# THE PRESENT SITUATION IN QUANTUM MECHANICS:

# A TRANSLATION OF SCHRÖDINGER'S "CAT PARADOX PAPER"

Erwin Schrödinger
Translator: John D. Trimmer[*]

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**Contents**

- Introductory Note
- 1. The Physics of Models
- 2. Statistics of Model Variables in Quantum Mechanics
- 3. Examples of Probability Predictions
- 4. Can One Base the Theory on Ideal Ensembles?
- 5. Are the Variables Really Blurred?
- 6. The Deliberate About-face of the Epistemological Viewpoint
- 7. The Psi-function as Expectation-catalog
- 8. Theory of Measurement, Part One
- 9. The Psi-function as Description of State
- 10. Theory of Measurement, Part Two
- 11. Resolution of the "Entanglement" Result Dependent on the Experimenter's Intention
- 12. An example
- 13. Continuation of the Example: All Possible Measurements are Entangled Unequivocally
- 14. Time-dependence of the Entanglement. Consideration of the Special Role of Time
- 15. Natural Law or Calculating Device?
- Notes

**Introductory Note**

This is a translation of Schrödinger's three-part 1935 paper[1] in *Die Naturwissenschaften*. Earlier that same year there had appeared the Einstein, Podolsky, Rosen paper[2] (also famous in "paradoxology") which, Schrödinger says, in a footnote, motivated his offering. Along with this article in German, Schrödinger had two closely-related English-language publications.[3] But the German, aside from its one-paragraph presentation of the famous cat, covers additional territory and gives many fascinating insights into Schrödinger's thought. The translator's goal has been to adhere to the logical and physical content of the original, while at the same time trying to convey something of its semi-conversational, at times slightly sardonic flavor.

TRANSLATION

**1. The Physics of Models**

In the second half of the previous century there arose, from the great progress in kinetic theory of gases and in the mechanical theory of heat, an ideal of the exact description of nature that stands out as the reward of centuries-long search and the fulfillment of millennia-long hope, and that is called classical. These are its features.

Of natural objects, whose observed behavior one might treat, one set sup a representation - based on the experimental data in one's possession but without handcuffing the intuitive imagination - that is worked out in all details exactly, *much* more exactly than any experience, considering its limited extent, can ever authenticate. The representation in its absolute determinacy resembles a mathematical concept or a geometric figure which can be completely calculated from a number of *determining parts*; as, e.g., a triangle's one side and two adjoining angles, as determining parts, also determine the third angle, the other two sides, the three altitudes, the radius of the inscribed circle, etc. Yet the representation differs intrinsically from a geometric figure in this important respect, that also in *time* as fourth dimension it is just as sharply determined as the figure is in the three space dimensions. Thus it is a question (as is self-evident) always of a concept that changes with time, that can assume different *states*; and if a state becomes known in the necessary number of determining parts, then not only are all other parts also given for this moment (as illustrated for the triangle above), but likewise all parts, the complete state, for any given later time; just as the character of a triangle on its base determines its character at the apex. It is part of the inner law of the concept that it should change in a given manner, that is, if left to itself in a given initial state, that it should continuously run through a given sequence of states, each one of which it reaches at a fully determined time. That is its nature, that is the hypothesis, which, as I said above, one builds on a foundation of intuitive imagination.

Of course one must not think so literally, that in this way one learns how things go in the real world. To show that one does not think this, one calls the precise thinking aid that one has created, an *image* or a *model*. With its hindsight-free clarity, which cannot be attained without arbitrariness, one has merely insured that a fully determined hypothesis can be tested for its consequences, without admitting further arbitrariness during the tedious calculations required for deriving those consequences. Here one has explicit marching orders and actually works out only what a clever fellow could have told directly from the data! At least one then knows where the arbitrariness lies amd where improvement must be made in case of disagreement with experience: in the initial hypothesis or model. For this one must always be prepared. If in many various experiments the natural object behaves like the model, one is happy and thinks that the image fits the reality in essential features. If it fails to agree, under novel experiments or with refined measuring techniques, it is not said that one should *not* be happy. For basically this is the means of gradually bringing our picture, i.e., our thinking, closer to the realities.

The classical method of the precise model has as principal goal keeping the unavoidable arbitrariness neatly isolated in the assumptions, more or less as body cells isolate the nucleoplasm, for the historical process of adaptation to continuing experience. Perhaps the method is based on the belief that *somehow* the initial state *really* determines uniquely the subsequent events, or that a *complete* model, agreeing with reality in *complete exactness* would permit predictive calculation of outcomes of all experiments with complete exactness. Perhaps on the other hand this belief is based on the method. But it is quite probable that the adaptation of thought to experience is an infinite process and that "complete model" is a contradiction in terms, somewhat like "largest integer".

A clear presentation of what is meant by *classical model*, its *determining parts*, its *state*, is the foundation for all that follows. Above all, a *determinate model* and a *determinate state of the same* must not be confused. Best consider an example. The Rutherford model of the hydrogen atom consists of two point masses. As determining parts one could for example use the two times three rectangular coordinates of the two points and the two times three components of their velocities along the coordinate axes - thus twelve in all. Instead of these one could also choose: the coordinates and velocity components of the *center of mass*, plus the *separation* of the two points, *two angles* that establish the direction in space of the line joining them, and the speeds (= time derivatives) with which the separation and the two angles are changing at the particular moment; this again adds up of course to twelve. It is *not* part of the concept "R-model of the H-atom" that the determining parts should have particular numerical values. Such being assigned to them, one arrives at a *determinate state* of the model. The clear view over the totality of possible states - yet without relationship among them - constitutes "the model" or "the model in *any* state *whatsoever*". But the concept of the model then amounts to more than merely: the two points in certain positions, endowed with certain velocities. It embodies also knowledge for *every* state how it will change with time in absence of outside interference. (Information on how one half of the determining parts will change with time is indeed given by the other half, but how this other half will change must be independently determined.) *This* knowledge is implicit in the assumptions: the points have the masses m, M and the charges -e, +e and therefore attract each other with force e^2/r^2, if their separation is r.

These results, with definite numerical values for m, M, and e (but of course *not* for r), belong to the description *of the model* (not first and only to that of a definite state). m, M, and e are *not* determining parts. By contrast, separation r is one. It appears as the seventh in the second "set" of the example introduced above. And if one uses the first, r is not an independent thirteenth but can be calculated from the six rectangular coordinates:

r = | (x1 - x2)^2 + (y1 - y2)^2 + (z1 - z2)^2 | ^ (1/2) .

The number of determining parts (which are often called *variables* in contrast to *constants of the model* such as m, M, e) is unlimited. Twelve conveniently chosen ones determine all others, or the *state*. No twelve have the privilege of being *the* determining parts. examples of other especially important determining parts are: the energy, the three components of angular momentum relative to center of mass, the kinetic energy of center of mass motion. These just named have, however, a special character. They are indeed *variable*, i.e., they have different values in different states. But in every *sequence* of states, that is actually passed through in the course of time, they retain the same value. So they are also called *constants of the motio*n - differing from constants of the model.

**2. Statistics of Model Variables in Quantum Mechanics**

At the pivot point of contemporary quantum mechanics (Q.M.) stands a doctrine, that perhaps may yet undergo many shifts of meaning but that will not, I am convinced, cease to be the pivot point. It is this, that models with determining parts that uniquely determine each other, as do the classical ones, cannot do justice to nature.

