

**The Present Status of Quantum Mechanics**

By E. Schrödinger, Oxford.

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§ 1. *The Physics of Models.*

In the second half of the last century an idealized description of the physical sciences emerged from the great successes of the kinetic theory of gases and the mechanical theory of heat. It was the crown on centuries of research and was the ultimate realization of millenia of expectation. It is called the classical description of physics and has the following main characteristics.

Using the experimental data, but without neglecting the imagination, one constructs a picture of the physical objects of which one wants to explain the experimentally observed behaviour. This picture is far more detailed than any careful observation could ever reveal. In all its exactness it is like a mathematical construct or a geometrical figure, which can be completely determined from a number of *determining elements*, just like in a triangle for example, one side and its adjacent angles completely determine the third angle, the other two sides, the three heights, the radius of the inscribed circle, etc. There is an important difference between this picture and a geometrical figure, however, namely that it is also fully determined in the fourth dimension, i.e. the *time*. That is to say, it is a construct that changes with time (as is obvious), that is, it can be in different states, and when one such state is given by the necessary determining elements, then not only are all other characteristics determined at a given moment in time, but even at any later time; similarly to how the configuration at the basis of a triangle determines that at the top. It is part of the nature of the construct to change in a certain way. That is, when it is left undisturbed in a particular initial state, it will traverse a particular sequence of states in a continuous manner, attaining each successive state at a certain fully determined point in

time. This is the nature of its being, it is the hypothesis, as mentioned above, which was assumed for intuitive reasons.

Obviously, one was not so naive as to think that one could actually determine exactly what would happen in the world. To make clear that this is not the case, the precise description as given above, is called a *model*. The extreme precision stipulated, though it can in practice never be attained, has the simple justification that the consequences of any particular hypothesis about the model can be checked without introducing new assumptions in the middle of the long calculations needed to derive these consequences. The way to go is in principle completely determined, and a 'clever clog' might be able to read them straight from the given data. In that way, one knows at least where the arbitrary assumptions have been made, and hence where they have to be improved if the model does not agree with the observations. If, after many different types of experiments, the object behaves just like the model, then one is pleased, and believes that the model is in essence a true picture of the real system. If, on the other hand, it no longer agrees with a new or refined experiment, this does *not* mean that one is then less pleased. For, in principle this is the way in which the model, and thus our understanding of reality, is constantly improved and adjusted

The main aim of the classical method using a precise model is to isolate the necessary arbitrariness in the assumptions, almost like the yolk and the white of an egg, to allow for the adjustment to improved experience. Perhaps this method is based on the belief that *somehow* the initial state *really* determines the evolution completely, i.e. that a *complete model* which corresponds *exactly* with reality would determine the outcome of all experiments precisely. It is, however, rather more probable that the adjustment process is infinite, and that a "complete model" is a contradiction in terms, a bit like "the largest integer".

A clear understanding of what is meant by a classical model, its determining elements, and its state, is fundamental in all the following. Especially, one must not confuse a *particular model* with a *particular state thereof*. It is perhaps best to give an example. Rutherford's model of the hydrogen atom consists of two point masses. One can take as determining elements for example the two sets of 3 right-angled coordinates of the two points, and the two sets of 3 components of their velocities in the direction of the coordinate axes - twelve in total. Instead, one can also choose: the coordinates and velocity components of the centre of mass, and in addition, the distance between the two point masses, the two angles determining the direction of the line connecting the points in space, and the velocities (i.e. derivatives w.r.t. time) with which these quantities are changing in time at a given moment of time. These are of course also twelve in total. It is not part of the concept of "Rutherford model of the hydrogen atom", that these determining elements should have particular values. Such values would determine a given state of the model. A clear description of the entire set of possible states - without any relation between them - constitutes the model, or the model in an arbitrary state. But the model consists of more than two points in arbitrary position and with arbitrary velocities. It also determines how every state changes in time, as long as no exterior influence is present. This knowledge is given by the following statements: the points have

masses  $m$  resp.  $M$ , and charges  $-e$  and  $e$ , and therefore attract each other with a force  $e^2/r^2$ , when  $r$  is their distance.

These statements, with particular values for  $m$ ,  $M$ , and  $e$  (but not for  $r$  of course) are part of the description of the model (not just that of a particular state).  $m$ ,  $M$ , and  $e$  are not determining elements, whereas  $r$  is. In the second choice of elements above,  $r$  is in fact the seventh, and if we use the first set of determining elements, it is given by

$$r = \sqrt{[(x_1 - x_2)^2 + (y_1 - y_2)^2 + (z_1 - z_2)^2]}.$$

The number of determining elements (often also called *variables*, as opposed to *model constants* like  $m$ ,  $M$ , and  $e$ ) is unlimited. Twelve suitably chosen variables determine all others, and hence the state, but no twelve are privileged. Other particular examples are: the energy, the three components of the angular momentum and the kinetic energy of the centre of mass. These latter elements have in fact a special property: although they are variables, i.e. they have different values in different states, they are constant in time. They are also called *constants of the motion*, as opposed to model constants.

## § 2. The Statistics of Model Variables in Quantum Mechanics.

At the heart of the present theory of Quantum Mechanics (Q.M.) is a new principle which, although it may still need reformulation, will in my opinion, remain at the heart of the theory. It says that models with determining elements which completely determine all other variables in the classical sense as outlined above, cannot describe Nature faithfully.

It might seem that for anybody who believes this, classical models are of no further use. But this is not the case. In fact, the classical models are used not just to show the contrast with the new principle, but also to express the reduced relation that remains between the same variables in these same models. This goes as follows.

A. The classical concept of *state* is lost, in the sense that at most a well-chosen *half* of the complete set of variables can be assigned a definite value; in Rutherford's model for example the 6 right-angled coordinates, or the 6 velocity components (there are other possible groups). The other half remains completely indeterminate, while other quantities can have various degrees of indeterminateness. In general *all variables* in a complete set (twelve variables in Rutherford's model) will have inaccurately determined values. The easiest way to describe the degree of inaccuracy is to choose the variables in so-called *canonically conjugate pairs* as in classical mechanics. A simple example is the coordinate  $x$  of a point mass and the component of the momentum  $p_x$  (mass times velocity) in the same direction. Such variables restrict each other in the accuracy with which they can be known simultaneously in that the product of their standard deviations (indicated by the suffix  $\Delta$ ) cannot be less than a certain universal constant, i.e.

$$\Delta x \cdot \Delta p_x > h$$

(Heisenberg's uncertainty relation.)

( $h = 1.041 \times 10^{-27}$  ergsec. In the literature one usually denotes this by an  $h$  with a stroke through it, whereas  $h$  stands for ours multiplied by  $2\pi$ .)

B. When, at a given moment in time, not all variables are fully determined by a subset, then of course they cannot be determined either at a later moment in time from accessible data at an earlier time. This might be called a breach of causality, but it is basically nothing new compared to A. When at no time a classical state is fully determined, its time evolution cannot be defined. What does change in time is the *statistics* or *probabilities*, and *those* in a fully deterministic way. During the time evolution, some variables can become more accurately defined, others less so. Overall, one can say that the total indeterminateness does not change, as follows from the fact that the restriction on the accuracies as described under A are the same at every instant of time.

