖDINGER [*59*] and ECKART [*15*] have validly established the full logical equivalence of wave mechanics and matrix mechanics: there are some loopholes in these equivalence proofs. In this point I agree with NORWOOD RUSSELL HANSON'S [*25*] (and E. L. HILL'S [*30*]), although some of my views on the logic of the equivalence or identity of theories differ somewhat from HANSON'S.

Yet I do not think that such a proof is impossible, *in spite of the great difference between the conceptual frameworks of the two theories*. (What would be needed for a valid proof is something approaching an axiomatization of both theories, and a proof that to every theorem $t_{1,n}$ of T_1 corresponds a theorem $t_{2,n}$ of T_2 such that, with the help of some system of definitions of the concepts of T_1 and of T_2 we can show that $t_{1,n}$ and $t_{2,n}$ are logically equivalent. It would not be necessary for either T_1 or T_2 itself to contain the means needed for formulating these definitions; for these means may be supplied by some *extensions* of the theories. Incidentally, the fact that definitions may be needed for such an equivalence proof does not mean that they are needed within a physical theory.)

Now since theories *can* be equivalent even though their "underlying" conceptual frameworks are utterly different (there are many other examples showing that this may be possible), it is clearly a mistake to identify a theory with its "underlying" conceptual framework or even to believe that these two *must* be very closely related. The conceptual framework of a theory *may* be replaced by a very different one without changing the theory essentially; and *vice versa:* incompatible theories *may* be expressed within the same conceptual framework. (For example, if we replace NEWTON's inverse square law by an inverse law with the power 2.0001, then we have a different theory within the same framework; and the difference will increase if the difference between the two parameters becomes greater. We might even introduce into NEWTON's theory a finite velocity for gravitational interactions and still say that we are operating within the same conceptual framework. If the velocity is very great, the two theories may be experimentally indistinguishable; if it is small, the theories may differ widely in their empirical implications, though still remaining within the same conceptual framework.)

What is of real importance for the pure scientist is the *theory.* And the theory is not merely an "instrument" for him, it is more: he is interested in its truth, or in its approximation to the truth. (Cp. [*49*], chapter 10.) The conceptual system, on the other hand, is exchangeable and is one among several possible instruments that may be used for formulating the theory. It provides merely a language for the theory; perhaps a better and simpler language than another, perhaps not. In any case, it remains (like every language) to some extent vague and ambiguous. It cannot be made "precise": the meaning of concepts cannot, essentially, be laid down by any definition, whether formal, operational, or ostensive. Any attempt to make the meaning of the conceptual system "precise" by way of definitions must lead to an infinite regress, and to merely *apparent* precision, which is the worst form of imprecision because it is the most deceptive form. (This holds even for pure mathematics.)

Thus we are ultimately interested in theories and in their truth, rather than in concepts and their meaning.

This point, however, is rarely seen. HEINRICH HERTZ said (and WITTGENSTEIN repeated it) that in science we make ourselves "*pictures*" (*„Bilder"*) of the facts, or of reality; and he said that we choose our "pictures" in such a way that "the logically necessary consequences" (*„die denknotwendigen Folgen"*) of the "pictures" agree with "the necessary natural consequences" (*„die naturnotwendigen Folgen"*) of the real objects or facts. Here it is left open whether the "pictures" are *theories* or *concepts*. MACH, in discussing HERTZ (cp. [*41*], p. 318), suggested that we should interpret HERTZ's "pictures" as "concepts". BOHR's view seems to be similar when he speaks (as he so often does) of the "*particle* *picture*" and the "*wave picture*"; in fact, his way of speaking indicates strongly the (at least indirect) influence of HERTZ and MACH.

But "pictures" are unimportant. They are especially unimportant if they are more or less synonymous with "concepts", and almost as unimportant when they are meant to characterize theories. *A theory is not a picture.* It need not be "understood" by way of "visual images": *we understand a theory if we understand the problem which it is designed to solve, and the way in which it solves it better, or worse, than its competitors.* Some people may combine this kind of understanding with visual images, others may not. But the most vivid visualization does not amount to an understanding of a theory unless these other conditions are realized: an understanding of the problem situation, and of the arguments for and against the competing theories.

These considerations are important because of endless talk about the "particle picture" and the "wave picture" and their alleged "duality" or "complementarity", and about the alleged necessity, asserted by BOHR, of using "classical pictures" because of the (admitted but irrelevant) difficulty, or perhaps impossibility, of "visualizing" and thus "understanding" atomic objects. But *this* kind of "understanding" is of little value; and the denial that we can understand quantum theory has had the most appalling repercussions, both on the teaching and on the real understanding of the theory.

In fact, all this talk about pictures has not the slightest bearing on either physics, or physical theories, or the understanding of physical theories. And the fashionable thesis that it is vain to try to "understand" modern physical theories because they are essentially "ununderstandable" (though useful instruments for calculation) amounts to the somewhat absurd assertion that we cannot know what problems they are intended to solve, or why they solve them better, or worse, than their competitors.

If *concepts* are comparatively unimportant, *definitions* must also be unimportant. Thus although I am pleading here for *realism* in physics, I do not intend to define "realism" or "reality". In pleading for realism I wish, in the main, to argue that nothing has changed since GALILEO or NEWTON or FARADAY concerning the status or the role of the "observer" or of our "consciousness" or of our "information" in physics. I am at the same time quite ready to point out that even in NEWTON's physics, "space" was somewhat less real than "matter" (because although it acted upon matter it could not be acted upon); and that in EINSTEIN's special theory of relativity an inertial frame was less real than a spatio-temporal coincidence of two events, or the spatio-temporal distance between them. In a similar way, the number of degrees of freedom of a physical system is a more abstract idea, and perhaps less real, than the atoms or molecules constituting the system; but still, I should be opposed to saying that the degrees of freedom of a system are not real, that they are *nothing but* a conceptual device, and not a real *physical property of the system*. In other words, I do not intend to argue about words, including the word "real"; and by and large I regard as excellent LANDÉ's suggestion to call physically real what is "kickable" (and able to kick back if kicked) — though there are, I am inclined to think, degrees of kickability: we can't kick quasars, DAVID BOHM reminds me.

I have been in doubt whether I should not perhaps first analyse and criticize the central tenets of the Copenhagen interpretation, and then later show that a perfectly realistic interpretation of the theory is possible. I have decided to proceed differently. I am going to expound, in the form of thirteen theses and a summary, my own realistic interpretation, for what it is worth; and I shall criticize the Copenhagen interpretation as I go along. I am sure I shall shock many physicists who, after having reached my fourth, or at the most my sixth thesis, will stop reading this rubbish: it is to help them not to waste their time that I have decided to proceed as I do.

1. My first thesis concerns the most important thing for understanding quantum theory: the kind of *problems* which the theory is supposed to solve. These, I assert, are essentially *statistical problems*. (a) It was so with PLANCK's problem in 1899—1900 which led to his radiation formula. (b) It was so with EINSTEIN's photon hypothesis and his derivation of PLANCK's formula. (c) It was so (at least in part) with BOHR's problem of 1913 which led to his theory of spectral emissions: the explanation of the Rydberg-Ritz combination principle was, clearly, a statistical problem (especially after EINSTEIN's photon hypothesis had been proposed). Admittedly, there was a second problem, thought by BOHR to be *the* fundamental one: the problem of *atomic stability*, or of the "stationary state" of non-radiating electrons in the atom. BOHR "solved" this problem — by a postulate (of "quantum states" or "preferred orbits"). So far as there is any *explanatory* solution to this problem, it is due to wave mechanics; which *in the light of* BORN's *interpretation* means that it is due to the substitution of a statistical problem for a mechanical problem. (See below.) (d) It was so with the set of problems which were solved first by BOHR's most fruitful "principle of correspondence": these were, in the main, problems of the *intensities* of the emitted spectral lines. However, BOHR's correspondence arguments were largely qualitative or, at best, approximations. The central problem which led to the new quantum mechanics was to improve on this by obtaining exact statistical results.

However, this is not at all the way in which BOHR and his school looked at the problem. They did *not* look for a generalization of classical *statistical* mechanics, but rather for a "*generalization of classical [particle] mechanics* suited to allow for the existence of the quantum of action", as BOHR put it as late as 1948; a generalization of particle mechanics which would offer "a frame sufficiently wide to account for ... the characteristic features of *atomic stability which gave the first impetus to the development of quantum mechanics* ...". (Cp. [*5*], p. 316. The italics are mine.)

Most formulations of the *problem* of quantum mechanics which I have been able to find are similar, except perhaps those "inductivistic" ones that start from the experiments and look upon theory as "the attempt to classify and synthesize the results ... of scientific experiment" (cp. [*26*], p. 1, and [*29*]), as if the scientific experiments referred to were not, in the main, only the results of theoretical problems, and significant only because of their conflict with, or support of, some theory. (A similar inductivist attitude appears to be DIRAC's starting point, when he discusses "The Need for a Quantum Theory". (Cp. [*14*], pp. 1 ff.)

I should admit, however, that BOHR's (in my opinion mistaken) programme of reforming particle mechanics so as to solve the problem of atomic stability appeared to have some prospect of being successfully carried out between 1924 and 1926. I refer, of course, to LOUIS DE BROGLIE's doctoral thesis of 1923–1924 in which he applied to electrons the Einsteinian idea that photons were somehow "associated" with waves, and showed that BOHR's quantized "preferred orbits" (and with them, stability) could be explained by wave interference. This was without doubt one of the boldest, deepest, and most far-reaching ideas in this whole development.