One might think that for anyone believing this, the classical models have played out their roles. But this is not the case. Rather one uses precisely *them*, not only to express the negative of th new doctrine, but also to describe the diminished mutual determinacy remaining afterwards as though obtaining among the same variables of the same models as were used earlier, as follows:

*A*. The classical concept of *state* becomes lost, in that at most a well-chosen *half* of a complete set of variables can be assigned definite numerical values; in the Rutherford example for instance the six rectangular coordinates *or* the velocity components (still other groupings are possible). the other half then remains completely indeterminate, while supernumerary parts can show highly varying degrees of indeterminacy. In general, of a complete set (for the R-model twelve parts) *all* will be known only uncertainly. One can best keep track of the degree of uncertainty by following classical mechanics and choosing variables arranged in *pairs* of so-called canonically-conjugate ones. The simplest example is a space coordinate x of a point mass and the component p_x along the same direction, its linear momentum (i.e., mass times velocity). Two such constrain each other in the precision with which they may be simultaneously known, in that the product of their tolerance- or variation-widths (customarily designated by putting a Delta ahead of the quantity) cannot fall *below* the magnitude of a certain universal constant,[4] thus:

Delta-x . Delta-p_x >= hbar.

(Heisenberg uncertainty relation.)

*B*. If even at any given moment not all variables are determined by some of them, then of course neither are they all determined for a later moment by data obtainable either. This may be called a break with causality, but in view of *A*., it is nothing essentially new. If a classical state does not exist at any moment, it can hardly change causally. What do change are the *statistics* or *probabilities*, *these* moreover causally. Individual variables meanwhile may become more, or less, uncertain. Overall it may be said that the total precision of the description does not change with time, because the principle of limitations decsribed under *A*. remains the same at every moment.

Now what is the meaning of the terms "uncertain", "statistics", "probability"? Here Q.M. gives the following account. It takes over unquestioningly from the classical model the entire infinite roll call of imaginable variables or determining parts and proclaims each part to be *directly measurable*, indeed measuravle to arbitrary precision, so far as it alone is concerned. If through a well-chosen, constrained set of measurements one has gained that maximal knowledge of an object which is just possible according to *A*., then the mathematical apparatus of the new theory provides means of assigning, for the same or for any later instant of time, a fully determined *statistical distribution* to *every* variable, that is, an indication of the fraction of cases it will be found at this or that value, or within this or that small interval (which is also called probability.) The doctrine is that this is in fact the probability of encountering the relevant variable, if one measures it at the relevant time, at this or that value. By a single trial the correctness of this *probability prediction* can be given at most an approximate test, namely in the case that it is comparatively sharp, i.e., declares possible only a small range of values. To test it thoroughly one must repeat the entire trial *ab ovo* (i.e., including the orientational or preparatory measurements) *very* often and may use only those cases in which the *preparatory* measurements gave exactly the same results. For these cases, then, the statistics of a particular variable, reckoned forward from the preparatory measurements, is to be confirmed by measurement - this is the doctrine.

One must guard against criticizing this doctrine because it is so difficult to express; this is a matter of language. But a different criticism surfaces. Scarcely a single physicist of the classical era would have dared to believe, in thinking about a model, that its determining parts are measurable on the natural object. Only much remoter consequences of the picture were actually open to experimental test. And all experience pointed towards one conclusion: long before the advancing experimental arts had bridged the broad chasm, the model would have substantially changed through adaptation to new facts. --Now while the new theory calls the classical model incapable of specifying all details of the *mutual interrelationship of the determining parts* (for which its creators intended it), it nevertheless considers the model suitable for guiding us as to just which measurements can in principle be made on the relevant natural object. This would have seemed to those who thought up the picture a scandalous extension of their thought-pattern and an unscrupulous proscription against future development. Would it not be pre-established harmony of a peculiar sort if the classical-epoch researchers, those who, as we hear today, had no idea of what *measuring* truly is, had unwittingly gone on to give us as legacy a guidance scheme revealing just what is fundamentally measurable for instance about a hydrogen atom!?

I hope later to make clear that the reigning doctrine is born of distress. Meanwhile I continue to expound it.

**3. Examples of Probability Predictions**

All of the foregoing pertains to determining parts of a classical model, to positions and velocities of point masses, to energies, angular momenta, etc. The only unclassical feature is that only probabilities are predicted. Let us have a closer look. The orthodox treatment is always that, by way of certain measurements performed *now* and by way of their resulting prediction of results to be expected of other measurements following thereafter either immediately or at some given time, one gains the best possible probability estimates permitted by nature. Now how does the matter really stand? In important and typical cases as follows.

If one measures the energy of a Planck oscillator, the probability of finding for it a value between E and E' cannot possibly be other than zero unles between E and E' there lies at least one value from the series 3.pi.hbar.nu, 5.pi.hbar.nu, 7.pi.hbar.nu, 9.pi.hbar.nu,... For any interval containing none of these values the probability is zero. In plain English: other measurement results are excluded. The values are odd multiples of the *constant of the model* pi.hbar.nu

(Planck constant) / 2.pi, nu = frequency of the oscillator.

Two points stand out. First, no account is taken of preceding measurements - these are quite unnecessary. Second, the statement certainly doesn't suffer an excessive lack of precision - quite to the contrary it is sharper than any actual measurement could ever be.

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Figure 1.             /|\
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               O.......F
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Angular momentum. M is a material point, O a geometric reference point. The vector arrow represents the momentum (= mass times velocity) of M. Then the *angular momentum* is the product of the length of the arrow by the length OF.

Another typical example is magnitude of angular momentum. In Fig. 1 let M be a moving point mass, with the vector representing, in magnitude and direction, its momentum (mass times velocity). O is any arbitrary fixed point in space, say the origin of coordinates; thus not a physically significant point, but rather a geometric reference point. As magnitude of the angular momentum of M about O classical mechanics designates the product of the length of the momentum vector by the length of the *normal OF*. In Q.M. the magnitude of angular momentum is governed much as the energy of the oscillator. Again the probability is zero for any interval not containing some value(s) from the following series

hbar(2)^(1/2), hbar(2x3)^(1/2), hbar(3x4)^(1/2), hbar(4x5)^(1/2),...;

that is, only one of these values is allowed. Again this is true without reference to preceding measurements. And one readily conceives how important is this precise statement, *much* more important than knowing which of these values, or what probability for each of them, would actually pertain to a given case. Moreover it is also noteworthy here that there is no mention of the reference point: however it is chosen one will get a value from the series. This assertion seems unreasonable for the model, because the normal OF changes *continuously* as the point O is displaced, if the momentum vector remains unchanged. In this example we see how Q.M. does indeed use the model to read of those quantities which one can measure and for which it makes sense to predict results, but finds the classical model inadequate for explicating relationships among these quantities. Now in both examples does one not get the feeling that the essential content of what is being said can only with some difficulty be forced into the Spanish boot of a prediction of probability of finding this or that measurement result for a variable of the classical model? Does one not get the impression that here one deals with fundamental properties of *new* classes of characteristics, that keep only the name in common with classical ones? And by no means do we speak here of exceptional cases, rather it is precisely the truly valuable statements of the new theory that have this character. There are indeed problems more nearly of the type for which the mode of expression is suitable. But they are by no means equally important. Moreover of no importance whatever are those that are naively set up as class exercises. "Given the position of the elctron in the hydrogen atom at time t=0, find the statistics of its position at a later time." No one cares about that.

The big idea seems to be that all statements pertain to the intuitive model. But the useful statements are scarcely intuitive in it, and its intuitive aspects are of little worth.