What do the expressions "inaccurate", "statistics", "probability" refer to? Q.M. tells us the following. It accepts all possible variables from the classical model and declares each to be directly measurable, even with arbitrary accuracy, as long as it is considered in isolation. If, after a suitably chosen, restricted number of measurements, a maximal knowledge has been obtained as allowed by the rule under A, then the mathematical apparatus of the new theory can give us a well-defined *probability distribution* for every variable at the same time as well as any later time. This means that it gives the fraction of the number of times that each variables takes a certain value or lies in a certain small interval. It suggests that this is indeed the probability that the given variable, at a given moment in time, will assume the particular value or lie in the particular interval. A single experiment can verify this probabilistic prediction at best in an approximate way, namely only when the variable is reasonably sharply determined, i.e. it lies in all probability within a small interval. To check the prediction fully, the complete experiment, including preparatory measurements, has to be repeated *many* times, and can only take into consideration those cases in which the preparatory measurements had given exactly the same results. In those cases, the predicted statistics for a given variable, given the measured values in the preparatory measurements, should then agree with those obtained in the experiment. This is the theory.

One should be careful not to criticize this theory just because it is difficult to express: that is caused by the inadequacy of our language. However, another objection suggests itself. Hardly any classical physicist dared to propose, when constructing a model, that its determining elements are actually directly measurable at the object. Only derived consequences from the model were actually experimentally verifiable. And all experience has shown that long before the wide gap between theory and experimental technique had been bridged, the model would have changed substantially by constant adjustments to new experimental results. While the new theory declares on the one hand that the classical model is unsuitable *for describing the relation between determining elements*, it is on the other hand so bold as to prescribe what *measurements* could in principle be performed on the object. To those that invented the classical picture, this must have seemed like an incredible exaggeration of their abilities, a thoughtless presumption of future development.

Was it not a remarkable predestination that the researchers from the classical period, who did not even know what *measurement* really means, nevertheless, in their innocence, were able to give us a map to orient us as to what one can basically measure on a hydrogen atom, for example !?

I hope to clarify later that the current theory was forced upon us. For the moment, I shall continue the exposition.

### § 3. *Examples of Probabilistic Predictions.*

All predictions, therefore, are as before about determining elements of a classical model, positions and velocities of point masses, energies, angular momenta, etc. But, unlike the classical theory, only probabilities of results can be predicted. Let us have a closer look at this. Officially, it is always the case that by means of a number of presently performed measurements and their results, the probabilities of results of other measurements, either performed immediately or after some time, are derived. How does this work in practice? In some important and typical cases it is as follows.

If the energy of a Planckian oscillator is measured, the probability that one finds a value between  $E$  and  $E'$  can only be nonzero if the interval between  $E$  and  $E'$  contains a value from the sequence  $3\pi h\nu, 5\pi h\nu, 7\pi h\nu, 9\pi h\nu, \dots$

For each interval which does not contain any of these values, the probability is zero, that is, other values are excluded. These numbers are odd multiples of the model constant  $\pi h\nu$  ( $h = \text{Planck's constant}$ ,  $\nu = \text{the oscillator frequency}$ ). Two things attract the attention. First of all, there is no reference to previous measurements; these are not necessary. Secondly, the statement certainly does not lack in precision, on the contrary, it is far more accurate than any real measurement could ever be.

Another typical example is the value of the angular momentum. In Fig. 1, let  $M$  be a moving point mass, where the arrow represents the length and direction of its momentum (i.e. mass times velocity).  $O$  is an arbitrary fixed point in space, the origin of a coordinate system say; not a point with physical meaning therefore, but a geometrical point of reference. In classical mechanics, the value of the angular momentum of  $M$  w.r.t.  $O$  is the product of the length of the arrow for the momentum and the length of the perpendicular  $OF$ .

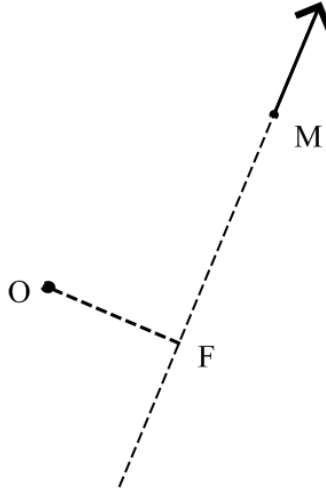


Fig. 1. *Angular momentum:*

M is a material point, O is a geometric point of reference. The arrow represents the momentum (= mass times velocity) of M. The angular momentum is then the product of the length of the arrow and the length of OF.

In Q.M. the angular momentum behaves quite similarly to the energy of the oscillator. Again, the probability is zero for every interval that does not contain a value from the following sequence:

$$0, h\sqrt{2}, h\sqrt{2 \times 3}, h\sqrt{3 \times 4}, h\sqrt{4 \times 5}, \dots$$

That is, only values from this sequence can appear. Again, this holds without reference to any prior measurement. And one can well imagine how important this precise statement is: *much* more important than the knowledge of which of these values actually occurs, or with what probability each value occurs in particular cases. Moreover, notice that the point of reference does not play any role: no matter where it is chosen, the result is always a value from this sequence. For the model, this claim makes no sense, for the perpendicular OF changes continuously as the point O is shifted, whereas the momentum arrow remains unchanged. We see from this example how Q.M. does make use of the model to read off which quantities can be measured and about which sensible predictions can be made, but on the other hand does not consider it suitable for expressing relations between these quantities.

Does one not get the feeling that in both cases the essence of what can be said has been forced into the straightjacket of a prediction for the probability that a classical variable has one or another measurement value? Does one not get the impression that this is in fact about fundamentally new properties, which have only the name in common with their classical counterparts? These are by no means exceptional cases; on the contrary, precisely the most valuable predictions of the new theory have this character. There are

indeed also problems of a type for which the original description is approximately valid. But these are not nearly as important. And those that one could construct as examples where this description is completely correct, have no meaning. "Given the position of the electron in a hydrogen atom at time  $t=0$ ; construct the statistics of the positions at a later time." This is of no interest.

It may sound as if all predictions are about the visual model. But in fact, the most valuable predictions cannot be easily visualised, and the most easily visualised characteristics are of little value.

#### § 4. *Can the Theory be Built on Ideal Quantities?*

In Q.M. the classical model plays the role of Proteus. Each of its determining elements can in certain circumstances become the subject of interest and acquire a certain authenticity. But never all at the same time; sometimes these and sometimes those, but always at most half of a complete set of variables, which would provide a clear picture of the instantaneous state of the model. What happens in the mean time with the others? Are they not real at all, or do they perhaps have a fuzzy reality; or are they always *all real*, but is it simply impossible to have simultaneous *knowledge* about them as in Rule A of § 2 ?

The latter interpretation is extremely attractive to those who are familiar with the statistical viewpoint developed during the second half of the last century, especially if one realises that it was this viewpoint that gave rise to the quantum theory, namely in the form of a central problem of the statistical theory of heat: Max Planck's theory of thermal radiation, Dec. 1899. The essence of that theory is exactly that one almost never knows all determining elements of a system, but usually *far fewer*. To describe a real object at any given moment, one therefore uses not just *one* state of the model, but rather a so-called *Gibbs ensemble*. This is an ideal, i.e. imaginary, collection of states mirroring our restricted knowledge about the real object. The object then is supposed to behave in the same way as an *arbitrary state* from this collection. This idea has had tremendous success. Its greatest triumph was in those cases where *not* all states from the collection correspond to an identical observed behaviour of the object. It turned out that the object in that case indeed varies in its behaviour exactly as predicted (thermodynamic fluctuations). It is tempting equally to relate the often fuzzy predictions of Q.M. to an ideal collection of states, one of which applies in any individual case, but one does not know which.

The one example of angular momentum shows for all that this is impossible. Imagine that in Fig. 1, the point M is placed in all possible positions w.r.t. O, and the arrow of momentum has all possible lengths and directions. Consider the collection of all these possibilities. Then one can choose the positions and arrows in such a way that, in each case, the product of the length of the arrow and the length of the perpendicular OF has one of the allowed values w.r.t. the point O. But then they do not have allowed values with respect to other points O' of course. The use of a collection of states therefore does not help.