DE BROGLIE's idea was, quite consciously, an inversion of EINSTEIN's idea of associating light quanta or photons with light waves. In EINSTEIN's theory, which thus was the model of DE BROGLIE's, light is emitted and absorbed in the form of "particles" or "light quanta" or "photons"; and thus in the form of things which have a pretty sharp spatio-temporal location, at least while they *interact with matter* by being emitted or absorbed. Light is, however, *propagated* like waves. The square of the amplitude of these waves determines, according to EINSTEIN, the density (that is, the statistical probability) of the photons; and the amplitude of the waves at the place where an atom (in an appropriate state) or a free electron is located determines the probability of the absorption of a photon.

However it was more than two years, during which DE BROGLIE's theory of electrons grew into SCHRÖDINGER's "wave mechanics", before MAX BORN applied to this new wave mechanics the statistical interpretation of the relationship between photons and light waves which we owe to EINSTEIN. MAX BORN himself says about his statistical interpretation of wave mechanics: "The solution ... was suggested by a remark of EINSTEIN'S about the connection between the wave theory of light and the photon hypothesis. The intensity [of course, what is meant is the square of the amplitude] of the light waves was to be a measure of the density of the photons or, more precisely, of the probability of photons being present." (Cp. [*10*], p. 104.)

Thus through BORN'S statistical interpretation of matter waves even the one problem of quantum theory which appeared not to be statistical — the problem of atomic stability — was reduced to, or replaced by, a statistical problem: BOHR'S quantized "preferred orbits" turned out to be those for which the *probability* of an electron's being found on them differed from zero.

All this is to support my thesis that *the problems of the new quantum theory were essentially of a statistical or probabilistic character.*

2. My second thesis is that *statistical questions demand, essentially, statistical answers*. Thus quantum mechanics must be, essentially, a statistical theory.

I believe that this argument (although its validity is by no means generally admitted) is perfectly straightforward and logically cogent. (The argument may be traced back to RICHARD VON MISES [*45*] and it has been beautifully illustrated by ALFRED LANDÉ; cp. [*36*], pp. 3f., and [*38*], pp. 27ff. and 39.)

Statistical conclusions cannot be obtained without statistical premises. And therefore answers to statistical questions cannot be obtained without a statistical theory.

Yet largely owing to the fact that the *problems* of the theory were not (and still often are not) seen to be statistical, other reasons were invented to explain the widely admitted statistical character of the theory.

Foremost among these reasons is the argument that it is our (necessary) *lack of knowledge* — especially the limitations to our knowledge discovered by HEISENBERG and formulated in his "*principle of indeterminacy*" or "*principle of uncertainty*" — which forces us to adopt a probabilistic, and consequently a statistical, theory. (This argument is criticized in my fifth thesis below.)

3. My third thesis is that it is this mistaken belief that we have to explain the probabilistic character of quantum theory by our (allegedly necessary) *lack of knowledge*, rather than by the statistical character of our problems, which has led to *the intrusion of the observer, or the subject, into quantum theory*. It has led to this intrusion because the view that a probabilistic theory is the result of lack of knowledge leads inescapably

to the *subjectivist interpretation of probability theory;* that is, to the view that the probability of an event measures the degree of somebody's (incomplete) knowledge of that event, or of his "belief" in it.

However, as I have tried to show for many years, it would be sheer magic if we were able to obtain knowledge — statistical knowledge — out of ignorance. (Cp. [*50, 53, 54, 55*].)

4. My fourth thesis is that, as a consequence, we are faced with what I shall call *the great quantum muddle.* (It seems to me that the only adherent to BOHR'S "Principle of Complementarity" who is free of this muddle — following almost exactly EINSTEIN'S despised ideas in a new garb — is FRITZ BOPP, in his paper [*8*].)

In order to explain this great muddle, I shall have to say a few words about statistical theories.

Every probabilistic or statistical theory assumes the following.

(a) Certain *events* (5 turning up) which happen to certain *elements* (dice) in certain *experimental situations* (being shaken in a beaker, and thrown on a table). These form the "population" for our statistics.

(b) Certain physical properties of these events, elements, and experimental situations; for example that the dice are of homogeneous material, and that only one of the six sides is marked with a "5"; and that the experimental situation permits a certain width of variation.

(c) A set of the *possible* events (*possible* under the experimental conditions), called the points in the *sample space* or the *probability space* (the notion stems from RICHARD VON MISES).

(d) A number associated with each point (or, in the case of a continuous sample space, with each region) of the sample space, determined by some mathematical function, called the distribution function. (The sum of these numbers is equal to 1; this can be achieved by some "normalization".) In the continuous case the distribution function is a density function.

Example: our sample space may be the United Kingdom, or more precisely, the set of events of a man or a woman living at some spot in the United Kingdom. The distribution function can be given by a (continuous) density distribution (normalized to 1) of the population; that is, the actual number of people living in a region, "normalized" by being divided by the total population of the United Kingdom. We then can say that this *information* helps us to answer all questions of the type: what is the probability that an Englishman lives at a certain spot (region); or that an Englishman lives in "the South of England"? (Here we assume that we have a proper division between North and South.)

Now it is clear that the statistical distribution function (whether normalized or not) may be looked upon as *a property characterizing the* *sample space* — in our case the United Kingdom. It is *not* a physical property characteristic of the *events* (5 turning up; or of Mr. Henry Smith's, a resident in the United Kingdom, being domiciled in Oxford); still less is it a property of the *elements* (the die; or Mr. Smith).

This is particularly clear of Mr. Smith: he is, for the statistical theory, nothing but an element under consideration. (In fact, the statistical theory will tell us almost the same about Mr. Smith as it tells us, say, about his bed or his wristwatch: the statistical distributions of these physically very different elements will be almost identical.) It is, perhaps, less clear of the die: in this case the distribution function is, we conjecture, *related* to its physical properties (its having six sides, the homogeneity of its material). However, this relation is not as close as it may seem at first sight. For the distribution function will be the same for big or small dice, and for dice made of some light plastic or uranium. And the probability of 5 turning up will be the same for all dice that have only one side marked "5" — whatever the markings of the other sides may be (though these may greatly influence other probabilities); and it will be a different one for all dice having more, or less, than one side marked "5", or for non-homogeneous dice.

Now what I call the great quantum muddle consists in taking a distribution function, i.e. a statistical measure function characterizing some *sample space* (or perhaps some "population" of events), and treating it as *a physical property of the elements of the population*. It *is* a muddle: the sample space has hardly anything to do with the elements.

Unfortunately many people, including physicists, talk as if the distribution function (or its mathematical form) were a property of the *elements* of the population under consideration. They do not discriminate between utterly different categories or types of things, and rely on the very unsafe assumption that "my" probability of living in the South of England is, like "my" age, one of "my" properties — perhaps one of my physical properties.

Now my thesis is that this muddle is widely prevalent in quantum theory, as is shown by those who speak of a "duality of particle and wave" or of "wavicles".

For the so-called "wave" — the ψ-function — may be identified with the mathematical form of a function, $f\left(P, \frac{d}{dt} P\right)$, which is a *function of a probabilistic distribution function* P, where $f=\psi=\psi(q, t)$, and $P=|\psi|^2$ is a density distribution function. (See, for example, the footnote 6, with a reference to E. FEENBERG, in H. MEHLBERG'S excellent discussion of LANDÉ'S views in [42], p. 363.) On the other hand, the *element* in question has the properties of a particle. The wave shape (in configuration space) of the ψ-function is a kind of accident which poses a problem to probability theory, but which has next to nothing to do with the physical properties of the particles. It is as if I were called a "Gauss-man" or a "non-Gauss-man" in order to indicate that the distribution function of my living in the South of England has a Gaussian or non-Gaussian shape (in an appropriate sample space).

5. My fifth thesis concerns HEISENBERG'S famous formulae:

$$\Delta E \Delta t \geqq h, \tag{1}$$

$$\Delta p_x \Delta q_x \geqq h. \tag{2}$$

I assert that these formulae are, beyond all doubt, validly derivable *statistical formulae* of the quantum theory. But I also assert that they have been *habitually misinterpreted* by those quantum theorists who said that these formulae can be interpreted as determining some upper limits to the *precision of our measurements* (or some lower limits to their imprecision).

My thesis is that these formulae set some lower limits to the *statistical dispersion or "scatter"* of the results of sequences of experiments: they are *statistical scatter relations*. They thereby limit the precision of certain individual *predictions*.

But I also assert that *in order to test these scatter relations, we have to be able (and are able) to make measurements which are far more precise than the range or width of the scatter.*

The situation is like this: a statistical theory may tell us something about the distribution or scatter of the population in the environment of industrial towns. In order to *test* it, it will be necessary to fix the places where people live with a precision far exceeding the range of the predicted scatter. Our statistical laws may tell us that we cannot reduce the scatter below a certain limit. But to conclude from this that we are unable to "measure" the positions of the places where the people live more precisely than the minimum statistical scatter is simply a muddle.