**4. Can One Base the Theory on Ideal Ensembles?**

The classical model plays a Protean role in Q.M. Each of its determining parts can under certain circumstances become an object of interest and achieve a certain reality. But never all of them together - now it is these, now those, and indeed always at most *half* of the complete set of variables allowed by a full picture of the momentary state. Meantime, how about the others? *Have* they then no reality, perhaps (pardon the expression) a blurred reality; or are all of them always real and is it merely, according to Theorem *A.* of Sect. 2, that simultaneous *knowledge* of them is ruled out?

The second interpretation is especially appealing to those acquainted with the *statistical viewpoint* that came up in the second half of the preceding century; the more so, considering that on the eve of the new century quantum theory was born *from it*, from a central problem in the statistical theory of heat (Max Planck's *Theory of Heat Radiation*, December, 1899). The essence of this line of thought is precisely this, that one practically never knows all the determining parts of the system, but rather *much* fewer. To describe an actual body at a given moment one relies therefore not on *one* state of the model but on a so-called *Gibbs ensemble*. By this is meant an ideal, that is, merely imagined ensemble of states, that accurately reflects our limited knowledge of the actual body. The body is then considered to behave as though in a single state *arbitrarily chosen from this ensemble*. This interpretation had the most extensive results. Its highest triumphs were in those cases for which *not* all states appearing in the ensemble led to *the same* observable behavior. Thus the body's conduct is now this way, now that, just as foreseen (thermodynamics fluctuations). At first thought one might well attempt likewise to refer back the always uncertain statements of Q.M. to an ideal ensemble of states, of which a quite specific one applies in any concrete instance - but one does not know which one.

That this won't work is shown by the *one* example of angular momentum, as one of many. Imagine in Fig. 1 the point M to be situated at various positions relative to O and fitted with various momentum vectors, and all these possibilities to be combined into an ideal ensemble. Then one can indeed so choose these positions and vectors that in every case the product of vector length by length of normal OF yields one or the other of the acceptable values - relative to the particular point O. But for an arbitrarily different point O', of course, unacceptable values occur. Thus appeal to the ensemble is no help at all. --Another example is the oscillator energy. Take the case that it has a sharply determined value, e.g., the lowest, 3.pi.hbar.nu. The separation of the two point masses (that constitute the oscillator) then appears very *unsharp*. To be able to refer this statement to a statistical collective of states would require the distribution of separations to be sharply limited, at least toward large values, by that separation for which the *potential energy* alone would equal or exceed the value 3.pi.hbar.nu. But that's not the way it is - arbitrarily large separations occur, even though with markedly reduced probability. And this is no mere secondary calculation result, that might in some fashion be circumvented, without striking at the heart of the theory: along with many others, the quantum mechanical treatment of radioactivity (Gamow) rests on this state of affairs. --One could go on indefinitely with more examples. One shoudl note that there was no question of any time-dependent changes. It would be of no help to permit the model to vary quite "unclassically", perhaps to "jump". Already for the single instant things go wrong. At no moment does there exist an ensemble of classical states of the model that squares with the totality of quantum mechanical statements of this moment. The same can also be said as follows: if I wish to ascribe to the model at each moment a definite (merely not exactly known to me) state, or (which is the same) to *all* determining parts definite (merely not eactly known to me) numerical values, then there is no supposition as to these numerical values *to be imagined* that would not conflict with some portion of quantum theoretical assertions.

That is not quite what one expects, on hearing that the pronouncements of the new theory are always uncertain compared to the classical ones.

**5. Are the Variables Really Blurred?**

The other alternative consists of granting reality only to the momentarily sharp determining parts - or in more general terms to each variable a sort of realization just corresponding to the quantum mechanical statistics of this variable at the relevant moment.

That it is in fact not impossible to express the degree and kind of blurring of *all* variables in *one* perfectly *clear* concept follows at once from the fact that Q.M. as a matter of fact has and uses such an instrument, the so-called wave function or psi-function, also called system vector. Much more is to be said about it further on. That it is an abstract, unintuitive mathematical construct is a scruple that almost always surfaces against new aids to thought and that carries no great message. At all events it is an imagined entity that images the blurring of all variables at every moment just as clearly and faithfully as does the classical model its sharp numerical values. Its equation of motion too, the law of its time variation, so long as the system is left undisturbed, lags not one iota, in clarity and determinacy, behind the equations of motion of the classical model. So the latter could be straight-forwardly replaced by the psi-function, so long as the blurring is confined to atomic scale, not open to direct control. In fact the function has provided quite intuitive and convenient ideas, for instance the "cloud of negative electricity" around the nucleus, etc. But serious misgivings arise if one notices that the uncertainty affects macroscopically tangible and visible things, for which the term "blurring" seems simply wrong. The state of a radioactive nucleus is presumably blurred in such a degree and fashion that neither the instant of decay nor the direction, in which the emitted alpha-particle leaves the nucleus, is well-established. Inside the nucleus, blurring doesn't bother us. The emerging particle is described, if one wants to expain intuitively, as a spherical wave that continuously emanates in all directions and that impinges continuously on a surrounding luminescent screen over its full expanse. The screen however does not show a more or less constant uniform glow, but rather lights up at *one* instant at *one* spot - or, to honor the truth, it lights up now here, now there, for it is impossible to do the experiment with only a single radioactive atom. If in place of the luminescent screen one uses a spatially extended detector, perhaps a gas that is ionised by the alpha-particles, one finds the ion pairs arranged along rectilinear columns,[5] that project backwards on to the bit of radioactive matter from which the alpha-radiation comes (C.T.R. Wilson's cloud chamber tracks, made visible by drops of moisture condensed on the ions).

One can even set up quite ridiculous cases. A cat is penned up in a steel chamber, along with the following device (which must be secured against direct interference by the cat): in a Geiger counter there is a tiny bit of radioactive substance, *so* small, that *perhaps* in the course of the hour one of the atoms decays, but also, with equal probability, perhaps none; if it happens, the counter tube discharges and through a relay releases a hammer which shatters a small flask of hydrocyanic acid. If one has left this entire system to itself for an hour, one would say that the cat still lives *if* meanwhile no atom has decayed. The psi-function of the entire system would express this by having in it the living and dead cat (pardon the expression) mixed or smeared out in equal parts.

It is typical of these cases that an indeterminacy originally restricted to the atomic domain becomes transformed into macroscopic indeterminacy, which can then be *resolved* by direct observation. That prevents us from so naively accepting as valid a "blurred model" for representing reality. In itself it would not embody anything unclear or contradictory. There is a difference between a shaky or out-of-focus photograph and a snapshot of clouds and fog banks.

**6. The Deliberate About-face of the Epistemological Viewpoint**

In the fourth section we saw that it is not possible smoothly to take over models and to ascribe, to the momentarily unknown or not exactly known variables, nonetheless determinate values, that we simply don't know. In Sect. 5 we saw that the indeterminacy is not even an actual blurring, for there are always cases where an easily executed observation provides the missing knowledge. So what is left? From this very hard dilemma the reigning doctrine rescues itself by having recourse to epistemology. We are told that no distinction is to be made between the state of a natural object and what I know about it, or perhaps better, what I can know about it if I go to some trouble. Actually - so they say - there is intrinsically only awareness, observation, measurement. If through them I have procured at a given moment the best knowledge of the state of the physical object that is possibly attainable in accord with natural laws, then I can turn aside as *meaningless* any further questioning about the "actual state", inasmuch as I am convinced that no further observation can extend my knowledge of it - at least, not without an equivalent diminution in some other respect (namely by changing the state, see below).

Now this sheds some light on the origin of the proposition that I mentioned at the end of Sect. 2 as something very far-reaching: that all model quantities are measurable in principle. One can hardly get along without this article of belief if one sees himself constrained, in the intersts of physical methodology, to call in as dictatorial help the above-mentioned philosophical principle, which no sensible person can fail to esteem as the supreme protector of all empiricism.