Another example is the energy of an oscillator. In one case it has a perfectly determined value, e.g. the ground state  $3\pi\hbar\nu$ . The distance between the two point masses which make up the oscillator is then very undetermined. But, to relate this result to a statistical collection of states, the statistics of the distances should at least be bounded by that distance for which the potential energy attains the value  $3\pi\hbar\nu$ . This is not the case, however: even arbitrarily large distances are possible, albeit with rapidly decreasing probability. And that is not simply an inconsequential result of calculations, which can be ignored without seriously affecting the theory: it is, among other things, the quantum mechanical explanation of radioactivity (Gamov). There are infinitely many other examples. Notice that changes in time have not been considered. It would not help us if we were to allow the model to change in a very "unclassical" way, for example to "jump". Even for a single moment in time, it does not work. At no time is there a collection of classical model states which can describe all quantum mechanical predictions. In other words: if we want to prescribe a given (unknown) state to the model at every instant of time, or, equivalently, prescribe certain values (not exactly known to us) to all determining variables, then there is no conceivable assumption about these values which would not contradict at least part of the quantum theoretical assertions.

This is not exactly what one might expect if one is told that the statements of the new theory are always imprecise compared with the classical theory.

#### *§ 5. Are the Variables in Fact Smeared Out?*

The other alternative was to assume that the previously sharply defined variables are real, or more generally that each of these variables is realised in such a way that it exactly matches the quantum mechanical statistics of this variable at any given moment in time.

The fact that Q.M. actually makes use of an instrument, i.e. the wave function or  $\psi$ -function, that describes the amount and nature of the smearing of all variables in a consistent way, shows that this is not unrealistic. We will speak much more about this wave function below. As is usual with new concepts, it is an abstract, difficult to visualise mathematical construct, but this is of no importance. In any case, it is an imaginary entity which describes the smearing-out of all variables at an arbitrary moment in time equally clearly and precisely as their values are given in a classical model. The law for its evolution, or change in time, is also just as clear and definite as the laws of motion in the classical models, provided the system is left undisturbed. Hence the  $\psi$ -function could replace the classical variables as long as the fuzziness is restricted to atomic dimensions, which escape direct control. Indeed, one has derived from the wave function various easily visualised conceptions like for example the "cloud of negative electricity" around a positive nucleus, etc. Doubts arise, however, when we notice that the uncertainty can also pertain to coarse things that can be felt and seen, where the designation "smeared out" is simply wrong. The state of a radioactive nucleus is probably smeared out in such a way and to such an extent that neither the time of its decay nor the direction in which the  $\alpha$ -particle leaves the nucleus is determined. Inside the nucleus, this smearing out does not bother us. The particle exiting the nucleus can be visualised as a spherical wave steadily moving away from the nucleus in all directions and hitting a nearby screen in its full



extent. The screen does not light up in a steady, dim way, however. Instead, it flashes up in one instant at *one* particular place. To be fair, it flashes sometimes here, and sometimes there, because it is impossible to do the experiment with a single radioactive atom. If we use, instead of the screen, a spatially extended detector, for example a gas which is ionized by the  $\alpha$ -particle, then we find that the ion pairs are aligned along straight columns, which, when extended backwards, hit the grain of matter from which the radiation is emitted (Wilson tracks made visible by vapour droplets condensing on the ions).

It is also possible to construct very burlesque cases. Imagine a cat locked up in a room of steel together with the following hellish machine (which has to be secured from direct attack by the cat): A tiny amount of radioactive material is placed inside a Geiger counter, so tiny that during one hour *perhaps* one of its atoms decays, but equally likely none. If it does decay then the counter is triggered and activates, via a relais, a little hammer which breaks a container of prussic acid. After this system has been left alone for one hour, one can say that the cat is still alive *provided* no atom has decayed in the mean time. The first decay of an atom would have poisoned the cat. In terms of the  $\psi$ -function of the entire system this is expressed as a mixture of a living and a dead cat.

Typical about these cases is that an originally atomic uncertainty has been transformed into a coarse-grained uncertainty, which can then be *decided* by direct observation. This prevents us from considering a smeared-out model naively as an image of the real world. This does not represent anything vague or contradictory in itself. It is the difference between a blurred or poorly focussed photograph and a photograph of clouds and wafts of mist.

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(Continuation)

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§ 7. The  $\psi$ -Function as a Catalogue of Expectations.

§ 8. Theory of Measurement; Part I.

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§ 10. Theory of Measurement. Part II.

§ 6. *The Deliberate Change of Knowledge-Theoretical Viewpoint.*

In Section 4, we have seen that it is not possible to simply adopt the classical models, and assign definite values to the previously unknown or inexactly known variables after all, but which are unknown to us. In § 5 we saw that the uncertainty is not a true fuzziness either because there are cases where a simple observation can supply the missing information. Where do we now stand? The prevailing opinion takes refuge in knowledge theory to escape from this difficult dilemma. We are told that there is no distinction between the true state of an object and all that we know about it, or more precisely, all that we could know about it if we tried. *Factual*, it is said, are only: observation, experiment, measurement. When, at a certain moment in time, I have obtained the optimal knowledge about the state of a physical object as allowed by the laws of physics, then any further question about the "true state" has to be rejected as *superfluous*. This is provided I am satisfied that any further observation cannot extend my knowledge about the state in certain respects without reducing it in others (namely, by changing the state, see below).

This throws some light on the origin of the statement made at the end of § 2, which I claimed was very far-reaching: that all model variables are in principle measurable. One can hardly argue against this belief after having been forced to take heed of the philosophical principle mentioned above, which as patron of all empiricism, cannot deny the validity of any reasonable measurement.

Reality refuses to be copied by a model. One therefore has to relinquish naive realism and rely instead directly on the unquestionable thesis that (for a physicist) *reality* in the end lies in observation and measurement. Then all our future physical theories must be based entirely on the results of measurements which can in principle be carried out. Our thinking should explicitly exclude any reference to other types of reality or any model. All numbers that occur in our calculations must be declared to be possible measurement results. However, we have not arrived on this world with a clean sheet to start science afresh, but are using a certain calculational apparatus which we do not wish to discard,

especially since the successes of Q.M. We are therefore forced to dictate from our desks which measurements should be possible in principle in order to sufficiently support our calculational schemes. This allows a sharply defined value for each individual model variable (and even "half a determining set"), so each individual variable must be measurable to arbitrary accuracy. We cannot be satisfied with anything less because we have lost our naive-realistic innocence. Only our calculational scheme can indicate where Nature draws the line of ignorance, i.e. what is the *best possible knowledge* about an object. If not, then our measurement reality would depend on the diligence or laziness of the experimenter, how much effort he makes to get the information. We will have to tell him how far he could get if he were skillful enough. Otherwise, we would have to fear that, wherever we forbid any further investigation, there would in fact be more valuable information to acquire.

### § 7. *The $\psi$ -Function as a Catalogue of Expectations.*

Continuing with the exposition of the official teaching, let us turn to the  $\psi$ -function mentioned above (§ 5). It is now the instrument for predicting the probability of measurement outcomes. It embodies the totality of theoretical future expectations, as laid down in a catalogue. It is, at any moment in time, the bridge of relations and restrictions between different measurements, as were in the classical theory the model and its state at any given time. The  $\psi$ -function has also otherwise much in common with this classical state. In principle, it is also uniquely determined by a finite number of suitably chosen measurements on the object, though half as many as in the classical theory. Thus is the catalogue of expectations laid down initially. From then on, it changes with time, as in the classical theory, in a well-defined and deterministic ("causal") way - the development of the  $\psi$ -function is governed by a partial differential equation (of first order in the time variable, and resolved for  $d\psi/dt$ ). This corresponds to the undisturbed motion of the model in the classical theory. But that lasts only so long until another measurement is undertaken. After every measurement, one has to attribute to the  $\psi$ -function a curious, somewhat sudden adaptation, which *depends on the measurement result* and is therefore *unpredictable*. This alone already shows that this second type of change of the  $\psi$ -function has nothing to do with the regular development between two measurements. The sudden change due to measurement is closely connected with the discussion in § 5, and we will consider it in depth in the following. It is the most interesting aspect of the whole theory, and it is precisely *this* aspect that requires a breach with naive realism. For *this* reason, the  $\psi$ -function cannot immediately replace the model or the real thing. And this is not because a real thing or a model could not in principle undergo sudden unpredictable changes, but because from a realistic point of view, measurements are natural phenomena like any other, and should not by themselves cause a sudden interruption of the regular evolution in Nature.

