At the end I will add some philosophical thinking to the mixture.

First, we listen to Heisenberg, express correctly (in 1955) what is happening.
He beautifully reflects on the 1920s.

Then we will delve into the philosophical ideas that are inherent in the correct interpretation of Quantum Mechnics

THE DEVELOPMENT OF THE INTERPRETATION OF THE QUANTUM THEORY
according to Heisenberg

I. The fact that Planck's quantum theory would cause some changes in the foundations of physics must have been realized after Einstein's work on light quanta in 1905, if not earlier.

The quantum theory, nevertheless, developed for almost another 20 years without its principles being clarified, and the work of Bohr, Kramers and Slater in 1924 was the first serious attempt to resolve the paradoxes of radiation into rational physics.

In what follows we shall briefly outline the history of this clarification from 1924 to 1927, and we shall then inquire into the criticisms which have recently been made against the Copenhagen interpretation of the quantum theory.

In 1924 Bohr, Kramers and Slater asserted, first of all, that the wave propagation of light on the one hand, and its absorption and emission in quanta on the other, are experimental facts, which must be made the basis of any attempt at clarification, and not explained away; the fundamental consequences of this state of affairs must, therefore, be taken seriously.

They therefore introduced the hypothesis that the waves are of the nature of probability waves: that they represent not a “reality" in the classical sense, but rather the "possibility" of such a reality.

The hypothesis was that the waves defined the probability, at every point, that an atom present there is emitting or absorbing radiation.

The absorption and emission were assumed to take place in quanta $h\nu$.

It seemed to follow from this that the law of conservation of energy cannot be maintained in the individual processes, and Bohr, Kramers and Slater assumed that it holds only for the statistical average.

Although the assumption that the energy conservation law does not hold for individual processes later proved to be incorrect (the relations were considerably less simple than could then be foreseen), the attempt at interpretation made by Bohr, Kramers and Slater nevertheless contained some very important features of the later, correct, interpretation.

The most important of these was the introduction of the probability as a new kind of "objective" physical reality.

This probability concept is closely related to the concept of possibility, the "potentia" of the natural philosophy of the ancients such as Aristotle; it is, to a certain extent, a transformation of the old "potentia" concept from a qualitative to a quantitative idea.

On the other hand, the single quantum jump of Bohr, Kramers and Slater is "factual" in nature; it "happens" in the same manner as an event in everyday life, or the deflection of a galvanometer.

Somewhat later, Bothe and Geiger showed experimentally by means of the Compton effect that the energy conservation law is valid for individual processes also.

In the summer of 1925 quantum mechanics was developed, and in the spring of 1926 Schrodinger's wave mechanics, based on earlier work by de Broglie, began to be evolved.

The mathematical equipment of the new quantum theory was thus complete in its most important parts by the middle of 1926, but the physical significance was still extremely unclear.

An important step forward was made by the work of Born in the summer of 1926.

In this work, the wave in **configuration space** was interpreted as a probability wave, in order to explain collision processes on Schrodinger's theory.

This hypothesis contained two important new features in comparison with that of Bohr, Kramers and Slater.

The first of these was the assertion that, in considering "probability waves," we are concerned with processes **not** in ordinary three-dimensional space, but in an **abstract** configuration space (a fact which is, unfortunately, sometimes overlooked even today);
the second was the recognition that the probability wave is related to an individual process.

The probability wave describes the behavior, not of a large number of electrons, but only of one system of particles whose number is finite and is given by the number of dimensions in the configuration space; the wave can be conceived as representing a statistical assembly only in so far as the experiment concerned can be repeated as often as we please.

This can be more exactly expressed as follows: the probability wave in a configuration space of 3n dimensions contains statistical statements about only one system of n electrons, which can for this purpose be imagined, as in Gibbs' thermodynamics, as a sample selected arbitrarily from an infinite statistical assembly of identically constructed systems.

Shortly afterwards, Born's hypothesis was extended and generalized by the following result, which had been obtained in the analysis of fluctuations.

The interpretation of the diagonal matrix elements as time averages in matrix mechanics necessarily leads to the conclusion that the squared moduli $|S_{ab}|^2$ of the elements of the transformation matrix must be interpreted as the probabilities that the system will be found to be in the state b if it is in state a .

Since Schrodinger had recognized that the wave functions were the elements of the transformation matrices for the transition from energy states to position states, Born's hypothesis formed a particular case of this more general assumption, which fitted naturally into the scheme of quantum-mechanics.