Since HEISENBERG'S formulae in their various proper interpretations are (as will be shown in detail in my next thesis) *statistical laws of nature, derivable from a statistical theory*, it is quite obvious that it is impossible to use them in order to explain why quantum mechanics is probabilistic or statistical. Moreover, being statistical laws, they *add* to our knowledge: it is a mistake to think that they set limits to our knowledge. What they do set limits to is the scatter of particles (or more precisely, the scatter of the result of sequences of certain experiments with particles). This scatter, they tell us, cannot be suppressed. It is also a mistake to think that the alleged limitation to our knowledge could ever be validly used for explaining the statistical character of the quantum theory. (See my eighth thesis, below.) And ultimately, it is just the old muddle again if it is said that the Heisenberg formulae provide us with that *vagueness* which is allegedly needed for asserting without inconsistency the "dual" character of particles and waves; that is, their character as "wavicles".

6. My sixth thesis is that, however remarkable the statistical laws of the theory are, including the Heisenberg formulae (1) and (2), they refer to a population of *particles* (or of experiments with *particles*) which are, quite properly, endowed with positions and momenta (and mass-energy, and various other physical properties such as spin). It is true that the scatter relations tell us that *we cannot prepare experiments* such that we can avoid, upon repetition of the experiment, (1) scattering of the energy if we *arrange* for a narrow time limit, and (2) scattering of the momentum if we *arrange* for a narrowly limited position. But this means only that there are limits to the *statistical homogeneity* of our experimental results. Yet not only is it possible to *measure* energy *and* time, or momentum *and* position, with a precision greater than formulae (1) and (2) seem to permit, but *these measurements are necessary for testing the scatter* predicted by these very formulae.

I shall now try to produce some arguments for what I have said in my last two theses. These arguments will show, incidentally, that the Heisenberg formulae (1) and (2) can be derived from theories which are much older than the commutation relations of quantum mechanics.

We can derive HEISENBERG'S formula

$$\Delta E \, \Delta t \geqq h \tag{1}$$

from PLANCK'S quantum condition of 1900,

$$E = h\,\nu .$$

This leads, in view of the constancy of h, at once to

$$\Delta E = h\,\Delta\nu ,$$

a formula in which "Δ" may be interpreted in various ways. In order to obtain HEISENBERG'S formula (1) we only have to combine this formula with an even older principle of optics, the *principle of harmonic resolving power*. (Both HEISENBERG and BOHR base their derivations of the indeterminacy relations directly or indirectly upon this principle; cp. [*26*], pp. 21 and 27.) This principle states that if a monochromatic wave train of frequency ν is cut up by a *time shutter* into one stretch or several stretches ("wave packets") of the duration Δt, then the width $\Delta\nu$ of the spectral line will become

$$\Delta\nu \geqq 1/\Delta t .$$

This is, for various reasons, a remarkable law. (It contains the principle of superposition.) It leads from

$$\Delta E = h\,\Delta\nu$$

immediately to

$$\Delta E \geqq h/\Delta t$$

and thus to formula (1).

But in so deriving formula (1), *we are no longer free* to interpret "Δ" in various ways (for example, as the width of imprecision of a measurement). We are, rather, bound in our interpretation by the meaning given to "Δ" by the principle of harmonic resolving power. This principle interprets "$\Delta\nu$" as the width of spectral lines. Accordingly, Planck's principle (in Einstein's interpretation) forces us to interpret this width as the scatter of the energy of the particles (photons) which make up the spectral lines; for a spectral line of frequency ν is to be interpreted as the statistical result of incoming photons of energy $E = h\nu$, and consequently the width $\Delta\nu$ of the spectral line as the range ΔE of the *statistical scatter* of the energies of the photons which together form the spectral line. Thus formula (1) states the law that, if we vary at will the period Δt of our shutter, we are bound to influence inversely the scatter ΔE of the energy of the incoming photons.

This derivation shows clearly that (1) is a *statistical law*, and part of the statistical theory. It can be tested only by ascertaining the distribution of the incoming photons on the photographic film or plate; and in order to do this, we must measure the places where the photons hit the spectral line with an imprecision, say δE, very much smaller than the width ΔE of the line:

$$\delta E \ll \Delta E\,.$$

Thus the testing of the law expressed by (1) and of its statistical predictions demand that we can *measure* the incoming particle with a precision δE which satisfies

$$\delta E\,\Delta t \ll h\,.$$

This kind of thing is done every day; and it shows that the Heisenberg formulae are valid for *statistical predictions about many particles, or about sequences of many experiments* with individual particles, but that they are misinterpreted as limiting the precision of *measurements* of individual particles.

There is a derivation of the second Heisenberg formula

$$\Delta p_x \Delta q_x \geqq h \tag{2}$$

which is analogous to the derivation with the help of the *time shutter*. We start again with a (flat) monochromatic wave train ν and cut it;

this time by a *screen* (vertical to the direction z of the beam), *with one slit* of variable width Δq_x. (The "*one slit experiment*".) When the slit is very wide, there will be only a marginal effect upon the wave train. But when it narrows, we get a scattering (diffraction) effect: the narrower the slit Δq_x, the wider will be the angle by which the rays diverge from their original direction: here another form of the principle of harmonic resolving power applies ($\tilde{\nu}_x$ is the projection on the x-axis of the wave number, that is, the number of waves per centimetre):

$$\Delta \tilde{\nu}_x \approx 1/\Delta q_x.$$

Multiplying both sides by h we get

$$h\Delta \tilde{\nu}_x \approx h/\Delta q_x.$$

Using instead of PLANCK's formula $E = h\nu$ that of DE BROGLIE in the form $p_x = h\tilde{\nu}_x$, we can write "Δp_x" for "$h\Delta\tilde{\nu}_x$"; and so we arrive at (2).

When the slit Δq_x is very small we obtain, according to HUYGENS's principle, waves emerging from it which spread not only in the z direction but also in the $+x$ and $-x$ direction (cylinder waves). This means that the particles which, before reaching the slit, had a momentum $p_x = 0$ (since they were proceeding in the z direction), will now have a considerable scatter of momenta Δp_x, in the $+x$ and $-x$ direction. We can test this scatter again by measuring the various momenta with a spectrograph in various positions. There is, in principle, hardly a limit to the precision δp of the measurements of the various momenta in the various directions; that is, we have again

$$\delta p_x \ll \Delta p_x$$

and thus

$$\delta p_x \Delta q_x \ll h.$$

Again, we could not test the statistical law (2) in this *one slit experiment* without these more precise measurements $\delta p_x \ll \Delta p_x$.

Incidentally, *we measure the momentum*, p_x, of the incoming particle *by its position* on the film of the spectrograph. And this is typical. It should hardly be necessary to stress that we almost always measure momenta by positions. (For example, if we measure a Doppler effect, we do so with the help of a spectral line, that is by measuring the position of the line on a photographic plate.) It has, unfortunately, become necessary to emphasize this point, because of BOHR's repeated assertion that position measurements and momentum measurements are incompatible (and "complementary") owing to "the mutual exclusion of any two experimental procedures, permitting the unambiguous definition of complementary physical qualities ...". (Cp. BOHR in [*4*], p. 234, quoting from his reply [*3*] to EINSTEIN, PODOLSKY, and ROSEN [*21*].) The two experimental procedures, we are told by BOHR, exclude each other because the momentum measurements need a *movable screen* (as depicted in [*4*], p. 220), while the position measurement needs a *fixed screen* or a fixed photographic plate. But we often measure momenta by fixed photographic plates, that is, by positions; but never by a movable screen. (Incidentally, the use of BOHR's movable screen would entail at least two *position measurements* of the screen.)

A famous problem for particle theory is posed by the *two slit experiment* (or n slit experiment), with two (or more) slits with the (periodic) distance Δq_x between them. This has been recently cleared up by ALFRED LANDÉ ([*38*], pp. 9—12), using the Duane-Landé space-periodicity formula:

$$\Delta p_x = n\, h/\Delta q_x \qquad (n = 1, 2, \ldots).$$

The two slit experiment turns out to be a space-periodicity experiment with the periodicity Δq_x. The particles transfer to the screen (or the grid) a momentum packet Δp, or its multiples, such that

$$\Delta p_x \Delta q_x = h.$$

As a consequence (as shown by LANDÉ, *loc. cit.*) we get the wave-like fringes.

The usual question "how does the particle which goes through slit 1 'know' that slit 2 is open rather than closed?" can now be reasonably well cleared up. It is the *screen* (or the grid, or the crystal) which "knows" whether there is a periodicity Δq_x built into it or not, and which therefore "knows" whether it can absorb momentum packets of the size $\Delta p_x = h/\Delta q_x$. The particle does not need to "know" anything: it simply interacts with the screen (which "knows"), according to the laws of conservation of momentum *and* of space periodicity; or more precisely, it interacts with the total experimental arrangements (see my eighth and especially my tenth thesis below).

I have so far spoken mainly about particles and their (indirect) measurements, for example, momentum measurements by way of position measurements. But there are other methods, of course: Geiger counters may measure (not very precisely) position, *and* time; and so may Wilson chambers. And the position measurement in a Wilson chamber may be an indirect momentum measurement. However, the time measurement of the incoming particle may be of particular interest to us in every case in which the frequency (or energy) of the emission is very sharp — as it is in the classical case of a BOHR hydrogen atom.