## § 8. Theory of Measurement; Part I.

The rejection of realism has logical consequences. A variable *does not have* a value before it is measured. This implies that its measurement does *not* mean: determining the value that it *has*. So, what does it mean? There must surely be a criterion as to whether a measurement is good or bad, right or wrong, accurate or inaccurate - whether it deserves the name "measurement" at all. Playing round with a dial instrument near another body and now and again making a reading, surely cannot be called a measurement on the body. Now, it is rather obvious that if reality does not determine the measurement value, then at least the measured value must determine reality. That is, *after* the measurement, the only acceptable value is such as has just been measured. This means that the desired criterion can be as follows: repetition of the measurement should result in the same value. By repeating the measurement several times, I can test the accuracy of the method and show that I am not just playing a game. It is pleasing to realise that this instruction agrees exactly with the experimental procedure, in which the "true value" is also unknown beforehand. We can formulate the main point as follows:

*The organised interaction of two systems (measurement object and measurement instrument) is called a measurement on the first when a sensitive variable characteristic of the second (dial position) reproduces itself, within certain error bounds, upon immediate repetition of the procedure (with the same measurement object, which in the mean time has not been subjected to other influences).*

This explanation still leaves much more to be added, it is not a perfect definition. Empiricism is more complicated than mathematics and cannot be easily captured by smooth theorems. *Before* the first measurement, there could have been an arbitrary quantum mechanical prediction about it. *After* the measurement, the prediction is *always*: within the error bounds the same value. The catalogue of predictions (= the  $\psi$ -function) has therefore been modified by the measurement corresponding to the measured variable. If the measurement device was already known to be reliable then the first measurement immediately reduces the theoretical expectation of the variable to within the error bounds around the value found, irrespective of what the expectation was before the measurement. This is the typical abrupt change in the  $\psi$ -function upon measurement discussed above. But the catalogue of expectations in general also changes in an unpredictable way for other variables, esp. the "canonically conjugate" variable. For example, when the *momentum* of a particle could be predicted quite accurately before a measurement of its *position*, the latter more accurately than Prop. A of § 2 allows, then the prediction for the momentum must have been modified. The quantum mechanical calculational machinery does this automatically though: there is no  $\psi$ -function which contradicts Prop. A. when it is used properly to read off the expectations. Since the catalogue of expectations changes radically during a measurement, the object is then no longer suited to verify all statistical predictions in their complete extent prior to the measurement. This holds at the very least for the variable itself as it will now constantly have (nearly) the same value. That is the reason for the prescription that was given in § 2: the predictions for the probability can be checked completely, but one has to repeat the entire experiment *ab ovo*. The measurement object (or one identical to it) must be pretreated in exactly the same way so that the same catalogue of expectations (=  $\psi$ -function) exists as before the first

measurement. Then it is “repeated”. (Therefore, repeating now has a quite different meaning than previously !) All this has to be done many times. Then the predicted statistics will apply – according to the accepted view.

Notice the difference between the error bounds and error statistics *of the measurement* on the one hand, and the theoretically predicted statistics on the other. They have nothing to do with one another. They apply to the two entirely different types of *repetition* just considered.

Here we have an opportunity to further sharpen the above delimitation of a measurement. There are measurement instruments which get stuck in the position reached after a measurement. The dial could also have got stuck due to an accident. In that case one would always get the same reading, and according to our description, this would be a very accurate measurement. That is true, but it is a measurement of the instrument itself!. Indeed, our description is lacking an important point, which could not easily have been given before, namely, what exactly *is* the difference between the object and the instrument (the fact that the latter is being read, is merely superficial). We have just seen that the instrument must, when necessary, be returned to its neutral position before a control measurement is performed. Experimenters know this very well. Theoretically, this is best described by demanding that before every measurement the instrument is pretreated in the same way so that it has the same catalogue of expectations (=ψ-function) every time it is brought into contact with the object. The object, however, must not be tampered with in any way when a *control measurement* has to be made, a "*repetition of the first kind*" (which leads to the *error statistics*). That is the characteristic distinction between object and instrument. For a "*repetition of the second kind*" (intended to check the quantum predictions), there is no difference. In that case the distinction is indeed insignificant.

We conclude from this further that for a second measurement another, similar, and equally pretreated instrument can also be used; it need not be exactly the same. This is in fact often done to check the first instrument. It can even happen that two quite different instruments are related in such a way that if one makes a measurement with one after the other (repetition of the first kind!) their two indicators are in one-to-one correspondence. In that case they measure essentially the same variable of the object - the same after appropriate mapping of the scales.

### § 9. *The ψ-Function as Description of the State.*

The rejection of realism also creates obligations. From the point of view of the classical model, the predictive content of the ψ-function is very incomplete, it contains only about 50% of a complete description. From the new point of view, it must be complete, for reasons that have already been outlined in § 6. It must be impossible to add new correct statements without changing it otherwise. If not, then one does not have the right to declare all questions which it cannot answer as meaningless.

It follows that two different catalogues which are valid for the same system in different circumstances or at different times, can perhaps overlap but never in such a way that one

is entirely contained in the other. For otherwise it could be completed by additional statements, namely those in its complement. The mathematical structure of the theory satisfies this requirement automatically. There is no  $\psi$ -function which contains the same statements as another and more besides. Therefore, when the  $\psi$ -function of a system changes, either by itself or through measurement, the new function must lack certain statements contained in the original. The catalogue cannot only have new entries, it must also have deletions. But knowledge cannot not be forfeited. The deletions therefore infer that some previously correct statements have subsequently become false. A correct statement can only become false if the subject which it concerns has changed. I consider it indisputable to conclude the following:

Theorem 1. *Different  $\psi$ -functions represent different states of a system.*

If one only considers systems for which there is a  $\psi$ -function, then the converse of this theorem can be formulated as:

Theorem 2. *Identical  $\psi$ -functions represent the same state of a system.*

This converse does not follow from Theorem 1, but rather immediately from the completeness or maximality. If one were to still admit various possibilities for identical catalogues of expectations, this would be an admission that these do not provide an answer to all legitimate questions. Almost all authors assume that the above two theorems hold. They do construct a new kind of reality, completely legitimately, I believe. By the way, they are not trivial tautologies, simply new words for "state". Without the assumption of maximality of the catalogue of expectations, the change in the  $\psi$ -function could have been caused by the gathering of new information.

We have to counter another objection to Theorem 1. All the statements or knowledge that we are dealing with here are in the form of probabilities, to which the designation *right* or *wrong* does not actually apply in individual cases, but rather in a collective which is formed when the system is prepared thousands of times in the same way (followed by the same measurement, cf. § 8). That is true, but we have to declare all members of the collective to be similar since for each the same  $\psi$ -function applies, the same catalogue of statements, and we cannot make distinctions that are not included in the catalogue (compare the justification for Theorem 2). The individual cases in a collective are therefore all identical. When a statement about the collective is false, then the individual cases must also have changed; otherwise the collective would have remained the same.