Reality resists imitation through a model. So one lets go of niave realism and leans directly on the indubitable proposition that *actually* (for the physicist) after all is said and done there is only observation, measurement. Then all our physical thinking thenceforth has as sole basis and as sole object the results of measurements which can in principle be carried out, for we must now explicitly *not* relate our thinking any longer to any other kind of reality or to a model. All numbers arising in our physical calculations must be interpreted as measurement results. But since we didn't just now come into the world and start to build up our science from scratch, but rather have in use a quite definite shceme of calculation, from which in view of the great progress in Q.M. we would less than ever want to be parted, we see ourselves forced to dictate from the writing-table which measurements are in principle possible, that is, must be possible in order to support adequately our reckoning system. This allows a sharp value for each single variable of the model (indeed for a whole "half set") and so each single variable must be measurable to arbitrary exactness. We cannot be satisfied with less, for we have lost our naively realistic innocence. We have nothing but our reckoning scheme, i.e., what is a *best possible* knowledge of the object. And if we couldn't do that, then indeed would our measurement reality become highly dependent on the diligence or laziness of the experimenter, how much trouble he takes to inform himself. We must go on to tell him how far he could go if only he were clever enough. Otherwise it would be seriously feared that just there, where we forbid further questions, there might well still be something worth knowing that we might ask about.

**7. The Psi-function as Expectation-catalog**

Continuing to expound the official teaching, let us turn to the already (Sect. 5) mentioned psi-function. It is now the means for predicting probability of measurement results. In it is embodied the momentarily-attained sum of theoretically based future expectation, somewhat as laid down in a *catalog*. It is the relation- and determinacy-bridge between measurements and measurements, as in the classical theory the model and its state were. With this latter the psi-function moreover has much in common. It is, in principle, determined by a finite number of suitably chosen measurements on the object, half as many as were required in the classical theory. Thus the catalog of expectations is initially compiled. From then on it changes with time, just as the state of the model of classical theory, in constrained and unique fashion ("causally") - the evolution of the psi-function is governed by a partial differential equation (of first order in time and solved for delta(psi)/delta(t)). This corresponds to the undisturbed motion of the model in classical theory. But this goes on only until one again carries out any measurement. For each measurement one is required to ascribe to the psi-function (= the prediction-catalog) a characteristic, quite sudden change, which *depends on the measurement result obtained*, and so *cannot be foreseen*; from which alone it is already quite clear that this second kind of change of the psi-function has nothing whatever in common with its orderly development *between* two measurements. The abrupt change by measurement ties in closely with matters discussedin Sect. 5 and will occupy us further at some length; it is the most interesting point of the entire theory. It is precisely *the* point that demands the break with naive realism. For *this* reason one can *not* put the psi-function directly in place of the model or of the physical thing. And indeed because one might never dare impute abrupt unforeseen changes to a physical thing or to a model, but because in the realism point of view observation is a natural process like any other and cannot *per se* bring about an interruption of the orderly flow of natural events.

**8. Theory of Measurement, Part One**

The rejection of realism has logical consequences. In general, a variable *has* no definite value before I measure it; then measuring it does *not* mean ascertaining the value that it *has*. But then what does it mean? There must still be some criterion as to whether a measurement is true or false, a method is good or bad, accurate, or inaccurate - whether it deserves the name of measurement process at all. Any old playing around with an indicating instrument in the vicinity of another body, whereby at any old time one then takes a reading, can hardly be called a measurement on this body. Now it is fairly clear; if reality does not determine the measured value, then at least the measured value must determine reality - it must actually be present *after* the measurement in *that* sense which alone will be recognised again. That is, the desired criterion can be merely this: repetition of the measurement must give the same result. By many repetitions I can prove the accuracy of the procedure and show that I am not just playing. It is agreeable that this program matches exactly the method of the experimenter, to whom likewise the "true value" is not known beforehand. We formulate the essential point as follows:

*The systematically arranged interaction of two systems (measured object and measuring instrument) is called a measurement on the first system, if a directly-sensible variable feature of the second (pointer position) is always reproduced within certain error limits when the process is immediately repeated (on the same object, which in the meantime must not be exposed to any additional influences)*.

This statement will require considerable added comment: it is by no means a faultless definition. Empirics is more complicated than mathematics and is not so easily captured in polished sentences.

*Before* the first measurement there might have been an arbitrary quantum-theory prediction for it. *After* it the prediction *always* runs: within error limits again the same result. The expectation-catalog (= psi-function) is therefore changed by the measurement in respect to the variable being measured. If the measurement procedure is known from beforehand to be *reliable*, then the first measurement at once reduces the theoretical expectation within error limits on to the value found, regardless of whatever the prior expectation may have been. This is the typical abrupt change of the psi-function discussed above. But the expectation-catalog changes in unforeseen manner not only for the measured variable itself, but also for others, in particular for its "canonical conjugate". If for instance one has a rather sharp prediction for the *momentum* of a particle and proceeds to measure its *position* more exactly than is compatible with Theorem A of Sec. 2, then the *momentum* prediction must change. The quantum mechanical reckoning moreover takes care of this automatically; there is no psi-function whatsoever that would contradict Theorem A when one deduces from it the combined expectations.

Since the expectation-catalog changes radically during measurement, the object is then no longer suited for testing, in their full extent, the statistical predictions made earlier; at the very least for the measured variable itself, since for it now the (nearly) same value would occur over and over again. *That* is the reason for the prescription already given in Sect. 2: one can indeed test the probability predictions completely, but for this one must repeat the entire experiment *ab ovo*. One's prior treatment of the measured object (or one identical to it) must be exactly the same as that given the first time, in order that the same expectation-catalog (= psi-function) should be valid as before the first measurement. Then one "repeats" it. (This repeating now means of course something quite other than earlier!) All this one must do not twice but very often. Then the predicted statistics are established - that is the doctrine.

One should note the difference between the error limits and the error distribution of the *measurement*, on the one hand, and the theoretically predicted statistics, on the other hand. They have nothing to do with each other. They are established by the two quite different types of *repetition* just discussed.

Here there is opportunity to deepen somewhat the above-attempted delimitation of *measuring*. There are measuring instruments that remain fixed on the reading given by the measurement just made. Or the pointer could remain stuck because of a defect. One would then repeatedly make exactly the same reading, and according to our instruction that would be a spectacularly accurate measurement. Moreover that would be true not merely for the object but also for the instrment itself! As a matter of fact there is still missing from our exposition an important point, but one which could not readily be stated earlier, namely what it is that truly makes the difference between *object* and *instrument* (tat it is the latter on which the reading is made, is more or less superficial). We have just seen that the instrument under certain circumstances, as required, must be set back to its neutral initial condition before any control measurement is made. This is well known to the experimentalist. Theoretically the matter may best be expressed by prescribing that on principle the instrument should be subjected to the identical prior treatment before each measurement, so that *for it* each time the same expectation-catalog (= psi-function) applies, as it is brought up to the object. For the object it is just the other way around, any interference being forbidden when a control measurement is to be made, a "repetition of the first kind" (that leads to *error* statistics). That is the characteristic difference between objectand instrument. It disappears for a "repetition of the second kind" (that serves for checking the quantum predictions). Here the difference between the two is actually rather insignificant.

>From this we gather further that for a second measurement one may use a similarly built and similarly prepared instrument - it need not necessarily be *the same one*; this is in fact sometimes done, as a check on the first one. it may indeed happen that two qute differently built instruments are so related to each other that if one measures with them one after the other (repetition of the first kind!) their two indications are in one-to-one correlation with each other. They then measure on the object esssentially the same variable - i.e., the same for suitable calibration of the scales.