Even then, however, a complete interpretation of the quantum theory had not been achieved, for the question remained how to define the word "state" in the theory.

A hydrogen atom in its normal state could be represented in the mathematical scheme of the theory.

There were, however, entirely different states.

For example, the track of an electron as seen in the cloud chamber.

How should one represent in the theory an electron which is moving at a definite point with a definite velocity?

Meanwhile (it was now the autumn of, 1926), a quite new and different proposal for the interpretation of quantum theory had been made, which arose out of the development of wave mechanics.

Schrodinger attempted to deny entirely the existence of discrete energy values and quantum jumps, and to resolve quantum theory into a simple classical wave theory.

The motive for this attempt was the discovery that the discrete eigenvalues appeared in wave mechanics not as energies, but as the eigenfrequencies of waves, and that the electric charge densities, when represented as products of waves, gave the correct radiation amplitudes.

At the invitation of Bohr, Schrodinger visited Copenhagen in September, 1926, to lecture on wave mechanics.

Long discussions, lasting several days, then took place concerning the foundations of quantum theory, in which Schrodinger was able to give a convincing picture of the new simple ideas of wave mechanics, while Bohr explained to him that not even Planck's Law could be understood without the quantum jumps.

"*If we are going to stick to this damned quantum-jumping, then I regret that I ever had anything to do with quantum theory,*" Schrodinger finally exclaimed in despair, to which Bohr replied: "*But the rest of us are thankful that you did, because you have contributed so much to the clarification of the quantum theory.*"

At any rate, wave mechanics had brought a new viewpoint, a new element of simplicity, into the quantum theory, which had to be incorporated into its interpretation.

The months which followed Schrodinger's visit were a time of the most intensive work in Copenhagen, from which there finally emerged what is called the **"Copenhagen interpretation of quantum theory,"** and I remember with pleasure the exhaustive discussions with Bohr, often lasting till late at night, in which the usefulness of every new attempt at interpretation was by means of real or imagined experiments examined in the closest detail.

Bohr intended to work the new simple pictures, obtained by wave mechanics, into the interpretation of the theory, while I for my part attempted to extend the physical significance of the transformation matrices in such a way that a complete interpretation was obtained which would take account of all possible experiments.

The clarification of these two approaches, at first sight apparently different, took place in the early part of 1927, when Bohr had gone to Norway for several weeks on a skiing holiday.

At this time Bohr developed the foundations of his idea of "complementarity." while I tried to solve the problem of how to pass from an experimentally given situation to its mathematical representation, by inverting the question, that is, by the hypothesis that only those states which can be represented as vectors in Hilbert space can occur in nature or be realized experimentally.

This method of solution, concerning which I had an exhaustive correspondence with Pauli at the time, had its prototype in Einstein's special theory of relativity.

Einstein had removed the difficulties of electrodynamics by saying that the "apparent" time of the Lorentz transformation was the real time; he had assumed that Nature is such that the real time always corresponds to the letter t' in the Lorentz transformation.

Similarly, it was now assumed in quantum mechanics that real states can always be represented as vectors in Hilbert space (or as "mixtures" of such vectors).

The uncertainty principle was the simple expression for this assumption.

Bohr's concept of complementarity resulted in the same restrictions to the applicability of classical concepts, owing to the appearance of quite different simple pictures which were “complementary" and which could co-exist without contradiction only if their range of application was restricted.

Some time later, Bohr's view of complementarity found another, very impressive representation in the mathematical scheme of the quantum theory, when Jordan, Klein and Wigner were able to show that, starting from a simple (three-dimensional) theory of material waves in Schrodinger's sense, one could quantize this theory and so come back to the Hilbert space of quantum mechanics.

The complete equivalence of the particle and wave pictures in the quantum theory was thus demonstrated for the first time, and Schrodinger's viewpoint of a three-dimensional wave theory of matter had found its rigorous basis.

The Pauli exclusion principle and the Bose statistics thereby also achieved their proper place in quantum theory.

From the spring of 1927, therefore, there existed at last a complete, unambiguous mathematical procedure for the interpretation of experiments on atoms orfor predicting their results.

The interpretation, too, contained the well-known statistical elements, which had appeared long since in the experiments (e.g. in α-decay, the photoelectric effect, etc.).

In the autumn of 1927 the Solvay Conference took place in Brussels, and here the new interpretation of quantum theory was exposed to the most ingenious criticism, particularly on the part of Einstein, and thereby received its crucial test.