#### *§ 10. Theory of Measurement. Part II.*

It has been said before (§ 7) and explained (§ 8) that every measurement suspends the law governing the steady change in time of the  $\psi$ -function, and brings about an entirely different change, which is not governed by a law but by the result of the measurement. However, it cannot be that during a measurement, other Laws of Nature hold than usual, because, objectively considered, a measurement is a natural phenomenon like any other and cannot interrupt the normal course of natural events. Since it interrupts that of the  $\psi$ -function, the latter *cannot* serve as a provisional image of the objective reality as in the classical model. But in the last chapter, something like that has nevertheless crystallized.

I'll try once again, staccato-like, to contrast the two: 1. The jumping of the catalogue of expectations is unavoidable, because, if the measurement is to make sense at all then the measured value must hold after the measurement. 2. The jump-like change is definitely not governed by the normal causal law because it depends on the measured value, which is unpredictable. 3. The change definitely entails a *loss* of knowledge (by maximality). But knowledge cannot be lost, so the *subject* must have changed - *also* during jump-like changes and *for those also* in an unpredictable manner, *not* as usual.

How can we make sense out of this? Things are not so simple. It is the most difficult and most interesting point of the theory. We obviously have to grasp the interaction between the measurement object and the measurement instrument in an objective fashion. For this we have to begin with some very abstract considerations.

It is as follows. When, for two completely separate bodies, more precisely for each individually, a complete catalogue of expectations - a maximal complement of knowledge - a  $\psi$ -function - is given, then it is obviously also given for the two together, i.e. if we consider them, not individually, but together as subject of our interest in formulating questions about the future<sup>1</sup>. But the reverse is not true. *Maximal knowledge about the total system does not necessarily imply maximal knowledge about all its parts, not even when these parts are completely separated and cannot influence one another at the given time.* It could be that part of the knowledge concerns relations between the two systems (we restrict ourselves here to two), as follows: when a certain measurement on the first system has a particular result then a measurement on the second has this or that statistics of expectations; if the same measurement has another result the second has a different expectation; for a third result, again another expectation holds for the second, etc. Thus one can go through the whole set of values that the given measurement on the first system could possibly produce. In this way, any particular measurement process, or equivalently any particular variable of the second system, can be linked to the as yet unknown value of a certain variable of the first, and vice versa of course. When this is the case, when the total catalogue contains such conditional statements, *then it cannot be maximal w.r.t. the individual systems.* For, the content of two maximal individual catalogues would already form a maximal catalogue for the two combined, there could not be additional conditional statements.

By the way, these particular predictions are not sudden new additions to the theory. They exist in every catalogue of expectations. When the  $\psi$ -function is known, and a measurement is made, yielding a certain outcome, then the new  $\psi$ -function is again known; that is all. It is simply that, in the present case, when the total system consists of two separated parts, the matter seems unusual. For, as a result, it makes sense to distinguish between measurements on the first and measurements on the second subsystem. This gives each a rightful claim to their own private maximal catalogue. On the other hand, it remains possible that part of the obtainable information for the total system has been wasted, in a manner of speaking, on conditional statements linking the

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<sup>1</sup> Obviously. We cannot have a shortage of statements about the relation between the two. Because that would mean that something could be added to the  $\psi$ -function of at least one of the bodies. And that is not possible.

two subsystems - even though the total catalogue is maximal, that is, even though the  $\psi$ -function of the total system is known.

Let's hold on a minute. This conclusion, in all its abstraction, says it all: The best possible knowledge about the whole does not necessarily imply the same for its parts. If we translate this into the language of § 9 it reads: If the whole is in a definite state, the parts might not be.

\_\_\_ How come? A system must surely be in some state or other.

=== No. A state is a  $\psi$ -function, its maximal amount of knowledge. I might not have obtained this, I might have been lazy. Then the system is in no definite state.

\_\_\_ OK. But then the agnostic ban on asking questions is not yet in force, and I could convince myself in this case that the part system must be in some state or other even though I don't know it.

=== Stop. Unfortunately not. "I just don't know" does not exist because there is maximal knowledge about the total system.

*The insufficiency of the  $\psi$ -function as replacement of a model is based entirely upon the fact that one does not always possess it. If one does, then it gives a good description of the state. But on occasions one does not have it, even though one might expect to. And in those cases one cannot postulate that "in truth it is well-defined, but we just don't know it". The established point of view forbids it. For, the  $\psi$ -function is a sum of knowledge, and knowledge that nobody knows is no knowledge.*

Let us continue. It can surely not happen that part of the knowledge floats *between* the two systems in the form of joint conditional statements if the two have been brought together from opposite ends of the Earth without interaction. Because then the two systems don't know anything about each other. A measurement on one can then impossibly give a clue about the other. If an "entanglement of statements" exists then this can apparently only have come about if the two systems previously were in a proper sense one system, i.e. they interacted, and have left *remnants* on one another. When two separated bodies that each are maximally known come to interact, and then separate again, then such an *entanglement* of knowledge often happens. The combined catalogue of expectations consists initially of the logical sum of the two catalogues; during the interaction it develops deterministically according to a known law (there is no measurement taking place). The knowledge remains maximal, but the result is that, after the bodies have separated again, it cannot be split into a logical sum of knowledge about the individual bodies. What remains of the latter may have become very far below maximal. - Notice the big difference with the classical model theory, where for given initial states and known interaction, the final states are individually known.

The measurement process described in § 8 falls exactly within this general scheme when it is applied to the total system of measurement object and measurement instrument. When making the same objective picture of this procedure as of any other, we might hope to clarify, if not do away with, the curious jumping of the  $\psi$ -function. One of the bodies is thus the measurement object, the other is the instrument. To avoid any external disturbance, we engineer it in such a way that the instrument automatically acts on the object and retracts from it at set times by means of an inbuilt clock. We delay the reading of the instrument, because we first want to investigate what happens "objectively". But



we make sure that the result is automatically registered by the instrument, as is commonly done nowadays.

What is now the situation after the automatically executed measurement? As before we possess a maximal catalogue of expectations for the total system. The registered measurement value is not part of it of course. With regard to the instrument, the catalogue is very incomplete therefore: it does not even tell us where the writing feather has left its traces. (Remember the poisoned cat !) That is, our knowledge has been dissolved into conditional statements: *if* the marker is at number 1 *then* this or that holds for the measurement object, *if* it is at number 2, *then* this or the other, if at number 3 then something else again, etc. Did the  $\psi$ -function of the measurement object make a jump? Or has it evolved deterministically (according to the partial differential equation)? Neither. It ceased to exist. It has got entangled with that of the measurement instrument as a result of the deterministic law of the combined  $\psi$ -function. *The catalogue of expectations of the object has split into a conditional division of catalogues*, like a map that has been carefully dissected. In addition, each section is given a probability for it to occur, derived from the original catalogue of the object. But *which* of the possibilities actually occurs, which part of the map is used for the onward journey, that can only be learnt by actual inspection.

And if we do not inspect ? Say, it has been registered photographically, and the film is exposed to light before it was developed. Or we inserted black paper instead of a film by mistake. Well, then we did not just learn nothing as a result of the failed experiment, we have actually lost information. That is not so surprising. Of course an external disturbance spoils the knowledge about a system. The disturbance has to be organised very carefully in order that it can later be recovered.

What did we gain by this analysis? First of all an insight into the branched splitting of the catalogue of expectations, which happens entirely continuously by embedding into a combined catalogue of object and instrument. The object can only be resolved from this immersion if the living subject actually takes note of the measurement result. This will have to happen at some stage if we want to call the procedure a measurement - no matter how much we try to treat the whole process objectively. And that is the *second* insight that we have gained: Only after inspection, which decides which branch is taken, does something discontinuous, a jump, take place. One could call it a mental act, since the object has already been turned off, is no longer physically involved. What happened to it has passed. But it would not be correct to say that the  $\psi$ -function of the object now changes abruptly as a result of the mental act, while it *otherwise* changes according to a partial differential equation, independently of the observer. Because it had been lost, it was no more. That which does not exist cannot change. It is reborn, reconstituted from the entangled knowledge. It is resolved by an act of observation which is definitely not a physical disturbance of the object. There is no continuous route from the form in which the  $\psi$ -function was last known to the new form in which it re-emerges - it leads via annihilation. Contrasting the two forms, they appear as a jump. In fact, something important has happened in between, namely the interaction of the two bodies during which the object has no private catalogue of expectations, nor has it a right to one, as it is not independent.