**9. The Psi-function as Description of State**

The rejection of realism also imposes obligations. From the standpoint of the classical model the momentary statement content of the psi-function is far from complete; it comprises only about 50 per cent of a complete description. >From the new standpoint it must be complete for reasons already touched upon at the end of Sect. 6. It must be impossible to add on to it additional correct statements, without otherwise changing it; else one would not have the right to call meaningless all statements extending beyond it.

Thence it follows that two different catalogs, that apply to the same system under different circumstances or at different times, may well partially overlap, but never so that the one is entirely contained within the other. For otherwise it would be susceptible to completion through additional correct statements, namely through those by which the other one exceeds it. --The mathematical structure of the theory automatically satisfies this condition. There is no psi-function that furnishes exactly the same statements as another and in addition several more.

Therefore if a system changes, whether by itself or because of measurements, there must always be statements missing from the new function that were contained in the earlier one. In the catalog not just new entries, but also deletions, must be made. Now knowledge can well be *gained*, but not *lost*. So the deletions mean that the previously correct statements have now become incorrect. A correct statement can become incorrect only if the *object* to which it applies changes. I consider it acceptable to express this reasoning sequence as follows:

Theorem 1: If different psi-functions are under discussion the system is in different states.

If one speaks only of systems for which a psi-function is in general available, then the inverse of this theorem runs:

Theorem 2: For the same psi-function the system is in the same state.

The inverse does not follow from Theorem 1 but independently of it, directly from *completeness* or *maximality*. Whoever for the same expectation-catalog would yet claim a difference is possible, would be admitting that it (the catalog) does not give information on all justifiable questions. --The language usage of almost all authors implies the validity of the above two theorems. Of course, they set up a kind of new reality - in entirely legitimate fashion, I believe. Moreover they are not trivially tautological, not mere verbal interpretations of "state". Without presupposed maximality of the expectation-catalog, change of the psi-function could be brought about by mere collecting of new information.

We must face up to yet another objection to the derivation of Theorem 1. One can argue that each individual statement or item of knowledge, under examination there, is after all a probability statement, to which the category of *correct*, or *incorrect* does not apply in any relation to an individual case, but rather in relation to a collective that comes into being from one's preparing the system a thousand times in identical fashion (in order then to allow the same measurement to follow; cf. Sect. 8.). That makes sense, but we must specify all members of this collective to be identically prepared, since to each the same psi-function, the same statement-catalog applies and we dare not specify differences that are not specified in the catalog (cf. the foundation of Theorem 2). Thus the collective is made up of identical individual cases. If a statement is wrong for *it*, then the individual case must have changed, or else the collective too would again be the same.

**10. Theory of Measurement, Part Two**

Now it was previously stated (Sect. 7) and explained (Sect. 8) that any *measurement* suspends the law that otherwise governs continuous time-dependence of the psi-function and brings about in it a quite different change, not governed by any law but rather dictated by the result of the measurement. But laws of nature differing from the usual ones cannot apply during a measurement, for objectively viewed it is a natural process like any other, and it cannot interrupt the orderly course of natural events. Since it does interrupt that of the psi-function, the latter - as we said in Sect. 7 - can *not* serve, like the classical mode, as an experimentally verifiable representation of an objective reality. And yet in the last Section something like that has taken shape.

So, using catchwords for emphasis, I try again to contrast: 1.) The discontinuity of the expectation-catalog due to measurement is *unavoidable*, for if measurement is to retain any meaning at all then the *measured value*, from a good measurement, *must* obtain. 2.) The discontinuous change is certainly *not* governed by the otherwise valid causal law, since it depends on the measured value, which is not predetermined. 3.) The change also definitely includes (because of "maximality") some *loss* of knowledge, but knowledge cannot be lost, and so the object *must* change - *both* along with the discontinuous changes and *also*, during these changes, in an unforeseen, *different* way.

How does this add up? Things are not at all simple. It is the most difficult and most interesting point of the theory. Obviously we must try to comprehend objectively the interaction between measured object and measuring instrument. To that end we must lay out a few very abstract considerations.

This is the point. Whenever one has a complete expectation-catalog - a maximum total knowledge - a psi-function - for two completely separated bodies, or, in better terms, for each of them singly, then one obviously has it also for the two bodies together, i.e., if one imagines that neither of them singly but rather the two of them together make up the object of interest, of our questions about the future.[6]

But the converse is not true. *Maximal knowledge of a total system does not necessarily include total knowledge of all its parts, not even when these are fully separated from each other and at the moment are not influencing each other at all.* Thus it may be that some part of what one knows may pertain to relations or stipulations between the two subsystems (we shall limit ourselves to two), as follows: if a particular measurement on the first system yields *this* result, then for a particular measurement on the second the valid expectation statistics are such and such; but if the measurement in question on the first system should have *that* result, then some other expectation holds for that on the second; should a third result occur for the first, then still another expectation applies to the second; and so on, in the manner of a complete disjunction of all possible measurement results which the one specifically contemplated measurement on the first system can yield. In this way, any measurement process at all or, what amounts to the same, any variable at all of the second system can be tied to the not-yet-known value of any variable of the first, and of course *vice versa* also. If that is the case, if such conditional statements occur in the combined catalog, *then it can not possibly be maximal in regard to the individual systems*. For the content of two maximal individual catalogs would by itself suffice for a maximal combined catalog; the conditional statements could not be added on.

These conditional predictions, moreover, are not something that has suddenly fallen in here from the blue. They are in every expectation-catalog. If one knows the psi-function and makes a particular measurement and this has a particular result, then one again knows the psi-function, *voila tout*. It's jst that for the case under discussion, because the combined system is supposed to consist of two fully separated parts, the matter stands out as a bit strange. For thus it becomes meaningful to distinguish between measurements on the one and measurements on the other subsystem. This provides to each full title to a private maximal catalog; on the other hand it remains possible that a portion of the attainable combined knowledge is, so to say, squandered on conditional statements, that operate between the subsystems, so that the private expectancies are left unfulfilled - even though the combined catalog is maximal, that is even though the psi-function of the combined system is known.

Let us pause for a moment. This result in its abstractness actually says it all: Best possible knowledge of a whole does not necessarily include the same for its parts. let us translate this into terms of Sect. 9: The whole is in a definite state, the parts taken individually are not.

"How so? Surely a system must be in some sort of state." "No. State is psi-function, is maximal sum of knowledge. I didn't necessarily provide myself with this, I may have been lazy. Then the system is in no state."

"Fine, but then too the agnostic prohibition of questions is not yet in force and in our case I can tell myself: the subsystem is already in some state, I just don't know which."

"Wait. Unfortunately no. There is no `I just don't know.' For as to the total system, maximal knowledge is at hand..."

*The insufficiency of the psi-function as model replacement rests solely on the fact that one doesn't always have it.* If one does have it, then by all means let it serve as description of the state. But sometimes one does not have it, in cases where one might reasonably expect to. And in that case, one dare not postulate that it "is actually a particular one, one just doesn't know it"; the above-chosen standpoint forbids this. "It" is namely a sum of knowledge; and knowledge, that no one knows, is none. ----

We continue. That a portion of the knowledge should float in the form of disjunctive conditional statements *between* the two systems can certainly not happen if we bring up the two from opposite ends of the world and juxtapose them without interaction. For then indeed the two "know" nothing about each other. A measurement on one cannot possibly furnish any grasp of what is to be expected of the other. Any "entanglement of predictions" that takes place can obviously only go back to the fact that the two bodies at some earlier time formed in a true sense *one* system, that is were interacting, and have left behind *traces* on each other. If two separated bodies, each by itself known maximally, enter a situation in which they influence each other, and separate again, then there occurs regularly that which I have just called *entanglement* of our knowledge of the two bodies. the combined expectation-catalog consists initially of a logical sum of the individual catalogs; during the process it develops causally in accord with known law (there is no question whatever of measurement here). The knowledge remains maximal, but at its end, if the two bodies have again separated, it is not again split into a logical sum of knowledges about the individual bodies. What still remains *of that* may have becomes less than maximal, even very strongly so. --One notes the great difference over against the classical model theory, where of course from known initial states and with known interaction the individual end states would be exctaly known.