Again those experiments were discussed whose interpretation had always offered the greatest difficulties, and it became apparent over and over again that the new interpretation contained no internal contradictions, and clearly led to the correct experimental results.

One of the finest documents of the discussions at this conference is Bohr's article for Albert Einsteins' 70th birthday.

Since the Solvay Conference of 1927, the "Copenhagen interpretation" has been fairly generally accepted, and has formed the basis of all practical applications of quantum theory.

It has, however, occasionally been contradicted and criticized as the **"orthodox"** theory.

The criticism of the theory, which we shall discuss in detail below was, however, partly concerned with another side of the problem, which became important only as time went on.

What was born in Copenhagen in 1927 was not only an unambiguous prescription for the interpretation of experiments, but also a language in which one spoke about Nature on the atomic scale, and in so far a part of philosophy.

Indeed, the way in which Bohr had thought about atomic phenomena since 1912 had always been something intermediate between physics and philosophy, and he had succeeded in explaining the periodic system of elements from atomic theory only by combining fundamental inquiry with the practical problems of experiment.

Thus he formulated the new interpretation of quantum theory in the philosophical language to which he had become accustomed by 15 years' acquaintance with atoms, and which seemed best suited to the problems involved.

This was not, however, the language of one of the traditional philosophies, positivism, materialism, or idealism; it was different in content, although it included elements from all these systems of thought.

II. The criticism of the interpretation of quantum theory came at first from the older physicists, who were not prepared to sacrifice so much of the edifice of ideas of classical physics as was here demanded of them.

Einstein, Schrodinger and von Laue did not regard the new interpretation as conclusive or convincing.

In recent years, however, various younger physicists also have taken their stand against the "orthodox" interpretation, and some have made counter-proposals, which we shall discuss below.

The work of the opponents of the Copenhagen interpretation can be divided into three groups.

The first and most numerous group takes over the interpretation of experiments from the Copenhagen theory without exception, at least in so far as experiments which have hitherto been performed are concerned, but it declares itself dissatisfied with the language used, i.e., the underlying philosophy, and replaces it by another.

The work of Alexandrow, Blochinzew, Bohm, Bop, de Broglie, Fenyes and Weizel belongs to this group.

The second group attempts actually to alter the quantum theory, so that the new theory, although it gives the same results as the old one in many cases, by no means does so in all cases.

The best-constructed attempt in this direction is by Janossy.

The third group, finally, expresses rather its general dissatisfaction with the quantum theory, without making definite counter-proposals, either physical or philosophical in nature.

The statements of Einstein, von Laue, Schrodinger, and recently of Renninger belong to this group.

However, all the opponents of the Copenhagen interpretation do agree on one point.

It would, in their view, be desirable to return to the reality concept of classical physics or, more generally expressed, to the ontology of materialism; that is, to the idea of an objective real world, whose smallest parts exist objectively in the same way as stones and trees, independently of whether or not we observe them.

We shall explain once more, in section III of this essay, that this is impossible, or only partly possible, although we shall not be able after so many discussions of the problem to advance any new arguments.

For the moment we shall subject the various counter-proposals against the Copenhagen interpretation to a short criticism; the details of the "orthodox" quantum theory will be supposed known to the reader.

(1) (a) Bohm tries to connect particle orbits with the waves in configuration space.

de Broglie also has recently taken up this idea to some extent.

For Bohm, the particles are "objectively real" structures, like the point masses of classical mechanics.

The waves in configuration space also are objective real fields, like electric fields; but the question of course remains open whether configuration space is a "real" space.

Only our uncertainty concerning the previous history of the system, and the properties of the measuring apparatus, are responsible for the statistical nature of our predictions.

Bohm has been able to carry out this idea in such a way that the results for any experiment are the same as in the Copenhagen interpretation.

The first consequence of this is that Bohm's interpretation cannot be refuted by experiment, and this is true of all the counter-proposals in the first group.

From the fundamentally "positivistic" (it would perhaps be better to say "purely physical") standpoint, we are thus concerned not with counter-proposals to the Copenhagen interpretation, but with its exact repetition in a different language.

The language used, however, is so different from the usual one that one at first supposes a difference in the physical assumptions also; thus, in Bohm's language, one must assert, as Pauli had already pointed out, that electrons in a stationary state without angular momentum are always at rest.

This looks, at first, like a contradiction to experiment, because it is well known that any measurement of electron momenta gives the momentum distribution $|\psi(p)|^2$.