**The Present Status of Quantum Mechanics**

By E. Schrödinger, Oxford.

(The end.)

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*§ 11. The Lifting of Entanglement. The Result Depends on the Will of the Experimenter.*

We return to the general case of entanglement, not just the special case of a measurement process. Suppose that the catalogues of expectations of two bodies A and B have been entangled as a result of interaction in the past. Then I can take one, say B, and complete my knowledge about it, which has become submaximal, by successive measurements. I claim that as soon as I have succeeded in doing this, and no sooner, the entanglement with A has been rescinded and, moreover, the measurements on B combined with the conditional statements allow us to obtain maximal knowledge of A as well.

To see this, note first of all that the knowledge about the total system will remain maximal, as it will not be spoiled by good and accurate measurements. Secondly, conditional statements of the form "when for A ... then for B ..." no longer exist as soon as we have a maximal catalogue for B. For such a catalogue is *not* conditional, and nothing concerning B can be added to it. Thirdly, conditional statements in the reverse direction ("when for B ... then for A ...") can be converted into statements about A alone since all probabilities for B are already known unconditionally. The entanglement has therefore been completely removed. And since the knowledge about the total system has remained maximal, the only possibility is that the maximal catalogue for B is complemented by one for A.

Nor can it happen that A has become maximal through measurements on B, while B is not yet maximal. Because then all the above reasoning can be reversed, and it follows that B is also maximal. Both systems will be maximal at the same time, as claimed. Notice, by the way, that this would also be true if the measurements are not restricted to one of the two systems. Most interesting is, however, that one *can* restrict it to one of the two; that this already has the desired effect.

Which measurements on B are used is entirely up to the experimenter. He does not need to use particular variables to be able to make use of the conditional statements. He can

simply make a plan to arrive at a maximal knowledge of B, even when he knew nothing about B. It is also no harm if he carries out this plan to the end. To save himself superfluous work, he might consider if he has already achieved his aim, however.

Which A-catalogue is thus obtained depends of course on the measured values for B (before the entanglement has vanished completely; not on those of the superfluous measurements). Now suppose that in a certain case I have found a catalogue for A in this way. Then I can think and wonder if I might have found a *different* catalogue if I had used a *different* plan for measuring B. Since I did not touch A in the actual experiment nor in the imaginary experiment, the statements in the other catalogue must *also* be correct, whatever they are. They must also be entirely contained in the first, and vice versa. It must therefore be identical to the first.

Strangely enough, the mathematical structure of the theory does not satisfy this condition automatically. Even stronger, one can construct examples in which this condition is necessarily violated. It is true that in each experiment, only one set of measurements can actually be performed (on B) after which the entanglement has vanished and one does not learn any more about A with further measurements on B. But there are types of entanglement for which there exist *two specific measurement programmes* for B such that 1. the entanglement is lifted, 2. the one leads to an A catalogue to which the other cannot possibly lead, no matter what values the measurements result in. The fact is that the two classes of A catalogues that can turn up in the one respectively the other programme, are purely separated and cannot have any element in common.

Only in very special cases is the situation so clear. In general a more precise analysis is needed. Given two programmes for measurements on B and two sets of A-catalogues to which these can lead, the fact that the two sets have some elements in common is no reason to think that probably one of those will happen, so the condition is "probably satisfied". That is not enough. For, considered as measurement of the total system, the probabilities of all measurement outcomes for B are known, and after a large number of measurements, the corresponding frequencies of occurrence must establish themselves. The two sets of catalogues therefore must agree elementwise, and moreover the probabilities in the two sets must be the same. And that holds not just for two programmes, but for the infinity of possible programmes. But this is not at all the case. The condition that the A catalogue obtained always be the same, whatever measurements on B have brought it about, is absolutely never satisfied.

We will consider an suitable example below.

### § 12. An Example<sup>2</sup>.

For simplicity we consider two systems with only *one degree of freedom*, i.e. each is characterised by one coordinate variable  $q$ , and one canonically conjugate variable, the momentum  $p$ . The classical picture would be a point mass which can only move on a line

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<sup>2</sup> A. Einstein, B. Podolsky and N. Rosen, Phys. Rev. **47**, 777 (1935). The appearance of this work was the impetus to the present - shall I say paper or general confession?

like a bead on an abacus.  $p$  is the product of the mass and the velocity. For the second system we denote the corresponding variables by  $Q$  and  $P$ . Whether the two are threaded on the same string or not, will not concern us here. But, whatever the case may be, it may still be useful to assume that  $q$  and  $Q$  are zero at different points. The equality  $q = Q$  therefore does not necessarily mean that the two points coincide; the systems can still be completely separate.

In the cited work it is shown that there can be an entanglement between the two systems at a certain moment in time, to which we constantly refer in the following, described by the equations

$$q = Q \text{ and } p = -P.$$

That is, I know that *when* a measurement of  $q$  is performed yielding a certain value, then an immediately executed  $Q$ -measurement will result in the same value and vice versa; and *when* a measurement of  $p$  is performed yielding a certain value, then an immediately executed  $P$ -measurement will result in the negative of that value and vice versa. One single measurement of  $q$  or  $p$  or  $Q$  or  $P$  lifts the entanglement and yields maximal knowledge about *both* systems. A second measurement on the same system affects *only that* system, not the other. The two identities can therefore not both be checked in the same experiment. But the measurement can be repeated a thousand times *ab ovo*; each time the same entanglement has to be reproduced and at will one or the other equality checked. Every time the selected equality will be confirmed.

Now suppose that in the thousand-and-first experiment, one decides not to do any more checks, but instead measures  $q$  on the first system and  $P$  on the other, and the results are

$$q = 4; \quad P = 7.$$

Is there then any doubt that for the first system, we have

$$q = 4; \quad p = -7;$$

and for the second system

$$Q = 4; \quad P = 7 ?$$

This cannot be fully verified in a single experiment, for that is never the case with quantum statements, but it is true nonetheless, because, whoever might have had doubts and decided to check anyway, could not be disappointed.

This is undoubtedly true. Each measurement is the first on its system, and measurements on separated systems cannot affect one another directly, that would be magic. It cannot be pure luck either when a thousand experiments show that fresh measurements must coincide.

The catalogue of expectations  $q=4, p=-7$  would of course be hypermaximal.

### *§ 13. Continuation of the Example: All Possible Measurements are Uniquely Entangled.*

In fact, according to Q.M., which we examine in all detail here, a prediction of this kind is not possible. Many of my friends have passified themselves with this and declare: *what a system would have told an experimenter if .....*, has nothing to do with a real measurement, and, from our knowledge-theoretical point of view, is of no consequence.