Here we have RYDBERG'S constant R, a wave number, so that Rc is a constant frequency, ν_R, which can be calculated, according to HAAS (1910) and BOHR (1913), with great precision from the constants of the theory (μ is the mass of the electron, e its charge):

$$\nu_R = Rc = 2\pi^2 e^4 \mu / h^3.$$

Then the Rydberg-Ritz combination principle (formulated by RITZ, using RYDBERG'S constant, in 1908, five years before BOHR'S theory of the hydrogen atom) asserts for the frequencies, $\nu_{m,n}$, of emission or absorption the relation

$$\nu_{m,n} = \nu_R/m^2 - \nu_R/n^2 \qquad (m, n = 1, 2, \ldots).$$

Multiplied by h this becomes BOHR'S quantization rule of emission and absorption (1913). Thus the permissible frequencies $\nu_{m,n}$ and the various corresponding Bohr-energies of the particles can be calculated from first principles, as it were — they are variables which can take on only certain discrete values ("*eigenvalues*" which might be described as quasi-constants). Accordingly, $\Delta\nu_{m,n}$ may be extremely small, and Δt, calculated with the help of the principle of harmonic resolving power, will be large.

But these sharp spectral lines, although they must not be interfered with by means of a time shutter, can be statistically investigated by timing the arrivals of the photons (which gives also the time of emission) by means such as a Wilson chamber or a Geiger counter. (Especially impressive here are the Compton-Simon photographs of high frequency X-ray photons of very precise frequency or energy.) For these arrival times we may get $\delta t \ll \Delta t$, and thus

$$\Delta E\, \delta t \ll h.$$

7. My seventh thesis is that all this, or most of it, was in effect admitted by HEISENBERG.

First I would repeat that the *predictions* of the theory are statistical, with a scatter given by the Heisenberg formulae. The *measurements* which must be *more precise than the scatter* (as I have pointed out) may serve as *tests* of these predictions: *these measurements are retrodictions.*

HEISENBERG saw, and said, that these highly precise retrodictive measurements were possible. *What he did not see was that they had a function in the theory — that they were needed for testing it* (and that they could be tested in their turn).

Thus he suggested, half-heartedly but pretty strongly, that these retrodictive measurements were *meaningless*. And this suggestion was taken up and *turned into a dogma* by the adherents of the Copenhagen interpretation, especially when it was found that there were no vectors in Hilbert space corresponding to any measurements sharper than the formulae (1) and (2).

But this fact does not really create any difficulty. *The vectors in Hilbert space correspond to the statistical assertions of the statistical theory.*

They say nothing about measurements, or about the *tests* of the statistical assertions by the determination of the position and momentum, or of the energy and time, of individual particles.

I shall now quote some evidence for my thesis regarding HEISENBERG'S admission that the measurements I have described can be made, and his suggestion that they are, if not completely meaningless, at best pointless and uninteresting, because they merely refer to the past. He says that measurements which "can never be used as initial conditions in any calculation of the future progress of the electron and [which] thus cannot be subjected to experimental verification" are devoid of physical significance. (Cp. [*26*], p. 20.) But this is a double mistake. For (a) the preparation of initial conditions admittedly is very important, but so are test statements which *always* look into the past and whose *main* function is not to be "verifiable" (that is, testable) in their turn but to "verify" (or more precisely to test). And (b) it is a mistake to think that these test statements, though looking into the past, are not "verifiable" (or more precisely, testable) in their turn. On the contrary, it is one of the principles of the quantum theory that every measurement can be "verified" or tested in the sense that its immediate repetition will yield the same result. (This principle, whose author seems to be VON NEUMANN, is not generally valid, unless it is trivially so in the sense explained below, under the heading of my ninth thesis.) Thus to say that these measurements which look into the past "cannot be subjected to experimental verification" is simply mistaken.

In order to show quite definitely that HEISENBERG and I are talking about the same measurements, and that we are in agreement that they are not subject to the uncertainty relations, I wish to remind the reader of the one slit experiment and of the fact that the measurements of p_x with the help of spectrographs at various positions (or of a photographic plate parallel to the horizontal screen) are, in fact, *position* measurements, so that we obtain our total information about position + momentum by way of *two position measurements:* the first is provided by the slit Δq_x, the second by the impact of the particle on the photographic plate. (We can take the frequency — or energy — of the beam as known.) Now it is precisely about such an arrangement consisting of *two position measurements* (which allow us to calculate the position *and* momentum *after* the first and *before* the second measurement) that HEISENBERG says the following:

"The ... most fundamental method of measuring velocity [or momentum] depends on the determination of position at two different times ... it is possible to determine *with any desired degree of accuracy* the velocity [or momentum] before the second [measurement] was made; but it is the velocity *after* this measurement which alone is of importance to the physicist ..." (Cp. [*26*], p. 25. The italics are mine, and I have changed the position of a phrase to improve the readability by avoiding an ambiguity.)

HEISENBERG is even more emphatic concerning experiments in which we measure the position of a particle whose momentum is known (say, because the particle belongs to a monochromatic beam): "... the uncertainty relation does not refer to the past", he writes; "if the velocity of the electron is at first known and the position then exactly measured, the position for times previous to the measurement may be calculated. Then for these past times $\Delta p \, \Delta q$ is smaller than the usual limiting value." (Cp. [*26*], p. 20.) So far we can agree. But now comes our subtle but important disagreement; for HEISENBERG continues: "but this knowledge of the past is of a purely speculative character, since it can never ... be used as an initial condition in any calculation of the future progress of the electron" (this I believe to be true) "and thus cannot be subjected to experimental verification" (this is false, as I shall show).

HEISENBERG adds to this: "It is a matter of personal belief whether such a calculation concerning the past history of the electron can be ascribed any physical reality or not." (*Loc. cit.*)

Almost every physicist who read HEISENBERG opted for "not".

But it is not a matter of personal belief: the measurements in question are *needed* for testing the statistical laws (1) and (2); that is, the scatter relations.

The particular case, of a position measurement of a particle from which retrodictively "the positions for times previous to the measurement may be calculated", as HEISENBERG puts it, plays a *most* important role in physics: if we measure the position of a particle (a photon or an electron) on the photographic film of any spectrograph, then we use this position measurement (together with the known arrangement of the experiment) for calculating, with the help of the theory, the frequency or energy and thus the momentum of the particle; always, of course, retrodictively. To question whether the so ascertained "past history of the electron can be ascribed any physical reality or not" is to question the significance of an indispensable standard method of measurement (retrodictive, of course); indispensable, especially, for quantum physics.

But once we ascribe physical reality to measurements for which, as HEISENBERG admits, $\Delta p \, \Delta q \ll h$, the whole situation changes completely: for now there can be no question whether, according to the quantum theory, *an electron can "have" a precise position and momentum. It can.*

But it was just this fact that was constantly denied: although HEISENBERG made it "a matter of personal belief", BOHR and the Copenhagen interpretation (partly because of the non-existence of those vectors in

Hilbert space) *insisted that an electron just cannot have a sharp position and momentum at the same time.* This dogma is the core of BOHR's thesis that quantum theory is "complete", presumably in the sense that a particle cannot have properties which the theory (allegedly) does not allow to be "measured".

Thus the so-called "paradox" of EINSTEIN, PODOLSKY, and ROSEN (cp. [*21*] and [*3*]) is not a paradox but a valid argument, for it established just this: that we must ascribe to particles a precise position *and* momentum, which was denied by BOHR and his school (though it is admitted by BOPP).

The Einstein-Podolsky-Rosen thought experiment has since become a *real* experiment, in connection with pair-creation, and pair-destruction with photon-pair creation. The times and energies of the pairs can be in principle measured with any degree of precision. Of course, the measurements are *retrodictive:* they are *tests* of the theory. (See for example O. R. FRISCH's [*23*].)

Why did BOHR and his followers deny that $\delta p_x \delta q_x \ll h$ is possible? Because of the great quantum muddle, the alleged dualism of particle and wave: it is said that there are two "*pictures*", the *particle picture* and the *wave picture*, and that they have been shown to be equivalent or "complementary"; that is to say, both valid. But this "complementarity" or "duality" must break down, it is said, if we allow the particle to have at the same time a sharp position and momentum.

It is from here, and from the subjective interpretation of probability to which we shall turn next, that the subjectivist interpretation of the quantum theory arose — almost of necessity.

8. My eighth thesis results from an attempt of mine to explain, though not to excuse, the great quantum muddle, as I have called it. My thesis is that *the interpretation of the formalism of quantum mechanics is closely related to the interpretation of the calculus of probability.*

By the calculus of probability I mean a *formal calculus* which contains *formal laws* such as

$$0 \leqq p(a, b) \leqq 1$$

where "$p(a, b)$" may be read "the probability of a relative to b" (or "the probability of a given b").

What "probability" (the function of functor "p") *means*, and what the arguments "a" and "b" *stand for*, is left open to *interpretation.*

It is assumed, however, that there is a set of entities, S, say, to which the arguments $a, b, c, \ldots,$ belong; and that if a belongs to S, then $-a$ (read "non-a") also belongs to S; and that if a and b belong to S, then ab (read "a-and-b") also does. Moreover, it is assumed that the meaning of all these symbols, though open to many different interpretations, is partly fixed by a number of formal rules which connect these symbols.