To this, however, Bohm can reply that the measurement itself can no longer be evaluated by means of the former laws; that a normal evaluation of the result of measurement would indeed lead to $|\psi(p)|^2$, but that when the quantum theory (particularly the "quantum potentials" introduced ad hoc by Bohm) for the measuring apparatus is taken into consideration, the conclusion that the electrons in a stationary state are in "reality" always at rest is admissible.

In addition, we find that the quantum potentials introduced by Bohm in this connection have very remarkable properties, e.g. they differ from zero at arbitrarily great distances.

At this price, Bohm considers himself able to assert: "We do not need to abandon the precise, rational and objective description of individual systems in the realm of quantum theory."

This objective "description," however, reveals itself as a kind of "ideological superstructure." which has little to do with immediate physical reality; for the "hidden parameters" of Bohm's interpretation are of such a kind that they can never occur in the description of real processes, if the quantum theory remains unchanged.

In order to escape this difficulty, Bohm does in fact express the hope that in future experiments (e.g. in the range beyond 10^{-13} cm) the hidden parameters may yet play a physical part, and that the quantum theory may thus be proved false.

Bohm, however, is likely to say, when such hopes are expressed, that they are similar in structure to the sentence: "We may hope that it will later turn out that sometimes $2 \times 2 = 5$, for this would be of great advantage for our finances."

In actual fact, the fulfillment of Bohm's hopes would cut the ground from beneath not only the quantum theory, but also Bohm's interpretation.

Of course, it must at the same time be emphasized that the analogy just mentioned, although complete, does not represent a logically compelling argument against a possible future alteration of the quantum theory in the manner suggested by Bohm.

For it would not be fundamentally unimaginable that, for example, a future extension of mathematical logic might give a certain meaning to the statement that in exceptional cases $2 \times 2 = 5$, and it might even be possible that this extended mathematics would be of use in calculations in the field of economics.

We are, nevertheless, actually convinced, even without cogent logical grounds, that such changes in mathematics would be of no help to us financially.

The author has therefore never understood how the mathematical proposals which Bohm indicates as a possible realization of his hopes could be used for the description of physical phenomena.

If we disregard this possible alteration of the quantum theory, then Bohm's language as we have already pointed out, says nothing about physics that is different from what the Copenhagen language says.

There then remains only the question of the suitability of his language.

Besides the objection already made, that in speaking of particle orbits we are concerned with a superfluous "idealogical superstructure," it must be particularly mentioned here that Bohm's language destroys the symmetry between p and q which is implicit in quantum theory.

$|\psi(q)|^2$ indeed denotes the probability distribution in position space, but $|\psi(p)|^2$ in his theory does not denote that in momentum space.

Since the symmetry properties always belong to the intrinsic physical substance of a theory, it is difficult to see what is gained by omitting them in the corresponding language.

The same objection applies to de Broglie's attempts to introduce pilot waves; here also $|\psi(q)|^2$ represents the probability distribution in position space, but $|\psi(p)|^2$ does not represent that in momentum space.

(b) A similar objection can be raised, in a somewhat different form, against the statistical interpretations of Bop and Feynes.

These interpretations again adhere entirely at first, as regards physical consequences, to the Copenhagen interpretation; they are thus, in the positivistic sense, isomorphic with it, as is Bohm's.

However, in the language they use, they violate the symmetry between wave and particle, which has to be regarded as an essential feature of quantum theory since Bohr's work in 1927 and the investigations of Jordan, Klein and Wigner.

Bopp and Feynes consider the particles as objective physical realities, more or less in the sense of materialistic ontology, but not the (three-dimensional) material waves or radiation waves in the formulation of Jordan, Klein and Wigner.

There is, however, no reason in the quantum theory to prefer particles to waves or vice versa.

Bopp considers the appearance or disappearance of a particle as the real fundamental process of quantum theory, and he interprets the laws of quantum mechanics as a special case of correlation statistics for such events.

Such an interpretation, as Bopp has shown, can be carried out without contradiction, and throws light upon the interesting relations between quantum theory and correlation statistics.

The symmetry between corpuscle and wave, however, could only be ensured if the corresponding correlation statistics were developed for three-dimensional waves as well, the question being to some extent left open whether the particles or the waves are to be considered as the "actual" reality.

Such an extension of Bopp's ideas has not yet been attempted.

Whereas Bopp otherwise takes expressly the standpoint of the ordinary quantum theory, Fenyes considers that large deviations are "basically" possible.

For example, he says that "the existence of the uncertainty principle" (which he connects with certain statistical relations) "by no means renders impossible the simultaneous measurement, with arbitrary accuracy, of position and velocity".