But let us make the situation absolutely clear. Let us concentrate the attention on the system with variables given by small letters  $q$  and  $p$ , and call it the "small system". It is as follows. I can ask the small system, through direct measurement, one of two questions, either about  $q$  or about  $p$ . Before doing so, I may decide to obtain the answer to one of these questions by measurement on the other, fully separate system (to be considered as auxiliary apparatus). Alternatively, I may have the intention to do so later. The small system, like a pupil at an exam, cannot possibly know whether I did so and for which question, or for which one I intend to do so later. From arbitrarily many previous experiments, I know that the pupil will always answer the first question I ask him correctly. It follows that he *knows* the answer to *both*! The fact that answering this first question makes the pupil so tired or confused that his subsequent answers are worthless, does not change this conclusion. No secondary school master would, when this scenario is repeated with thousands of pupils of equal ability, come to any other conclusion, much as he might wonder *what* makes these pupils so stupid or forgetful after answering the first question. He would not think that the fact that he looked up the answer in a handbook himself would have suggested it to the pupil, or indeed, that if he looked it up after the pupil gave an answer, the text in the pupil's notebook would have changed in the pupil's favour.

The small system must therefore have a definite answer ready for both the  $q$ -question and the  $p$ -question, just in case it is the first I am posing directly. This readiness cannot be affected one little bit by any measurement I might make on the auxiliary system (in the analogy: that the teacher looks up the question in his notebook and in doing so spoils the other side, where the other answer is, by an inkblot). Advocates of quantum mechanics claim that after a Q-measurement on the auxiliary system, the small system will have a  $\psi$ -function in which " $q$  is sharply defined but  $p$  is completely undetermined". And yet, as we said before, this has not changed one little bit the fact that the small system also has a definite answer ready for the  $p$ -question, namely the same as before.

The matter is in fact even more serious. Not only to the  $q$ -question and the  $p$ -question does the pupil have a definite answer ready, but also to thousands of other questions. And this without us being able to understand the mnemonics with which he achieves this feat.  $p$  and  $q$  are not the only quantities that can be measured. Any combination like

$$p^2 + q^2$$

also corresponds to a particular measurement according to Q.M. Now, it can be shown<sup>3</sup> that for this variable the answer can also be determined by a measurement on the auxiliary system, namely  $P^2 + Q^2$ , and the answers are in fact equal. According to the rules of Q.M., this sum of squares can only take a value from the sequence

$$h, 3h, 5h, 7h, \dots$$

The answer that my small systems has ready for the  $(p^2 + q^2)$ -question (in case it is the first one to be answered), must also be a number from this sequence. – The same holds

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<sup>3</sup> E. Schrödinger, Proc. Cambridge Philos. Soc. **31**, 555 (1935).

for the measurement of

$$p^2 + a^2 q^2,$$

where  $a$  is an arbitrary positive constant. In this case the answer must be a number from the sequence

$$a h, 3 a h, 5 a h, 7 a h, \dots$$

For each value of  $a$  we get a new question, and for each of these our small system has an answer ready from the appropriate sequence.

The remarkable thing is that these answers cannot possibly be related in the way suggested by these formulas ! For, suppose that  $q'$  is the answer for the  $q$ -question and  $p'$  that for the  $p$ -question, then

$$\frac{p'^2 + a^2 q'^2}{a h} = \text{an odd integer}$$

can impossibly be true for given values of  $p'$  and  $q'$  and *all arbitrary positive numbers*  $a$ . This is not just an operation with fictitious numbers, which one cannot actually obtain. Two of these numbers, e.g.  $q'$  and  $p'$ , can be obtained; one by direct measurement, the other by indirect measurement. And then one can convince oneself that the above expression with arbitrary  $a$ , is not an odd integer.

The lack of insight into the relation between the different prepared answers (the mnemonics of the pupil) is total, it cannot be resolved by a new type of algebra for Q.M. It is all the more disconcerting because one can prove on the other hand that the entanglement is fully determined by the two relations  $q = Q$  and  $P = -p$ . When we know that the coordinates are equal and the momenta are opposite, then there follows quantum mechanically a *very definite* one-to-one correspondence between *all possible* measurements of the two systems. For each measurement on the "small" system one can obtain the measured value by a measurement on the "large" system, and each measurement on the large system gives immediate information about the result of a particular measurement on the small system, which may or may not have been performed already (of course always in the same sense: only virgin measurements on both systems count). As soon as the two systems have been prepared in a state such that (broadly speaking) coordinates and momenta agree, then all other variables also agree (broadly speaking).

But we do not know how all these variables hang together in *one* system, even though the system must have a very definite value ready for each: for we can, if we wish, get practical knowledge of it from the auxiliary system and have it confirmed by direct measurement.

Should we now conclude that, since we do not know anything about the relation between the values of variables in *one* system, that no such relation exists; i.e. that arbitrary combinations can occur? That would mean that such a system, with only one degree of freedom, would need not just *two* numbers for its complete description, as in classical

mechanics, but many more, perhaps infinitely many. But then it is hardly likely that two systems for which *two* variables are the same, agree automatically on *all* others. One would have to also assume therefore that this is due to our clumsiness; that we are in practice unable to bring two systems into a situation in which they agree in two variables without *nolens volens* also making all other variables equal, although this would in principle not be necessary. These *two assumptions* would have to be made in order not to be embarrassed by the lack of understanding of the relation between the values of variables within one system.

§ 14. *The Time-Evolution of Entanglement. Objections to the Special Role of Time.*

One might need to be reminded that everything that was said in Chapters 12 and 13 concerned a single moment in time. Entanglement is not constant in time. Although it remains a one-to-one entanglement of all variables, the particular relations vary. That means the following. At a later time one can still get to know the values of  $q$  or  $p$  by a measurement on the auxiliary system, but these are in general *different* measurements. Which measurements they are can in simple cases be easily seen. Of course, this depends on the forces operating within both systems. Let us assume that there are no forces. For simplicity we assume that the masses of both systems are equal, denoted by  $m$ . In the classical model, the momenta  $p$  and  $P$  would remain constant as they are the velocities multiplied by the mass; and the coordinates at time  $t$ , which we denote by a subscript  $t$ ,  $(q_t, Q_t)$ , would be related to the initial values  $q$  and  $Q$  as follows:

$$q_t = q + (p/m) t$$

$$Q_t = Q + (P/m) t .$$

Let us first discuss the small system. The most natural way to describe it classically at time  $t$  is in terms of the coordinate and momentum at this time, i.e. by  $q_t$  and  $p$ . But one can also do it differently. One can replace  $q_t$  by  $q$ .  $q$  is also a determining element at time  $t$ , and even at every time  $t$ , and in fact one that does not change in time. This is similar to a certain determining element of my own person, namely my age, which can be given as the number 48, which changes in time and corresponds to  $q_t$  for the system, or by the number 1887, which is usual in official documents and corresponds to  $q$ . Now, by the above equations,

$$q = q_t - (p/m) t .$$

A similar relation holds for the second system. We therefore take as determining elements

$$\begin{array}{ll} \text{for the first system} & q_t - (p/m) t \text{ and } p \\ \text{for the second system} & Q_t - (P/m) t \text{ and } P. \end{array}$$

The advantage is that between these constantly the same entanglement persists:

$$\begin{array}{l} q_t - (p/m) t = Q_t - (P/m) t \\ p = - P, \end{array}$$

or, equivalently,

$$q_t = Q_t - 2 (P/m) t ; \quad p = - P.$$

What changes in time is therefore simply this: the coordinate of the small system is not simply determined by a coordinate measurement on the auxiliary system, but by a measurement of the aggregate quantity

$$Q_t - 2 (P/m) t.$$

One must not think that this means a measurement of  $Q_t$  and  $P$ , for that is impossible, remember. Instead, one has to imagine, as usual in Q.M., that there exists a direct measurement apparatus for this combination. For the rest, everything that was said in Chapters 12 and 13 holds for every point in time modulo this adjustment. In particular, the one-to-one entanglement of all variables at every time, with its nasty consequences.

The same holds when within each system a force operates, but in that case  $q_t$  and  $p$  are entangled with more complicated combinations of  $Q_t$  and  $P$ .