The following formulae are trivial examples of such formal rules:

$$p(a, a) = 1 .$$

$$p(a, b) + p(-a, b) = 1, \qquad \text{unless } p(-b, b) \neq 0 .$$

$$p(a, b) = p(aa, b) = p(a, bb) .$$

$$p(a, c) \geqq p(ab, c) \leqq p(b, c) .$$

We may also give a definition of "*absolute probability*", $p(a)$, in terms of "*relative probability*", $p(a, b)$:

$$p(a) = p\big(a, -((-a)a)\big) .$$

The task of selecting a number of these *formal rules* so that all the others are derivable from them, is the task of finding one or more suitable *axiomatizations* of the formal calculus of probability. (Cp. [*50*] and [*51*].) I mention it only in order to contrast it with the task of finding one or more suitable *interpretations*. (Cp. [*53*], [*54*], and [*55*].)

There is a great variety of interpretations, which may be divided into two main groups: the *subjective* and the *objective* interpretations.

The *subjective interpretations* are those which interpret the number $p(a, b)$ as measuring something like our knowledge, or our belief, in (the assertion) a, given (the information) b. Thus the arguments of the p-function, that is, $a, b, c, \ldots$ are in this case to be interpreted as items of belief or doubt, or items of information, or propositions, or assertions, or statements, or hypotheses.

For a long time it was thought (and it still is thought by many eminent mathematicians and physicists) that we may start from a subjectively interpreted system of probabilistic premises and then *derive from these subjectivist premises statistical conclusions*. However, *this is a grave logical blunder*.

The blunder may be traced back to some of the great founders of probability theory, to JACOB BERNOULLI and especially to SIMÉON DENIS POISSON, who thought that they had discovered, in their derivations of the various forms of the *law of great numbers*, a kind of logico-mathematical *bridge* leading from non-statistical assumptions to statistical conclusions; that is, to conclusions concerning the *frequency* of certain events.

The logical mistake was carefully analysed by RICHARD VON MISES (see especially [*45*]) and also by myself. (Cp. [*50*], chapter VIII, and [*53*].) MISES showed that at some stage or other in the derivation, the non-statistical meaning of the symbols is dropped and tacitly replaced by a statistical one. This is usually done by interpreting a probability approaching 1 as "almost certain" in the sense of "almost always to happen", instead of "almost certain" in the sense of "very strongly believed in" or perhaps "almost known". Sometimes the mistake consists in replacing "almost certainly known" by "known almost certainly to occur". However this may be, the mistake is very clear: from premises about degrees of belief we can never get a conclusion about the frequency of events.

It is strange that this idea that we can derive statistical conclusions from premises expressing uncertainty is still so strong among quantum theorists; for JOHN VON NEUMANN, one of the most influential among them, accepted in his famous book, *Mathematical Foundations of Quantum Mechanics*, the theory of probability of VON MISES. (Cp. [*46*], p. 298, note 156.) Yet VON NEUMANN'S praise of this theory does not seem to have induced quantum theorists to study carefully VON MISES'S arguments against the existence of a "*bridge*" from non-statistical premises to statistical conclusions.

I do not wish to imply that I accept VON MISES'S theory as a whole; but I believe that his criticism of the alleged "*bridge*" from non-statistical premises to statistical conclusions is unanswerable; and I do not even know of any serious attempt to refute it. Nevertheless, the subjective theory, under the name of "Bayesian probability", is widely and uncritically accepted.

I now proceed to the *objective interpretations* of the probability calculus. I shall here distinguish between *three* such interpretations:

(a) *The classical interpretation* (DE MOIVRE, LAPLACE) which takes $p(a, b)$ to be the proportion of equally possible cases compatible with the event b which are also favourable to the event a. For example, let a be the event "at the next throw of this die 5 will turn up" and take b to be the assumption "6 will *not* turn up" (or "only throws other than 6 will be considered as throws"); then $p(a, b) = \frac{1}{5}$.

(b) *The frequency interpretation* or statistical interpretation (JOHN VENN, GEORG HELM, VON MISES) which takes $p(a, b)$ as the relative frequency of the events a among the events b. This interpretation, which I developed by trying to remove some of its difficulties (cp. [*50*], chapter VIII and new Appendix* VI), is one which I upheld for about twenty years (from approximately 1930 to 1950), though I always stressed the existence of other interpretations (cp. [*50*]).

(c) *The propensity interpretation* which I developed from a criticism of my own form of the frequency interpretation and which may at the same time be regarded as a refinement of the classical interpretation.

I shall have to say a few things about each of the three objective interpretations.

In favour of (a), the classical interpretation, it may be said that it is used, almost as a matter of course and obviously with good reason, in situations where we conjecture that we have before us something like "*equally possible cases*": we do not need to experiment with a regular polyhedron in order to conjecture that, if it is of homogeneous material and has n sides, the probability for each of these sides turning up in any one throw will be $1/n$.

On the other hand, the classical interpretation has been severely criticized on several counts, of which I will mention only two: as it stands it is inapplicable to anything like unequally possible cases such as playing with a loaded die; and it succumbs, like the subjective interpretation, to VON MISES's criticism: there is no logical or mathematical *bridge* (like the law of great numbers) which leads from premises about *possibilities* to statistical conclusions about *relative frequencies*. (MISES showed this in great detail for POISSON'S derivation of his law of great numbers.) Nor does it make much sense to say of a ratio of many favourable to many possible cases that, even if it approaches 1, it tells us what is almost certainly going to happen: obviously, there occurs here (as VON MISES stressed) a shift of meaning in proceeding from possibilistic premises to statistical conclusion.

As to (b), the frequency interpretation, I feel confident that I have succeeded (cp. [*50*]) in purging it of all those allegedly unsolved problems which some outstanding philosophers like WILLIAM KNEALE (cp. [*33*]) have seen in it. Nevertheless, I found that a further reform was needed, and I tried to respond to this need in two papers. (Cp. [*54*] and [*55*].)

Thus I come to (c), to the *propensity interpretation* of probability.

Let me first make clear that nothing is further from my mind than an attempt to solve the pseudo-problem of giving a definition of the meaning of probability. It is obvious that the word "probability" can be used perfectly properly and legitimately in dozens of senses, many of which, incidentally, are incompatible with the formal calculus of probability. (For such senses see [*60*] and [*24*].) I do not even wish to say that the propensity interpretation of probability is the best interpretation of the formal probability calculus. I only wish to say that it is the best interpretation known to me *for the application of the probability calculus to a certain type of "repeatable experiment"*; in physics, more especially, and also, I suppose, in related fields such as experimental biology.

I fully agree with those who have criticized the propensity interpretation because they felt it was not clear how to apply it to the betting situation in horse racing. The formal probability calculus is applicable to a large class of "games of chance"; but I do not know how one could apply it to betting on horses. Yet should it be possible to apply it to this kind of betting I should see no reason to fear that the propensity interpretation would not fit this case. In brief, I am not trying to propose universally satisfactory meanings for the words "probable" and "probability", or even a universally applicable interpretation of the formal calculus. But I am trying to propose an interpretation of the probability calculus which is not *ad hoc*, and which solves some of the problems of the *interpretation of quantum theory*.

I shall here explain the propensity interpretation as a development of the classical interpretation. The latter, it will be remembered, explains $p(a, b)$ as the proportion of the equally possible cases compatible with b which are also favourable to the event a.

I propose, as a first step, to omit the word "equally" and to introduce "weights" and thus to speak, instead of "numbers of cases", of the "sum of the weights of the cases". And I propose, as a second step, to interpret these "weights" of the possibilities (or of the possible cases) as *measures of the propensity, or tendency, of a possibility to realize itself upon repetition.*

The main idea of this interpretation can also be put as follows: I propose to distinguish *probability statements* from *statistical statements*, and to look upon probability statements as statements about frequencies in *virtual* (infinite) sequences of well characterized experiments, and upon *statistical statements* as statements about frequencies in *actual* (finite) sequences of such experiments. In probability statements, the "weights" attached to the possibilities are measures of these (conjectural) virtual frequencies, *to be tested by actual statistical frequencies*.

To use an example: if we have a large die containing a piece of lead whose position is adjustable, we may *conjecture* (for reasons of symmetry) that the weights (that is, the propensities) of the six possibilities are *equal* as long as the centre of gravity is kept equidistant from the six sides, and that they become *unequal* if we shift the centre of gravity from this position. For example, we may increase the weight of the possibility of 6 turning up by moving the centre of gravity away from the side showing the figure "6". And we may here interpret the word "weight" to mean "a measure of the propensity or tendency to turn up upon repetition of the experiment". More precisely, we may agree to take as our measure of that propensity the (virtual) *relative frequency* with which the side turns up in a (virtual, and virtually infinite) sequence of repetitions of the experiment.

We then may test our conjecture by a sequence of repetitions of the experiment.

In proposing the propensity interpretation I propose to look upon *probability statements* as statements about some measure of a property (a physical property, comparable to symmetry or asymmetry) of *the whole experimental arrangement;* a measure, more precisely, of a *virtual* *frequency;* and I propose to look upon the corresponding *statistical statements* as statements about the corresponding *actual frequency.*

In this way we easily get over the objection raised by VON MISES against the classical interpretation, simply by replacing mere *possibilities* by *propensities* which we interpret as tendencies to produce frequencies.

Two further points are very important:

First, the probability is taken to be a property of the *single experiment,* relative to some rule specifying the conditions for accepting another experiment as a *repetition of the first.* For example, in dicing, the minimum time taken in shaking the beaker may or may not form part of this rule, or of these conditions or specifications.