Fenyes does not, however, state what nature such measurements would have in practice, and his considerations therefore seem to remain abstract mathematics.

Weizel, whose proposals are akin to those of Bohm and of Fenyes, relates the "hidden parameters" to a new kind of particle, the "zeron," which is not otherwise observable.

Such a concept, however, runs into the danger that the interaction between the real particles and the zerons dissipates the energy among the many degrees of freedom of the zeron field, so that the whole of thermodynamics becomes a chaos.

Weizel has not explained how he proposes to avoid this danger.

Furthermore, the same objections may be made to his proposals as to the other work hitherto discussed.

(c) The standpoint of the entire group of publications mentioned above can perhaps best be defined by recalling the similar discussion of the special theory of relativity.

Anyone who was dissatisfied with Einstein's negation of absolute space and absolute time could then argue somewhat as follows.

The non-existence of absolute space and absolute time is by no means proved by the special theory of relativity.

It has been shown only that true space and true time do not occur in any ordinary experiment, but if this aspect of the laws of Nature has been correctly taken into account, and thus the correct "apparent" times have been introduced for moving co-ordinate systems, there would be no arguments against the assumption of an absolute space.

It would even be plausible to assume that the center of gravity of our Galaxy is (at least approximately) at rest in absolute space.

The critic of the special theory of relativity might add that we may hope that future measurements will allow the definition of absolute space (that is, of the “hidden parameter" of the theory of relativity), and that the theory of relativity will thus be refuted.

It is seen at once that this argument cannot be refuted by experiment, since it as yet makes no assertions which differ from those of the special theory of relativity.

Such an interpretation of the theory of relativity, however, would destroy, at least in the language used, just the decisive symmetry property of the theory of relativity, namely the Lorentz invariance, and it must therefore be considered inappropriate.

The analogy to the quantum theory is obvious.

The laws of quantum theory are such that the "hidden parameters," invented ad hoc, can never be observed.

The decisive symmetry properties are thus destroyed if we introduce the hidden parameters as a fictitious entity into the interpretation of the theory.

(d) The work of Blochinzew and Alexandrow is quite different, in its statement of the problem, from those discussed above; these authors, expressly and from the first, restrict their objections to the philosophical side of the problem.

At the physical level they accept the Copenhagen interpretation unreservedly.

The external form of the polemic is so much the sharper: "Among the different idealistic trends in contemporary physics, the so-called Copenhagen school is the most reactionary. The present article is devoted to the unmasking of the idealistic and agnostic speculations of this school on the basic problems of quantum mechanics", writes Blochinzew] in his introduction.

The acerbity of the polemic shows that here it is a matter not of science alone, but of a confession of faith.

The aim is expressed at the end with a quotation from the work of Lenin: "However marvelous, from the point of view of a common human intellect, the transformation of the imponderable ether into ponderable matter, however strange the electron's lack of any but electromagnetic mass, however unusual the restriction of the mechanical laws of motion to but one realm of natural phenomena and their subordination to the deeper laws of electromagnetic phenomena, and so on — all this is but another confirmation of dialectical materialism."

Although the hypotheses in the work of Blochinzew and Alexandrow thus originate outside science, the discussion of their arguments is nevertheless very instructive.

Here, where the task is to rescue materialistic ontology, the attack is chiefly made against the introduction of the observer into the interpretation of the quantum theory.

Alexandrow writes: "We must therefore understand by 'result of measurement,' in the quantum theory, only the objective effect of the interaction of the electron with a suitable object.

Mention of the observer must be avoided, and we must treat objective conditions and objective effects.

A physical quantity is an objective characteristic of the phenomenon, but not the result of an observation."

According to Alexandrow, the wave function ψ characterizes the "objective" state of the electron.

In his presentation, Alexandrow overlooks the fact that the interaction of a system with a measuring apparatus, if the apparatus and the system are regarded as cut off from the rest of the world and are treated as a whole according to quantum mechanics, does not as a rule lead to a definite result (e.g., the blackening of a photographic plate at a given point).

If the defense against this reasoning is that "in reality" the plate is blackened at a given point after the interaction, the rejoinder is that the quantum-mechanical treatment of the closed system electron + plate is no longer being applied.

It is the "factual" character of an event describable in terms of the concepts of daily life which is not automatically contained in the mathematical formalism of the quantum theory, and which appears in the Copenhagen interpretation by the introduction of the observer.