I explained all this briefly so that we can consider the following. The fact that the entanglement changes in time gives us food for thought. Do all measurements that we talked about have to be performed *instantaneously* to justify all the arguments? Could we perhaps exorcise the ghost by the remark that all measurements take time? No. For there is only one measurement needed on each system; only the first one counts, subsequent ones are anyway irrelevant. How long the measurement takes need also not concern us, as we do not intend to follow up with a second. We only have to organise the measurements in such a way that they give us the values of the variables at the same, previously known, point in time, because we have to direct the measurements to a pair of variables which is entangled at the given point in time.

— Perhaps it is not possible to organise the measurements in this way?

== Perhaps. I even suspect so. But: the *present theory* of Q.M. must require it. Because it has been so constructed that its predictions always refer to a particular point in time. Since these predictions are about measurement values, they would make no sense if the corresponding variables could not be measured at a particular time, irrespective of how long the measurement itself might take.

It does not matter, of course, when we actually get to know the result. That is theoretically of as little importance as the fact that it takes several months to integrate the differential equation for the weather of the next three days. The drastic comparison with the school exam is, taken literally, inaccurate in several respects, but in spirit it is correct. The expression "the system knows" perhaps no longer means that the result follows from the situation at one moment in time, but rather is created from a succession of situations within a finite stretch of time. But even if that were the case, it need not worry us as long as the system can somehow create the answer without any help other than that we tell it *which* question we wish to have answered; and when the answer itself refers to a *definite point in time*. This has to be assumed, good or bad, for every measurement in the present theory of Q.M; otherwise the quantum mechanical predictions would have no meaning.

However, we have hit upon a new possibility in our discussion. If we could implement the idea that the quantum mechanical predictions do not refer to, or do not always refer to a definite point in time, then this would not be required for measurements either. The



composition of counter intuitive statements would then be much more difficult as the entangled variables change in time.

The sharp definition in time of quantum predictions is also for other reasons probably mistaken. The time variable is, like any other, the result of a measurement. Can the measurement with a clock be given an exceptional status? Should it not be like any other variable, which in general does not have a sharp value, at least not together with any arbitrary other variable? When the value of *another* variable is predicted at a *definite* point in time, would it not be more reasonable if *both* were in fact inaccurate? Within the present Q.M. this suspicion can hardly be investigated. Because the time is a priori considered to be constantly accurate even though one has to admit that every time one looks at the clock, its motion will be disturbed in an uncontrollable way.

I would like to repeat that we do not have a Q.M. whose predictions do not hold for a well-defined point in time. It seems to me that this shortcoming comes to the fore in exactly the above problems. Which is not to say that it is the only trouble with these problems.

#### *§ 15. Law of Nature or Computational Trick?*

The last few years I have constantly pointed out<sup>4</sup> the fact that the "precise time" is a contradiction within Q.M., and moreover, that the special status of time is a serious obstacle to adjusting Q.M. to the *relativity principle*. Unfortunately without a shadow of a useful counter proposition. In assessing the whole present situation, as I have tried to describe it here, another remark comes to mind about the very sought-after, but not yet really attained "relativisation" of Q.M. The strange theory of measurement, the apparent jumping of the  $\psi$ -function, and finally the counter intuitive entanglement, all arise from the simple way in which the calculational apparatus of Q.M. allows two separate systems to be mentally combined into one, and for which it seems to be predestined. We have seen that, when two systems start to interact, their  $\psi$ -functions do not start to interact, but instead they cease to exist and a new  $\psi$ -function for the total system takes their place. Briefly, it consists initially of the product of the two individual functions. Since these functions depend on entirely different variables, the product function depends on all these variables; it exists in a space of much higher dimension than the individual functions. As soon as the systems start to interact, the total function will no longer be a product, and also after the systems have separated, does not decompose into factors corresponding to the individual systems. As a result, one temporarily (until the entanglement is lifted by an actual measurement) only possesses a composite description of both in this region of high dimension. That is the reason why the knowledge of the individual systems can be reduced to the essential, yes even zero, while that of the total system remains maximal. Best possible knowledge of the total does not imply best possible knowledge of its parts - and that is at the root of the whole problem.

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<sup>4</sup> Berliner Ber., 16 April 1931; Ann. Inst. Poincare, p. 269 (Paris 1931); Cursos de la universidad internacional de verano en Santander, 1, p. 60 (Madrid, Signo, 1935).

When thinking about it, the following matter does make one wonder. The imagined act of combining two or more systems into a single one leads to great problems as soon as one tries to introduce the special principle of relativity into Q.M. The problem of a single relativistic was solved by P. A. M. Dirac<sup>5</sup> already seven years ago, in a remarkably simple and elegant way. A number of experimental confirmations, indicated by the headwords electron spin, positive electron and pair creation, leave no doubt about the essential correctness of this solution. But, first of all, it goes very much beyond the current scheme<sup>6</sup> of Q.M. (the one that I have tried to describe here), and secondly, it runs into strong resistance as soon as one tries to fathom the problem of more than one electron, using the nonrelativistic theory as an example. (That already shows that the solution lies outside the general scheme, for in this scheme the combination of subsystems is very simple, as we have seen.) I do not claim to have an opinion about the attempts made in this direction<sup>7</sup>. However, I do not think they have attained their goal, especially since the authors themselves do not claim this.

The situation is similar with another system, namely the electromagnetic field. Its laws are "the embodiment of relativity theory", a *nonrelativistic* treatment is not even possible. All the same, it was this field which was the first to be quantized. As classical model of thermal radiation, it gave the first impetus to the development of the quantum theory. That this could be done by simple means is due to the fact that it is slightly easier here because the photons, the "atoms of light", do not interact directly with one another at all, but only through the medium of charged particles<sup>8</sup>. Nevertheless, we still don't have a truly consistent quantum theory of the electromagnetic field<sup>9</sup>. It is true that one can get a long way by building it up from subsystems in the mold of nonrelativistic theory (Dirac's theory of light<sup>10</sup>), but it is not complete. Perhaps, the simple procedure of the nonrelativistic theory is only a convenient calculational trick, but it has at the moment attained a tremendous influence over our basic view of Nature.

With warm gratitude I acknowledge Imperial Chemical Industries Limited, London, for giving me the courage to complete this paper.

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<sup>5</sup> Proc. Roy. Soc. London, A **117**, 610 (1928).

<sup>6</sup> P. A. M. Dirac, The principles of quantum mechanics, 1st Ed., p. 239; 2nd Ed., p. 252. Oxford: Clarendon Press 1930, resp. 1935.

<sup>7</sup> Here are some important references to the literature: G. Breit, Phys. Rev. 34, 553 (1929) and 616 (1932). - C. Møller, Z. Physik 70, 786 (1931). - P. A. M. Dirac, Proc. Royal Soc. London, A 136, 453 (1932) and Proc. Cambridge Philos. Soc. 30, 150 (1934). - R. Peierls, Proc. Royal Soc. London, A 146, 420 (1934). - W. Heisenberg, Z. Physik 90, 209 (1934).

<sup>8</sup> This is probably only approximately true. Cf. M. Born and L. Infeld, Proc. Royal Soc. London, A 144, 425 and 147, 522 (1934); 150, 141 (1935). This is the most recent attempt at a quantum electrodynamics.

<sup>9</sup> Here again some of the most important works, they belong partly also to the previous citation: P. Jordan and W. Pauli, Z. Physik 47, 151 (1928). - W. Heisenberg and W. Pauli, Z. Physik 56, 1 (1929); 59, 168 (1930). - P. A. M. Dirac, V. A. Fock and B. Podolsky, Phys. J. Sov. Union 6, 468 (1932). - N. Bohr and L. Rosenfeld, Danske Vidensk. Selskab, math.-phys. Med. 12, 8 (1933).

<sup>10</sup> A suitable reference: E. Fermi, Rev. Mod. Phys. 4, 87 (1932).