The measuring process described in Sect. 8 now fits neatly into this general scheme, if we apply it to the combined system, measured object + measuring instrument. As we thus construct an objective picture of this process, like that of any other, we dare hope to clear up, if not altogether avoid, the singular jump of the psi-function. So now the one body is the measured object, the other the instrument. To suppress any interference from outside we arrange for the instrument by means of built-in clockwork to creep up automatically to the object and in like manner creep away again. The reading itself we postpone, as our immediate purpose is to investigate whatever may be happening "objectively"; but for later use we let the result be recorded automatically in the instrument, as indeed is often done these days.

Now how do things stand, after automatically completed measurement? We possess, afterwards same as before, a maximal expectation-catalog for the total system. The recorded measurement result is of course not included therein. As to the instrument the catalog is far from complete, telling us nothing at all about where the recording pen left its trace. (Remember that poisoned cat!) What this amounts to is that our knowledge has evaporated into conditional statements: *if* the mark is at line 1, *then* things are thus and so for the measured object, *if* it is at line 2, then such and such, if at 3, then a third, etc. Now has the psi-function of the measured *object* made a leap? Has it developed further in accord with natural law (in accord with the partial differential equation)? No to both questions. It is no more. It has become snarled up, in accord with the causal law of the *combined* psi-function, with that of the measuring instrument. *The expectation-catalog of the object has split into a conditional disjunction if expectation-catalogs* - like a Baedeker that one has taken apart in the proper manner. Along with each section there is given also the probability that it proves correct - transcribed from the original expectation-catalog of the object. But which one proves right - which section of the Baedeker should guide the ongoing journey - that can be determined only by actual inspection of the record.

And what if we *don't* look? Let's say it was photographically recorded and by bad luck light reaches the film before it was developed. Or we inadvertently put in black paper instead of film. Then indeed have we not only not learned anything new from the miscarried measurement, but we have suffered loss of knowledge. This is not surprising. It is only natural that outside interference will almost always spoil the knowledge that one has of a system. The interference, if it is to allow the knowledge to be gained back afterwards, must be circumspect indeed.

What have we won by this analysis? *First*, the insight into the disjunctive splitting of the expectation-catalog, which still takes place quite continuously and is brought about through embedment in a combined catalog for instrument and object. From this amalgamation the object can again be separated out only by the living subject actually taking cognizance of the result of the measurement. Some time or other this must happen if that which has gone on is actually to be called a measurement - however dear to our hearts is was to prepare the process throughout as objectively as possible. And that is the *second* insight we have won: *not until this inspection*, which determines the disjunction, does anything discontinuous, or leaping, take place. One is inclined to call this a *mental* action, for the object is already out of touch, is no longer physically affected: what befalls it is already past. But it would not be quite right to say that the psi-function of the object which changes *otherwise* according to a partial differential equation, independent of the observer, should *now* change leap-fashion because of a mental act. For it had disappeared, it was no more. Whatever is not, no more can it change. It is born anew, is reconstituted, is separated out from the entangled knowledge that one has, through an act of perception, which as a matter of fact is not a physical effect on the measured object. From the form in which the psi-function was last known, to the new in which it reappears, runs no continuous road - it ran indeed through annihilation. Contrasting the two forms, the thing looks like a leap. In truth something of importance happens in between, namely the influence of the two bodies on each other, during which the object possessed no private expectation-catalog nor had any claim thereunto, because it was not independent.

### 11. Resolution of the "Entanglement" Result Dependent on the Experimenter's Intention

We return to the general case of "entanglement", without having specifically in view the special case, just considered, of a measurement process. Suppose the expectation-catalogs of two bodies A and B have become entangled through transient interaction. Now let the bodies be again separated. Then I can take one of them, say B, and by successive measurements bring my knowledge of it, which had become less than maximal, back up to maximal. I maintain: just as soon as I succeed in this, and not before, then first, the entanglement is immediately resolved and, second, I will also have acquired maximal knowledge of A through the measurements on B, making use of the conditional relations that were in effect.

For in the first place the knowledge of the total system remains always maximal, being in no way damaged by good and exact measurements. In the second place: conditional statements of the form "if for A ... then for B B. For it is *not* conditional and to it nothing at all can be added on relevant to B. Thirdly: conditional statements in the inverse sense (if for B ... then for A ...) can be transformed into statements about A alone, because all probabilities for B are already known unconditionally. The entanglement is thus completely put aside, and since the knowledge of the total system has remaind maximal, it can only mean that along with the maximal catalog of B came the same thing for A.

And it cannot happen the other way around, that A becomes maximally known indirectly, through measurements on B, before B is. For then all conclusions work in the reversed direction - that is, B is too. The systems become simultaneously maximally known, as asserted. Incidentally, this would also be true if one did not limit the measurement to just one of the two systems. But the interesting point is precisely this, that one *can* limit it to one of the two; that thereby one reaches his goal.

*Which* measurements on B and in what sequence they are undertaken, is left entirely to the arbitrary choice of the experimenter. He need not pick out specific variables, in order to be able to use the conditional statements. He is free to formulate a plan that would lead him to maximal knowledge of B, even if he should know nothing at all about B. And it can do no harm if he carries through this plan to the end. If he asks himself after each measurement whether he has perhaps already reached his goal, he does only to spare himself from further, superfluous labor.

What sort of A-catalog comes forth in this indirect way depends obviously on the measured values that are found in B (before the entanglement is entirely resolved: not on more, on any later ones, in case the measuring goes on superfluously). Suppose now that in this way I derived an A-catalog in a particular case. then I can look back and consider whether I might perhaps have found a *different one* if I had put into action a *different* measuring plan for B. But since after all I neither have actually touched the system A, nor in the imagined other case would have touched it, the statements of the other catalog, whatever it might be, must *also* be correct. They must therefore be entirely contained within the first, since the first is maximal. But so is the second. So it must be identical with the first.

Strangely enough, the mathematical structure of the theory by no means satisfies this requirement automatically. Even worse, examples can be set up where the requireement is necessarily violated. It is true that in any experiment one can actually carry out only *one*> group of measurements (always on B), for once that has happened the entanglement is resolved and one leans nothing more about A from further measurements on B. But there are cases of entanglement in which *two definite programs* are specifiable, fo which each 1) must lead to resolution of the entanglement, and 2) must lead to an A-catalog to which the *other can* not possibly lead - whatsoever measured values may turn up in one case or the other. It is simply like this, that the *two series* of A-catalogs, that can possibly arise from the one or the other of the programs, are sharply separated and have in common not a single term.