Of course, the introduction of the observer must not be misunderstood to imply that some kind of subjective features are to be brought into the description of Nature.

The observer has rather only the function of registering decisions, i.e., processes in space and time, and it does not matter whether the observer is an apparatus or a human being; but the registration, i.e., the transition from the possible to the actual, is absolutely necessary here, and cannot be omitted from the interpretation of the quantum theory.

It must also be pointed out that in this respect the Copenhagen interpretation of quantum theory is in no way positivistic.

For whereas positivism is based on the sensual perceptions of the observer as the elements of reality, the Copenhagen interpretation regards things and processes which are describable in terms of classical concepts, i.e., the actual, as the foundation of any physical interpretation.

Blocjinzew formulates matters slightly differently from Alexandrow: "In quantum mechanics we describe, not the state of a particle in 'itself' but the fact that the particle belongs to this or that assembly. This belonging is completely objective, and does not depend on statements made by the observer".

Such formulations, of course, take us very far (probably too far) from materialistic ontology.

For, in classical thermodynamics for example, things are different.

The determination of the temperature of a system implies to the observer that the system is just one sample out of a canonical ensemble, and thus he may consider it as possibly having different energies.

“In reality," however, it has only one definite energy at a given time, and none of the others is realized; the observer has therefore been deceived if he considered a different energy at that moment as possible.

There are indeed difficulties at this point in the quantum theory, with the words "in reality”; we shall discuss this below.

If, however, a "completely objective" character is ascribed to a particle's belonging to a quantum-mechanical assembly (especially for a mixture of states), the word "objective" is used in a somewhat different sense from classical physics; for, "belonging to an assembly" always means in classical physics, at least where a past event is concerned, a statement also about the observer's degree of knowledge of the system.

Thus one sees: such concepts as "objective reality" have no immediately evident meaning, when they are applied to the situation which one finds in atomic physics.

Above all, we see from these formulations how difficult it is when we try to push new ideas into an old system of concepts belonging to an earlier philosophy, or, to use an old metaphor, when we attempt to put new wine into old bottles.

Such attempts are always distressing, for they mislead us into continually occupying ourselves with the inevitable cracks in the old bottles, instead of rejoicing over the new wine.

(2) Unlike the investigations discussed above, the work of Janossy makes its attack on the "orthodox" quantum theory entirely on the firm ground of physics.

Janossy has realized that the assumption that quantum mechanics is rigorously valid compels us to depart from the reality concept of classical physics, and he therefore seeks to alter quantum mechanics in such a way that, although many of the results remain true, its structure approaches that of classical physics.

His point of attack is what is called "the reduction of wave-packets", i.e., the fact that the wave function representing the system changes discontinuously when the observer takes cognizance of a result of measurement.

Janossy asserts that this reduction cannot be deduced from Schrodinger's equation, and he believes that he can conclude from this that there is an inconsistency in the "orthodox" interpretation.

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 (in the formalism, always for a statistical mixture of states), i.e., the actual is selected from the possible, which is done by the "observer," in the usual nomenclature.

The underlying assumption is that the interference terms are in the actual experiment removed by the partly undefined interactions of the measuring apparatus, with the system and with the rest of the world (in the formalism, the interaction produces a " mixture”).

Janossy now tries to alter quantum mechanics by the introduction of damping terms, in such a way that the interference terms disappear of themselves after a finite time.

Even if this corresponded to reality (and there is no reason to suppose this from the experiments which have yet been performed), there would still remain a number of alarming consequences of such an interpretation, as Janossy himself points out (e.g., waves which are propagated faster than the velocity of light, interchange of the time sequence of cause and effect for moving observers, i.e., distinction of certain co-ordinate systems), so that we should hardly be ready to sacrifice the simplicity of quantum theory for this kind of view until we are compelled by experiments to do so.

(3) Among the remaining opponents of the "orthodox" quantum theory, Schrodinger takes an exceptional position, in as much as he would ascribe the "objective reality" not to the particles, but to the waves, and is not prepared to interpret the waves as "probability waves only."

In his work "Are there quantum jumps?" he attempts to deny the existence of quantum jumps altogether.

Now Schrodingers' work, first of all, contains some misunderstandings of the usual interpretation.

He overlooks the fact that only the waves in configuration space, that is the transformation matrices, are probability waves in the usual interpretation, while the three-dimensional material waves or radiation waves are not.

The latter, according to Bohr and to Klein, Jordan and Wigner, have just as much (and just as little) "objective reality" as particles; they have no direct connection with probability waves, but have a continuous density of energy and of momentum, like a Maxwell field.