Secondly, we can look upon probability as a *real physical property of the single physical experiment* or, more precisely, of the *experimental conditions* laid down by the rule that defines the conditions for the (virtual) *repetition* of the experiment.

A propensity is thus a somewhat abstract kind of physical property; nevertheless it is a real physical property. To use LANDÉ'S terminology, *it can be kicked, and it can kick back.*

Take for example an ordinary symmetrical pin board, so constructed that if we let a number of little balls roll down, they will (ideally) form a normal distribution curve. This curve will represent the *probability distribution* for each single experiment with each single ball of reaching a certain possible resting place.

Now let us "kick" this board; say, by slightly lifting its left side. Then we also kick the propensity, and the probability distribution, since it will become more probable that any single ball will reach a point towards the right end of the bottom of the board. And the propensity will kick back: it will produce a differently shaped curve formed by the balls if we let them roll down and accumulate.

Or let us, instead, remove *one pin.* This will alter the probability for every single experiment with every single ball, *whether or not the ball actually comes near the place from which we removed the pin.* (This has its similarity with the two slit experiment, even though we have here no superposition of amplitudes; for we may ask: "How can the ball 'know' that a pin has been removed if it never comes near the place?" The answer is: the ball does not "know"; but the board as a whole "knows", and changes the probability distribution, or the *propensity,* for *every* ball; a fact that can be tested by statistical tests.)

Thus we can "kick" the probability field by making certain (gradual) changes in the conditions of the experiment, and the field "kicks back" by changing the propensities, an effect which we can test statistically by repeating the experiment under the changed conditions.

But there are further important aspects of the propensity interpretation, which we can again illustrate with the help of the pin board.

We can leave the pin board in its ordinary (symmetrical) state; and we can ask for the probability distribution of reaching the various final positions *for those balls which actually hit a certain pin* (or, alternatively, which hit the pin and then pass on its left side).

This new distribution will be, of course, quite different from the original distribution. It can be calculated from first principles (given a symmetrical board); and we can *test* our calculations in various ways. For example, we can let the balls roll down as usual, but list separately the final positions of those balls that hit the selected pin (or that hit it and pass on its left); or else, we can *remove* those balls at once which do not satisfy this new condition. In the first case, we merely *take note* of the new "position measurement" of the ball; in the second case, we *select* the balls which pass through some predetermined position.

In both cases we shall get tests of the calculated new distribution: the distribution of those balls which have undergone a "position measurement".

The theory of the pin board allows us, of course, to calculate from first principles the new distributions for any pin we choose; in fact, all these new distributions are implicit in calculating the original normal distribution. For this calculation assumes that the ball will hit, with such and such a probability, such and such a pin.

9. Ninth thesis. In the case of the pin board, the transition from the original distribution to one which assumes a "position measurement" (whether an actual one or a feigned one) is not merely analogous, *but identical with the famous "reduction of the wave packet"*. Accordingly, this is not an effect characteristic of quantum theory but of probability theory in general. (Cp. [*50*], section 76.)

Take our pin board example again: given not only the topography of the board but also its inclination and a few more facts, we may look at the probability distribution as a kind of descending wave front, starting to descend when the particle enters the board through its slit Δq. There will be no interference of amplitudes: if we have two slits Δq_1 and Δq_2, the two probabilities themselves (rather than their amplitudes) are to be added and normalized: we cannot imitate the two slit experiment. But this is not our problem at this stage. What I wish to show is this: we may calculate a probability wave, descending to the bottom of the board, and forming there a normal distribution curve very much like a wave packet.

Now if we let *one* actual ball roll down, then we can look at it from various points of view.

(a) We may say that the experiment as a whole determinés a certain probability distribution and retains it (upon repetition) irrespective of the particular pins hit by the ball.

(b) We may say that every time the ball actually hits a certain pin (or, say, passes on its left side), the *objective probability* distribution (the propensity distribution) is "suddenly" changed, whether or not anybody takes note of the course of the ball. But this is merely a loose way of saying the following: *if we replace the specification of our experiment by another one* which specifies that the ball hits that particular pin (or passes on its left), then we have a different experiment and accordingly get a different probability distribution.

(c) We may say that the *knowledge*, or the *information*, or the *consciousness*, or the *realization*, that a position measurement has taken place, leads to the *"collapse" or "reduction" of the original wave packet* and to its replacement by a new wave packet. But in speaking in this way, we only say the same as we said before under (b); except that we now use subjectivist language (or a subjectivist philosophy).

Obviously, if we do not, *know* which pin the ball has hit, we do not *know* with which new experimental set of conditions (propensities) we could replace, in this particular case, the old ones. But whether we know this or not — we *did* know from the very start that there was such and such a probability of the ball hitting such and such a pin, and thereby changing its propensity of hitting other pins, and ultimately of reaching a certain point (or column), a, at the bottom of the board. It was on this knowledge that we based our calculation of the original probability distribution (wave packet).

Let us call our original specification of the pin board experiment "e_1" and let us call the new specification (according to which we consider or select only those balls which have hit a certain pin, q_2, say, as repetitions of the new experiment) "e_2". Then it is obvious that the two probabilities of landing at a, $p(a, e_1)$ and $p(a, e_2)$, will not in general be equal, because the two experiments described by e_1 and e_2 are not the same. But this does not mean that the new information which tells us that the conditions e_2 are realized in any way changes $p(a, e_1)$: from the very beginning we could calculate $p(a, e_1)$ for the various a's, and also $p(a, e_2)$; and we knew that

$$p(a, e_1) \neq p(a, e_2).$$

Nothing has changed if we are informed that the ball has actually hit the pin q_2, except that we are now free, *if we so wish*, to apply $p(a, e_2)$ *to this case;* or in other words, we are free to look upon the case as an instance of the experiment e_2 instead of the experiment e_1. But we can, of course, continue to look upon it as an instance of the experiment e_1, and thus continue to work with $p(a, e_1)$: the probabilities (and also the probability packets, that is, the distribution for the various a's) are *relative probabilities:* they are *relative to what we are going to regard as a repetition of our experiment;* or in other words, they are relative to what experiments are, or are not, *regarded as relevant to our statistical test.*

Take another example, a very famous one, due to EINSTEIN, and discussed by HEISENBERG ([*26*], p. 39) and by myself ([*50*], end of section 76; English edition, pp. 235 f.). Take a semi-transparent mirror, and assume that the probability that light will be reflected by it is $\frac{1}{2}$. Thus the probability that light will pass through will also be $\frac{1}{2}$, and we have, if the event "passing through" or "transmitted" is a, and the experimental arrangement b,

$$p(a, b) = \tfrac{1}{2} = p(-a, b)$$

where "$-a$" (that is, "non-a") stands for the event "reflection". Now let the experiment be carried out with one single photon. Then the probability wave packet attached to this photon will split, and we shall have the *two* wave packets, $p(a, b)$ and $p(-a, b)$, for which our equation

$$p(a, b) = \tfrac{1}{2} = p(-a, b)$$

will hold. "After a sufficient time", HEISENBERG writes, "the two parts will be separated by any distance desired ...". Now let us assume that we "find", with the help of a photographic plate, that the photon (which is indivisible) was reflected. (HEISENBERG says that it is "in the reflected part of the packet", which is a misleading metaphor.) "Then the probability", he writes, "of finding the photon in the other part of the packet *immediately becomes zero*. The experiment at the position of the reflected packet thus exerts a kind of action (reduction of the wave packet) at the distant point occupied by the transmitted packet, and one sees that this action is propagated with a velocity greater than that of light." (Cp. [*26*], p. 39; the italics are mine.)

Now this is the great quantum muddle with a vengeance. What has happened? We had, and still have, the relative probabilities

$$p(a, b) = \tfrac{1}{2} = p(-a, b)\,.$$

If we take the information $-a$ (which says that the particle has been reflected), then relative to this information we get

$$p(a, -a) = 0\,, \qquad p(-a, -a) = 1\,.$$

The first of these probabilities or wave packets is indeed zero. But it is quite wrong to suggest that it is a kind of changed form of the original packet $p(a, b)$ which "*immediately becomes zero*". The original packet $p(a, b)$ remains equal to $\frac{1}{2}$, which is to be interpreted as meaning that if we repeat our original experiment, the virtual frequency of photons being transmitted will equal $\frac{1}{2}$.

And $p(a, -a)$, which is zero, is quite another relative probability: it refers to *an entirely different experiment* which, although it begins like the first, ends *according to its specification* only when we find (with the help of the photographic plate) that the photon has been reflected.

No *action* is exerted upon the wave packet $p(a, b)$, neither an action at a distance nor any other action. For $p(a, b)$ is the propensity of the state of the photon relative to the original experimental conditions. This has not changed, and it can be tested by repeating the original experiment.

It might be thought that it is unnecessary to repeat all this after 32 years. But more recently, HEISENBERG has suggested that the reduction of the wave packet is somewhat similar to a quantum jump. For, on the one hand, he speaks of "the reduction of wave-packets" as "the fact that the wave function ... changes discontinuously", adding, "It is well known that the reduction of wave-packets always appears in the Copenhagen theory when *the transition is completed from the possible to the actual* ..." that is, when "the actual is selected from the possible, which is done by the '*observer*' ...". On the other hand, he speaks on the next page of the "element of discontinuity [in] the world, which is found everywhere in atomic physics ... [and which in] the usual interpretation of quantum theory ... *is contained in the transition from the possible to the actual*". (Cp. [*27*], pp. 23 f.; the italics are mine.)