These are especially pointed cases, in which the conclusion lies so clearly exposed. In general one must reflect more carefully. If two programs of measurement on B are proposed, along with the two-series of A-catalogs to which they can lead, then it is by no means sufficient that the two series have one or more terms in common in order for one to be able to say: well now, surely one of these will always turn up - and so to set forth the requirements as "presumably fulfilled". That's not enough. For *indeed one knows* the probability of every measurement on B, considered as measurement on the total system, and under many ab-ovo-repetitions each one must occur with the frequency assigned to it. Therefore the two series of A-catalogs would have to agree, member by member, and furthermore the probabilities in each series would have to be the same. And that not merely for these two programs but also for each of the infinitely many that one might think up. But this is utterly out of the question. The requirement that the A-catalog that one gets should always be the same, regardless of what measurements on B bring it into being, this requirement, is plainly and simply never fulfilled.

Now we wish to discuss a simple "pointed" example.

**12. An example [7]**

For simplicity, we consider two systems with just *one* degree of freedom. That is, each of them shall be specified through a *single* coordinate q and its canonically conjugate momentum p. The classical picture would be a point mass that could move only along a straight line, like the spheres of those playthings on which small children learn to calculate. p is the product of mass by velocity. For the second system we denote the two determining parts by capital Q and P. As to whether the two are "threaded on the same wire" we shall not be at all concerned, in our abstract consideratin. But even if they are, it may in that case be convenient not to reckon q and Q from the same reference point. The equation $q=Q$ then does not necessarily mean coincidence. The two systems may in spite of this be fully separated.

In the cited paper it is shown that between these two systems an entanglement can arise, which at a particular moment, can be compactly shown in the two equations: $q = Q$ and $p = -P$. That means: *I know*, if a measurement of q on the system yields a certain value, that a Q-measurement performed immediately thereafter on the second will give the *same* value, and vice versa; and *I know*, if a p-measurement on the first system yields a certain value, that a P-measurement performed immediately thereafter will give the opposite value, and vice versa.

A single measurement of *q or p or Q or P* resolves the entanglement and makes both systems maximally known. A second measurement on the same system modifies only the statements about *it*, but teaches nothing more about the other. So one cannot check both equations in a single experiment. But one can repeat the experiment *ab ovo* a thousand times; each time set up the same entanglement; according to whim check one or the other of the equations; and find confirmed that one which one is momentarily pleased to check. We assume that all this has been done.

If for the thousand-and-first experiment one is then seized by the desire to give up further checking and then measure q on the first system and P on the second, and one obtains

$q = 4; P = 7;$

can one then doubt that

$q = 4; p = -7$

would have been a correct prediction for the first system, or

$Q = 4; P = 7$

a correct prediction for the second? Quantum predictions are indeed not subject to test as to their full content, ever, in a single experiment; yet they are correct, in that whoever possessed them suffered no disillusion, whichever half he decided to check.

There's no doubt about it. Every measurement is for its system the first. Measurements on separated systems cannot directly influence each other - that would be magic. Neither can it be by chance, if from a thousand experiments it is established that virginal measurements agree.

The prediction catalog $q = 4$, $p = -7$ would of course by hypermaximal.

**13. Continuation of the Example: All Possible Measurements are Entangled Unequivocally**

Now a *prediction* of this extent is thus utterly impossible according to the teaching of Q.M., which we here follow out to its last consequences. Many of my friends remain reassured in this and delcare: what answer a system *would have given* to the experimenter *if*...,--has notihng to do with an actual measurement and so, from our epistemological standpoint, does not concern us.

But let us once more make the matter very clear. Let us focus attention on the system labelled with small letters p, q and call it for brevity the "small" one. then things stand as follows. I can direct *one* of two questions to the small system, either that about q or that about p. Before doing so I can, if I choose, procure the answer to *one* of these questions b a measurement on the fully separated other system (which we shall regard as auxiliary apparatus), or I may intend to take care of this afterwards, My small system, like a schoolboy under examination, *cannot possibly know* whether I have done this or for which questions, or whether and for which I intend to do it later. From arbitrarily many pretrials I know that the pupil will correctly answer the first question that I put to him. From that it follows that in every case he *knows* the answer to *both* questions. That the answering of the first question, that it pleases me to put to him, so tires or confuses the pupil that his further answers are worthless, changes nothing at all of this conclusion. No school principal would judge otherwise, if this situation repeated itself with thousands of pupils of similar provenance, however he much he might wonder *what* makes all the scholars so dim-witted or obstinate after the answering of the first question. he wuold not come to think that his, the teacher's, consulting a textbook first suggests to the pupil the correct answer, or even, in the cases when the teacher chooses to consult it only after ensuing answers by the pupil, that the pupil's answer has changed the text of the notebook in the pupil's favor.

Thus my small system holds a quite definite answer to the q-question and to the p-question in readiness fpr the case that one or the other is the first to be put directly to it. Of this preparedness not an iota can be changed if I should perhaps measure the Q on the auxiliary system (in the analogy: if the teacher looks up one of the questions in his notebook and thereby inded ruins with an inkblot *the* page where the other answer stands). The quantum mechanician maintains that after a Q-measurement on the auxiliary system my small system has a psi-function in which "q is fully sharp, but p fully indeterminate". And yet, as already mentioned, not an iota is changed of the fact that my small system also has ready an answer to the p-question, and indeed the same one as before.

But the situation is even worse yet. Not only to the q-question and to the p-question does my clever pupil have a definite answer ready, but rather also to a thousand others, and indeed without my having the least insight into the memory technique by which he is able to do it. p and q are not the only variables that I can measure. Any combination of them whatsoever, for example

p^2 + q^2

also corresponds to a fully definite measurement according to the formulation of Q.M. Now it can be shown[8] that also for this the answer can be obtained by a measurement on the auxiliary system, namely by measurement of P^2 + Q^2, and indeed the answers are just the same. By general rules of Q.M. this sum of squares can only take on a value from the series

hbar, 3.hbar, 5.hbar, 7.hbar, ...

The answer that ym small system has ready for the (p^2+q^2)-question (in case this should be the first it must face) must be a number from this series. --It is very much the same with measurement of

p^2 + a^2 . q^2

where a is an arbitrary positive constant. In this case the answer must be, according to Q.M. a number from the following series

a.hbar, 3a.hbar, 5a.hbar, 7a.hbar, ...

For each numerical value of a one gets a different question, and to each my small system holds ready an answer from the series (formed with the a-value in question).

Most astonishing is this: these answers cannot possibly be related to each other in the way given by the formulas! For let q' be the answer held ready for the q-question, and p' for the p-question, then the relation

(p'^2 + a^2 . q'^2) / (a.hbar) = an odd integer

cannot possibly hold for given numerical values q' and p' and for *any positive numer a*. This is by no means an operation with imagined numbers, that one cannot really ascertain. One can in fact get two of the numbers, e.g., q' and p', the one by direct, the other by indirect measurement. And then one can (pardon the expression) convince himself that the above expression, formed with the numbers q' and p' and an arbitrary a, is not an odd integer.

The lack of insight into the relationships among the various answers held in readiness (into the "memory technique" of the pupil) is a total one, a gap not to be filled perhaps by a new kind of algebra of Q.M. The lack is all the stranger, since on the other hand one can show: the entanglement is already uniquely determined by the requirements q = Q and p = -P. If we know that the coordinates are equal and the momenta equal but opposite, then there follows by quantum mechanics a *fully determined* one-to-one arrangement of *all possible* measurements on both systems. For *every* measurement on the "small" one the numerical result can be procured by a suitably arranged measurement on the "large" one, and each measurement on the large stipulates the result that a particular measurement on the small would give or has given. (Of course in the same sense as always heretofore: only the virgin measurement on each system counts.) As soon as we have brought the two systems into the situation where they (briefly put) coincide in coordinate and momentum, then they (briefly put) coincide also in regard to all other variables.