Schrodinger therefore rightly emphasizes that at this point the processes can be conceived of as being more continuous than they usually are.

Of course, Schrodinger cannot hereby remove the element of discontinuity from the world, which is found everywhere in atomic physics (very obviously, for instance, on the scintillation screen).

In the usual interpretation of quantum theory, it is contained in the transition from the possible to the actual.

Schrodinger himself makes no counter-proposal as to how he intends to introduce the element of discontinuity, everywhere observable, in a different manner from the usual interpretation.

The criticism of quantum theory which has been expressed at times by Einstein and von Laue starts, like the other publications discussed so far, from the fear that the quantum theory might deny the existence of an objectively real world, and so might cause the world to appear in some way (by a misunderstanding of the tenets of idealistic philosophy) as an illusion.

The physicist must, however, postulate in his science that he is studying a world which he himself has not made, and which would be present, essentially unchanged, if he were not there.

Although this problem has already received detailed treatment in the literature, we shall give here a further analysis, showing to what extent this basis of all physics has been maintained in the Copenhagen interpretation of quantum theory.

III. We begin by recalling some of the considerations of Gibbs' thermodynamics, which Bohr has always pointed to as an especially clear application of the theory of knowledge in physics.

As an example, we imagine a piece of metal which can emit electrons in consequence of thermal motion (classical mechanics is supposed valid for the electrons); let a measuring apparatus be placed in the neighborhood, which registers the emission of an electron with a velocity above a certain limit by an irreversible process (e.g. the blackening of a photographic plate.

Let the apparatus be adjusted to such a threshold velocity of the triggering electrons from the metal that their emission takes place only infrequently, e.g. at an average interval of some hours.

The measurement of the temperature of the piece of metal leads to an "objective" determination of a property of the metal, which we express mathematically by regarding the "metal" system as a sample arbitrarily selected from a canonical ensemble.

Here "objective" means that any thermometer can be used, provided that it is usable as a measuring instrument, and that the results of the temperature measurement do not depend on either the measuring instrument or the observer.

If the metal and the measuring apparatus are entirely separate from the rest of the world, this system has also a constant energy, whose value is not exactly known, on account of the canonical distribution.

If, however, the metal is in contact with the external world, its energy varies with time and oscillates in temperature equilibrium about the mean value, in the manner indicated by the canonical distribution.

If the canonical ensemble of the mechanical system metal + measuring apparatus is followed by Newtonian mechanics, this ensemble evolves in the course of time in such a way that it contains a continually increasing proportion of states in which the photographic plate of the measuring apparatus is blackened (it being assumed unexposed at the start of the experiment).

The probability that the measuring apparatus will respond can hence be calculated, but the exact instant cannot be predicted.

If every detail of the system were known at the start of the experiment, we should have been able to determine the instant exactly beforehand, provided that the system was cut off from the external world.

The statement of the temperature would then have been completely meaningless.

If, however, the system was connected with the external world, even a knowledge of every detail of the metal and of the measuring apparatus in the beginning would have been of no avail for predicting the result of the experiment, because we do not know every detail of the external world.

We have till now called this entire description "objective," and have given the reasons for doing so.

Nevertheless, it also contains a "subjective" element, as we shall now see.

Namely, in the absence of an observer, the mathematical representation of the system would go on changing continuously, in the way we have outlined.

If, however, the observer is present, he will suddenly register the fact that the plate is blackened.

The transition from the possible to the actual is thereby completed as far as he is concerned; he correspondingly alters the mathematical representation discontinuously, and the new ensemble contains only the blackened photographic plate.

This discontinuous change is naturally not contained in the mechanical equations of the system or of the ensemble which characterizes the system; it corresponds exactly to the "reduction of wave-packets" in the quantum theory, as we shall explain below.

We see from this that the characterization of a system by an ensemble not only specifies the properties of this system, but also contains information about the extent of the observer's knowledge of the system.

To this extent, the use of the word "objective" for the characterization of the system by the ensemble is problematical.

After these preliminary remarks, let us return to quantum mechanics.

According to the situation, an individual atomic system can be represented by a wave function or by a statistical mixture of such functions, i.e., by an ensemble (mathematically, by a density matrix).

If the system interacts with the external world, only the second representation is possible, since we do not know the details of the "external world” system.

If the system is closed, we may in some circumstances have, at least approximately, a "pure case," and the system is then represented by a vector in Hilbert space.