Yet the reduction of the wave packet clearly has nothing to do with quantum theory: it is a trivial feature of probability theory that, whatever $p(a, b)$ may be, $p(a, a) = 1$ and (in general) $p(-a, a) = 0$.

Assume that we have tossed a penny. (The example is taken from p. 69 of my [*55*].) The probability of each of its possible states equals $\frac{1}{2}$. As long as we don't look at the result of our toss, we *can* still say that the probability will be $\frac{1}{2}$. If we bend down and look, it suddenly "changes": one probability becomes 1, the other 0. Was there a quantum jump, owing to our looking? Was the penny influenced by our observation? Obviously not. (The penny is a "classical" particle.) Not even the probability (or propensity) was influenced. There is no more involved here, or in *any* reduction of the wave packet, than the trivial principle: if our information contains the result of an experiment, then the probability of this result, relative to this information (regarded as part of the experiment's specification), will always trivially be $p(a, a) = 1$.

This explains also what is valid in VON NEUMANN'S principle, mentioned in my seventh thesis above, that if we repeat a measurement at once, then the result will be the same with certainty. Indeed, it is quite trite that if we look at our penny a second time, it will still lie as before.

And more generally: if we take a "measurement" like that of the arrived photon as *defining the conditions of the experiment*, then the outcome of the repetition of this experiment is certain, by virtue of the specification of the experiment together with the trite fact that $p(a, a) = 1$.

Before proceeding to my next thesis, I will just return for a moment to the pin board. Take

$$p(q_2, e_1) = r$$

to be the probability of a ball hitting the pin q_2, in the original experiment, and assume that we see the ball passing q_2 *without* hitting it. Then this can be interpreted exactly as HEISENBERG interprets the experiment with the semi-transparent mirror: we could say (it would be very misleading) that the wave packet $p(q_2, e_1)$ collapses, that it becomes zero with super-luminal velocity. I hope that the absurdity of the muddle need not be further elaborated.

10. My tenth thesis is that the propensity interpretation solves the problem of the relationship between particles and their statistics, and thereby that of the relationship between particles and waves.

DIRAC writes: "Some time before the discovery of quantum mechanics people [EINSTEIN, VON LAUE] realized that the connection between light waves and photons must be of a statistical character. What they did not clearly realize, however, was that the wave function gives information about the probability of *one* photon being in a particular place and not the probable number of photons in that place." (Cp. [*14*], p. 9.) And he continues with an example very much like the example discussed above of the semi-transparent mirror interacting with *one* photon.

Now this application of probability theory to single cases is precisely what the propensity interpretation achieves. But it does not achieve it by speaking about particles or photons. *Propensities are properties of neither particles nor photons nor electrons nor pennies.* They are *properties of the repeatable experimental arrangement* — physical and concrete, in so far as they may be statistically tested (and may lead, in the pin board case, to an actual characteristic physical arrangement of balls) — and abstract in so far as *any particular experimental arrangement* may be regarded as an instance of more than one specification for "its" repetition. (Take the tossing of a penny: it may have been thrown 9 feet up. Shall we say or shall we not say that this experiment is repeated if the penny is thrown to a height of 10 feet?) It is this *relativity* of the propensities that makes them sometimes look "unreal": it is the fact that they refer both to single cases *and* to their virtual repetitions, and that any single case has so many properties that we cannot say, just by inspection, which of them are to be included among the specifications defining what should be taken as "our" experiment, and as "its" repetition.

But this is true not only of all propensities or probabilities (classical or quantum-mechanical); it is also true of *all physical or biological experiments*, and it is one of the reasons why experimentation is impossible without theory: what seems to be completely irrelevant in one experiment e_1, or arbitrarily variable in its repetitions, may turn out in "another" experiment e_2 (otherwise indistinguishable) to be part of its most important specifications. Every experimentalist can give countless examples. Some so-called "chance discoveries" have been made by getting unwanted, or unexpected, results upon repeating an experiment, and then noticing that the change in the result depended upon some factor previously conjectured to be irrelevant, and therefore not included in (nor excluded by) the specification of the experiment.

Thus the *relativity to specification* of which we have spoken is characteristic neither of quantum experiments nor even of statistical experiments: *it is a permanent feature of all experimentation.* (And a propensity relation might be regarded, and intuitively understood, as a generalization of a "causal" relation, however we may interpret "causality".) For this reason it seems to me mistaken to regard statistical laws, statistical distributions, and other statistical entities, as non-physical or unreal. Probability fields are physical, even though they depend on, or are relative to, specified experimental conditions. (Cp. [*49*], pp. 213 f.)

11. My eleventh thesis is this: even though both the particles and the probability fields are real, it is misleading (as LANDÉ rightly insists) to speak of a "duality" between them: the particles are important *objects* of the experimentation; the probability fields are propensity fields, and as such important *properties of the experimental arrangement*, and of its specified conditions.

A simple example (taken from p. 89 of my [*55*]) may illustrate this. One is easily tempted to look upon the probability $\frac{1}{2}$ as a propensity of a homogeneous coin with a head and a tail side — that is, as a property of a thing, of a kind of "particle". *But the temptation must be resisted.* For let us assume an experimental arrangement in which the penny is not spun but tossed in such a way that it falls on a table with some slots in which it can be caught upright. Then we may distinguish between *three* possibilities: heads showing up; tails showing up; and neither showing up. Or even four possibilities: if the slots are all north-south, we may distinguish the caught pennies by the direction in which their heads face (east or west). This shows that conditions other than the structure of the penny (or the particle) may greatly contribute to the probability or propensity: the whole experimental arrangement determines the "sample space" and the probability distribution. (We also can easily conceive of specifications according to which the experimental conditions change, perhaps even in a certain "random" manner, while the experiment proceeds.)

Thus the propensity or probability is not (like baldness, or charge) a property of the member of the population (man, particle) but somewhat more like the popularity (and consequently, the sales statistic) of a certain brand of chocolate, depending on all kinds of conditions (advertisement, sales organization, statistical distribution in the population of preferential taste for various kinds of chocolate). And a wave-like distribution of a probability (or a probability amplitude) is, indeed, something which cannot be said to be an alternative "picture" of the member of the population (man; bar of chocolate; particle). It would be awkward to speak of a "duality" (a *symmetrical* relation) between a bar of chocolate and the shape of the distribution curve of its propensity to be sold tomorrow.

12. My twelfth thesis is that the mistaken idea of a duality of particle and wave is, partly, due to the hopes raised by DE BROGLIE and SCHRÖDINGER of giving a wave theory of the *structure of particles*.

There was a span of over two years between the beginning of wave mechanics and the successful analysis and interpretation of experiments as tests of BORN's statistical interpretation, first presented in 1926, of the ψ-function. (Cp. [*10*], p. 104.) In these years, the statistical problems seemed less important than the hope of solving the problems of atomic stability (and of quantum jumps) by a classical method — a very beautiful method, and an inspiring hope: the hope was nothing less than one of explaining matter and its structure by field concepts. When later SCHRÖDINGER and ECKART showed the (far-reaching though not complete) equivalence of the wave theory and HEISENBERG's particle theory, the two-picture interpretation was born, with its idea of a symmetry or duality between particle and wave. But so far as there was an equivalence, it was one between *two statistical theories* — a statistical theory ("matrix mechanics") which started from the statistical behaviour of particles, and a statistical theory which started from the wave-like shape of certain probability amplitudes. We might say (being wise after the event) that SCHRÖDINGER's hope that what he had found was a wave theory of the structure of matter should not have survived (cp. [*58*]) the successful tests of BORN's statistical interpretation of the wave theory.

13. My thirteenth and last thesis is this. Both classical physics and quantum physics are indeterministic. (Cp. [*52, 40*], and [*10*], pp. 107 to 110.) The peculiarity of quantum mechanics is the principle of the superposition of wave amplitudes — a kind of *probabilistic dependence* (called by LANDÉ "*interdependence*") that has apparently no parallel in classical probability theory. To my way of thinking, this seems to be a point in

favour of saying that propensities are physical and real (though *virtual*, as stressed by FEYNMAN). For the superposition can be kicked: coherence (the phase) can be destroyed by the experimental arrangement.

ALFRED LANDÉ has made a most interesting and it seems at least partly successful attempt to explain this peculiarity by showing mathematically that "The question ... *why do the probabilities interfere?* can ... be answered: they have no other choice if they 'want' to obey a *general interdependence law* at all." (Cp. [*38*], p. 82; the italics are partly mine.) Let us assume that LANDÉ's brilliant derivations of quantum theory from non-quantal principles of symmetry stand up to critical analysis: even then it seems to me that his own arguments show that these probabilities (propensities) whose amplitudes can interfere should be conjectured to be physical and real, and not merely a mathematical device (as he sometimes seems to suggest). Though their mathematical "pictures" may have the shape of "waves" only in "configuration space", *as propensities* they are physical and real, quite independently of the question whether or not they can be represented by a wave picture, or a function with a wave shape, or, indeed, by any picture or shape at all. The wave picture may thus have only a mathematical significance; but this is not true of the laws of superposition which express a real probabilistic dependence.

On the other hand, it seems to me clear from the Compton-Simon photographs that *photons can be kicked and can kick back*, and are therefore (in spite of LANDÉ'S sceptical views as to their existence) "*real*" in precisely the sense which LANDÉ himself has given to the term.

As always, nothing depends on words, but talking of "dualism of particle and wave" has created much confusion, as LANDÉ rightly emphasizes; so much so that I wish to support his suggestion to abandon the term "dualism". I propose that we speak instead (as did EINSTEIN) of the particle and its "*associated*" propensity fields (the plural indicates that the fields depend not only on the particle but also on other conditions), thus avoiding the suggestion of a symmetrical relation.

Without establishing some such terminology as this ("*association*" in place of "*dualism*") the term "dualism" is bound to survive, with all the misconceptions connected with it; for it does point to something important: the association that exists between particles and fields of propensities ("forces", decay propensities, propensities for pair production, and others).

Incidentally, among the misleading fashionable terms of the theory is the term "observable". (Cp. [*2a*], especially pp. 465 f.) It suggests something that does not exist: all "observables" are *calculated and inferred on theoretical grounds*, rather than observed or directly measured. Thus what is "observable" always depends upon the theory we use. However, here again one should not quarrel about words; no more about the word "observable" than about the word "real". Definitions, as usual, lead nowhere; but most of us know what we mean when we say that *there are* such things (observable things) as elephants, or electrons, or magnetic fields; or (more difficult to observe) propensities, such as the propensity to attract, or to understand, or to criticize; or the propensity of an experiment to yield some specified result.

14. To sum up. The alleged dualism of particle and wave and the subjective interpretation of probability, with which it is closely connected, are responsible for the subjectivistic and anti-realistic interpretation of quantum theory and for such characteristic statements as WIGNER'S, who says that "the laws of quantum mechanics itself cannot be formulated ... without recourse to the concept of consciousness" (cp. [*61*], p. 232); a view that he attributes also to VON NEUMANN; or HEISENBERG'S statement: "The conception of objective reality ... has thus evaporated ... into the transparent clarity of a mathematics that represents no longer the behaviour of particles but rather our knowledge of this behaviour." (Cp. [*28*], p. 100.) Or his assertion that if the observer is exorcized, and physics made objective, the ψ-function "contains no physics at all". (Cp. [*27*], p. 26.)

I have often argued in favour of the evolutionary significance of consciousness, and its supreme biological role in grasping and criticizing ideas. But its intrusion into the probabilistic theory of quantum mechanics seems to me based on bad philosophy and on a few very simple mistakes. These, I hope, will soon be forgotten, while the great physicists who happened to commit them will be for ever remembered by their marvellous contributions to physics: contributions of a significance and depth to which no philosopher can aspire.

References

[*1*] BOHM, D.: Quantum theory. 1951.

[*2*] — A suggested interpretation of quantum theory in terms of "hidden" variables. Phys. Rev. **85**, 166—179, 180—193 (1952).

[*2a*] —, and J. BUB: A proposed solution of the measurement problem in quantum mechanics by a hidden variable theory. Rev. Mod. Phys. **38**, 453—469 (1966).

[*3*] BOHR, N.: Can quantum-mechanical description of physical reality be considered complete? Phys. Rev. **48**, 696—702 (1935).

[*4*] — Discussion with Einstein on epistemological problems in atomic physics, [*57*], 201—241.

[*5*] — On the notions of causality and complementarity. Dialectica **2**, 312—319 (1948).

[*6*] — The quantum postulate and the recent development of atomic theory. Nature **121**, 580—590 (1928).

[*7*] —, u. L. ROSENFELD: Zur Frage der Meßbarkeit der elektromagnetischen Feldgrößen. Det Kgl. Danske Videnskabernes Selskab, Mathematisk-fysiske Meddelelser **12**, No. 8 (1933).

[43] MILLIKAN, R. A.: Electrons (+ and —), protons, photons, neutrons and cosmic rays. 1935.
[44] — Time, matter, and values. 1932.
[45] MISES, R. VON: Probability, statistics, and truth, 1939 (1st German ed. 1928; 3rd, 1951).
[46] NEUMANN, J. VON: Mathematical foundations of quantum mechanics, 1949, 1955 (German ed. 1932).
[47] PAULI, W. (ed.): Niels Bohr and the development of physics. 1955.
[48] POLANYI, M.: The logic of personal knowledge. 1961.
[49] POPPER, K. R.: Conjectures and refutations. 1963 and 1965.
[50] — The logic of scientific discovery. 1959 (1st German ed. 1934; 2nd 1966).
[51] — Creative and non-creative definitions in the calculus of probability. Synthese **15**, 167—186 (1963).
[52] — Indeterminism in quantum physics and in classical physics. Brit. J. Phil. Sci. **1**, 117—133 and 173—195 (1950).
[53] — Probability magic, or knowledge out of ignorance. Dialectica **11**, 354—374 (1957).
[54] — The propensity interpretation of probability. Brit. J. Phil. Sci. **10**, 25—42 (1959).
[55] — The propensity interpretation of the calculus of probability, and the quantum theory, [34], 65—70 and 88f.
[56] ROSENFELD, L.: Misunderstandings about the foundations of quantum theory, [34], 41—45.
[57] SCHILPP, P. A. (ed.): Albert Einstein: Philosopher-scientist. The library of living philosophers. 1949.
[58] SCHRÖDINGER, E.: The general theory of relativity and wave mechanics, [11], 65—74.
[59] — Über das Verhältnis der Heisenberg-Born-Jordanschen Quantenmechanik zu der Meinen. Ann. Physik **79**, 734—756 (1926).
[60] SHACKLE, G. L. S.: Decision, order, and time in human affairs. 1961.
[61] WIGNER, E. P.: The probability of the existence of a self-reproducing unit, [48], 231—238.

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[8] BOPP, F.: Statistische Mechanik bei Störung des Zustands eines physikalischen Systems durch die Beobachtung, [9], 128—149.
[9] — (ed.): Werner Heisenberg und die Physik unserer Zeit. 1961.
[10] BORN, M.: Bemerkungen zur statistischen Deutung der Quantenmechanik, [9], 103—118.
[11] — Scientific papers presented to Max Born. 1953.
[12] BUNGE, M.: Strife about complementarity. Brit. J. Phil. Sci. **6**, 1—12 and 141—154 (1955).
[13] — (ed.): The critical approach to science and philosophy. 1964.
[14] DIRAC, P. A. M.: The principles of quantum mechanics, 4th ed. 1958.
[15] ECKART, C.: Operator calculus and the solution of the equations of quantum dynamics. Phys. Rev. **28**, 711—726 (1926).
[16] EDDINGTON, A.: Relativity theory of protons and electrons. 1936.
[17] EINSTEIN, A.: Sidelights on relativity. 1922.
[18] — Quanten-Mechanik und Wirklichkeit. Dialectica **2**, 320—324 (1948).
[19] — Remarks concerning the essays brought together in this co-operative volume, [57], 665—688.
[20] — Zur Allgemeinen Relativitätstheorie. Sitzungsberichte der Preußischen Akademie der Wissenschaften 1915, Teil 2, 778—786 und 799—801.
[21] —, B. PODOLSKY, and N. ROSEN: Can quantum-mechanical description of physical reality be considered complete? Phys. Rev. **47**, 777—780 (1935).
[22] FEIGL, H., and G. MAXWELL (eds.): Current issues in the philosophy of science. 1961.
[23] FRISCH, O. R.: Observation and the quantum, [13], 309—315.
[24] HAMBLIN, C. L.: The modal "probably". Mind **68**, 234—240 (1959).
[25] HANSON, N. R.: Are wave mechanics and matrix mechanics equivalent theories?, [22], 401—425.
[26] HEISENBERG, W.: The physical principles of the quantum theory. 1930.
[27] — The development of the interpretation of the quantum theory, [47], 12—29.
[28] — The representation of nature in contemporary physics. Daedalus **87**, 95—108 (1958).
[29] — Über quantentheoretische Kinematik und Mechanik. Mathematische Annalen **95**, 683—705 (1926).
[30] HILL, E. L.: Comments on Hanson's "Are wave mechanics and matrix mechanics equivalent theories?", [22], 425—428.
[31] JÁNOSSY, L., and L. NAGY: Experiments on the Rossi curve. Acta Phys. Acad. Sci. Hung. **6**, 467—484 (1957).
[32] —, T. SÁNDOR, and A. SOMOGYI: On the photon component of extensive air showers. Acta Phys. Acad. Sci. Hung. **6**, 455—465 (1957).
[33] KNEALE, W. C.: Probability and induction. 1949.
[34] KÖRNER, S. (ed.): Observation and interpretation in the philosophy of physics, 1957. Dover ed. 1962.
[35] LANCZOS, C.: Albert Einstein and the cosmic world order. 1965.
[36] LANDÉ, A.: Foundations of quantum theory. 1955.
[37] —, From dualism to unity in quantum mechanics. 1960.
[38] —, New foundations of quantum mechanics. 1965.
[39] —, Quantum mechanics. 1951.
[40] —, Probability in classical and quantum theory, [11], 59—64.
[41] MACH, E.: The science of mechanics, 5th ed., 1941 (1st German ed. 1883).
[42] MEHLBERG, H.: Comments on Landé's "From duality to unity in quantum mechanics", [22], 360—370.