The representation is, in this particular case, completely "objective," i.e, it no longer contains features connected with the observer's knowledge; but it is also completely abstract and incomprehensible, since the various mathematical expressions $\psi(q)$, $\psi(p)$, etc., do not refer to real space or to a real property; it thus, so to speak, contains no physics at all.

The representation becomes a part of the description of Nature only by being linked to the question of how real or possible experiments will result.

From this point we must take into consideration the interaction of the system with the measuring apparatus and use a statistical mixture in the mathematical representation of the larger system composed of the system and the measuring apparatus.

It might appear that this could in principle be avoided if it were possible to separate the system and the measuring apparatus, as a compound system, entirely from the external world.

However, Bohr has rightly pointed out on many occasions that the connection with the external world is one of the necessary conditions for the measuring apparatus to perform its function, since the behavior of the measuring apparatus must be capable of being registered as something actual, and therefore of being described in terms of simple concepts, if the apparatus is to be used as a measuring instrument at all, and the connection with the external world is therefore necessary.

The compound system of system and measuring apparatus is therefore now described mathematically by a mixture, and thus the description contains, besides its objective features, also the previously discussed statements about the observer's knowledge.

If the observer later registers a certain behavior of the measuring apparatus as actual, he thereby alters the mathematical representation discontinuously, because a certain one among the various possibilities has proved to be the real one.

The discontinuous "reduction of wave-packets," which cannot be derived from Schrodinger's equation, is thus, exactly as in Gibbs' thermodynamics, a consequence of the transition from the possible to the actual.

Of course it is entirely justified to imagine this transition, from the possible to the actual, moved to an earlier point of time, for the observer himself does not produce the transition; but it cannot be moved back to a time when the compound system was still separate from the external world, because such an assumption would not be compatible with the validity of quantum mechanics for the closed system.

We see from this that a system cut off from the external world is potential but not actual in character, or, as Bohr has often expressed it, that the system cannot be described in terms of the classical concepts.

We may say that the state of the closed system represented by a Hilbert vector is indeed objective, but not real, and that the classical idea of "objectively real things" must here, to this extent, be abandoned.

The characterization of a system by its Hilbert vector is complementary to its description in terms of classical concepts, in a similar manner to the way in which the statement of the microscopic state is complementary in Gibbs' thermodynamics to the statement of the temperature.

The description of a fact can be effected in terms of classical concepts in just the approximation in which classical physics can be used.

The mathematics of quantum theory can be used for this description as well, i.e., the boundary between the object in quantum theory and the observer who describes or measures in time and space can be pushed further and further in the direction of the observer.

In this case the measuring apparatus must be characterized as a statistical mixture, and account must be taken of the fact that the individual states in such a mixture are again altered by interaction with the observer.

Knowledge of the "actual" is thus, from the point of view of the quantum theory, by its nature always an incomplete knowledge.

For the same reason, the statistical nature of the laws of microscopic physics cannot be avoided.

The criticism of the Copenhagen interpretation of the quantum theory rests quite generally on the anxiety that, with this interpretation, the concept of "objective reality" which forms the basis of classical physics might be driven out of physics.

As we have here exhaustively shown, this anxiety is groundless, since the "actual" plays the same decisive part in quantum theory as it does in classical physics.

The Copenhagen interpretation is indeed based upon the existence of processes which can be simply described in terms of space and time, i.e., in terms of classical concepts, and which thus compose our "reality" in the proper sense.

If we attempt to penetrate behind this reality into the details of atomic events, the contours of this "objectively real"world dissolve - not in the mist of a new and yet unclear idea of reality, but in the transparent clarity of a mathematics whose laws govern the possible and not the actual.

It is of course not by chance that "objective reality" is limited to the realm of what Man can describe simply in terms of space and time.

At this point we realize the simple fact that natural science is not Nature itself but a part of the relation between Man and Nature, and therefore is dependent on Man.

The idealistic argument that certain ideas are a priori ideas, i.e., in particular come before all natural science, is here correct.

The ontology of materialism rested upon the illusion that the kind of existence, the direct "actuality" of the world around us, can be extrapolated into the atomic range.

This extrapolation, however, is impossible.

Since all counter-proposals hitherto made against the Copenhagen interpretation have found themselves compelled to sacrifice essential symmetry properties of the quantum theory, we may well suppose that the Copenhagen interpretation is unavoidable if these symmetry properties, like the Lorentz invariance, are held to be a genuine feature of Nature; and every experiment yet performed supports this view.