But as to how the numerical values of all these variables of *one* system relate to each other we know nothing at all, even though for each the system must have a quite specific one in readiness, for if we wish we can learn it from the auxiliary system and then find it always confirmed by direct measurement.

Should one now think that because we are so ignorant about the relations among the variable-values held ready in *one* system, that none exists, that far-ranging arbitrary combination can occur? That would mean that such a system of "*one* degree of freedom" would need not merely *two* numbers for adequately describing it, as in classical mechanics, but rather many more, perhaps infinitely many. It is then nevertheless strange that two systems always agree in *all* variables if they agree in two. Therefore one would have to make the second assumption, that this is due to our awkwardness; would have to think that as a practical matter we are not competent to bring two systems into a situation such that they coincide in reference to two variables, without *nolens volens* bringing about coincidence also for all other variables, even though that would not in itself be necssary. One wold have to make these *two* assumptions in order not to perceive as a great dilemma the complete lack of insight into the interrelationship of variable values within one system.

**14. Time-dependence of the Entanglement. Consideration of the Special Role of Time**

It is perhaps not superfluous to recall that everything said in sections 12 and 13 pertains to a single instant of time. The entanglement is not constant in time. It does continue to be a one-to-one entanglement of *all* variables, but the arrangement changes. That means the following. At a later time t one can very well again learn the values of q or of p that *then* obtain, by a measurement on the auxiliary system, but the measurements, that one must undertake thereto on the auxiliary system, are *different*. Which ones they should be, one can easily see in simple cases. It now of course becomes a question of the forces at work within each of the two systems. Let us assume that no forces are working. For simplcity we will set the mass of each to be the same and call it m. Then in the classical model the momenta p and P would remain constant, since they are still the masses multiplied by the velocities; and the coordinates at time t, which we shall distinguish by giving them subscripts t, (q_t, Q_t), would be calculated from the initial ones, which henceforth we designate q,Q, thus:

$q_t = q + (p/m)t$ $Q_t = Q + (P/m)t$

Let us first talk about the small system. The most natural way of describing it classically at time t is in terms of coordinate and momentum *at this time*, i.e., in terms of q_t and p. But one may do it differently. In place of q_t one could specify q. It too is a "determining part at time t", and indeed at every time t, and in fact one that does not change with time. This is similar to the way in which I can specify a certain determining part of my own preson, namely my *age*, either through the hnumber 48, which changes with time and in the system corresponds to specifying q_t, or through the number 1887, which is usual in documents and corresponds to specifying q. Now according to the foregoing:

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

Similarly for the second system. So we take as determining parts

for the first system $q_t - (p/m)t$ and p. for the second system $Q_t - (P/m)t$ and P.

The advantage is that *among these the same entanglement goes on indefinitely*:

$q_t - (p/m)t = Q_t - (P/m)t$ $p = -P$

or solved:

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

So that what changes with time is just this: the coordinate of the "small" system is not ascertained simply by a coordinate measurement on the auxiliary system, but rather by a measurement of the aggregate

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

Here however, one must not get the idea that maybe he measures Q-t *and* P, because that just won't go. Rather one must suppose, as one always must suppose in Q.M., that there is a direct measurement procedure for this aggregate. Except for this change, *everything* that was said in Sections 12 and 13 applies at any point of time; in particular there exists at all times the one-to-one entanglement of *all* variables together with its evil consequences.

It is just this way too, if within each system a force works, except that then q_t and p are entangled with variables that are more complicated combinations of Q_t and P.

I have briefly explained this in order that we may consider the following. That the entanglement should change with time makes us after all a bit thoughtful. Must perhaps all measurements, that were under discussion, be completed in very short time, actually *instantaneously*, in zero time, in order that the unwelcome consequences be vindicated? Can the ghost be banished by reference to the fact that measurements take time? No. For each single experiment one needs just *one* measurement on each system; only the virginal one matters, further ones apart from this would be without effect. How long the measurement lasts need not therefore concern us, since we have no second one following on. One must merely be able to so arrange the two virgin measurements that they yield variable values for the same definite *point* of time, known to us in advance - known in advance, because after all we must direct the measurements at a pair of variables that are entangled at just this point of time.

"Perhaps it is not possible so to direct the measurements?"

"Perhaps. I even presume so. Merely: *today's* Q.M. must require this. For it is now set up so that its predictions are always made for a *point* of time. Since they are supposed to rlate to measurement results, they would be entirely without content if the relevant variables were not measurable *for* a definite point of time, whether the measurement itself lasts a long or a short while."

When we *learn* the result is of course quite immaterial. Theoretically that has as little weight as for instance the fact that one needs several months to integrate the differential equations of the weather for the next three days. --The drastic analogy with the pupil exmaination misses the mark in a few points of the law's letter, but it fits the spirit of the law. The expression "the system knows" will perhaps no longer carry the meaning that the answer comes forth from an instantaneous situation; it may perhaps derive from a succession of situations, that occupies a finite length of time. But even if it be so, it need not concern us so long as the system somehow brings forth the answer from within itself, with no other help than that we tell it (through the experimental arrangement) *which* question we would like to have answered; and so long as the answer itself is uniquely tied to a *moment* of time: which for better or for worse must be presumed for every measurement to which contemporary Q.M. speaks, for otherwise the quantum mechanical predictions would have no content.

In our discussion, however, we have stumbled across a possibility. If the formulation could be so carried out that the quantum mechanical predictions did not or did not always pertain to a quite sharply defined point of time, then one would also be freed from requiring this of the measurement results. thereby, since the entangled variables change with time, setting up the antinomical assertions would become much more difficult.

That prediction for sharply-defined time is a blunder, is probable also on other grounds. The numerical value of time is like any other the result of observation. Can one make exception just for measurement with a clock? Must it not like any other pertain to a variable that in general has no sharp value and in any case cannot have it simultaneously with *any* other variable? If one predicts the value of *another* for a particular *point of time*, must one not fear that both can never be sharply known together? Within contemporary Q.M. one can hardly deal with this apprehension. For time is always considered a priori as known precisely, although one would have to admit that every look-at-the-clock disturbs the clock's motion in uncontrollable fashion.

Permit to repeat that we do not possess a Q.M. whose statements should *not* be valid for sharply fixed points of time. It seems to me that this lack manifests itself directly in the former antinomies. Which is not to say that it is the only lack which manifests itslef in them.

**15. Natural Law or Calculating Device?**

That "sharp time" is an anomaly in Q.M. and that besides, more or less independent of that, the special treatment of time forms a serious hindrance to adapting Q.M. to the *relativity principle*, is something that in recent years I have brought up again and again, unfortunately without being able to make the shadow of a useful counterproposal.[9] In an overview of the entire contemporary situation, such as I have tried to sketch here, there comes up, in addition, a quite different kind of remark in relation to the so ardently sought, but not yet actually attained, "relativisation" of Q.M.

The remarkable theory of measurement, the apparent jumping around of the psi-function, and finally the "antinomies of entanglement", all derive from the simple manner in which the calculation methods of quantum mechanics allow two separated systems conceptually to be combined together into a single one; for which the methods seem plainly predestined. When two systems interact, their psi-functions, as we have seen, do not come into interaction but rather they immediately cease to exist and a single one, for the combined system, takes their place. It consists, to mention this briefly, at first simply of the *product* of the two individual functions; which, since the one function depends on qute different variables from the other, is a function of all these variables, or "acts in a space of much higher dimension number" than the individual functions. As soon as the systems begin to influence each other, the combined function ceases to be a product and moreover does not again divide up, after they have again become separated, into factors that can be assigned individually to the systems. Thus one disposes provisionally (until the entanglement is resolved by an actual observation) of only a *common* description of the two in that space of higher dimension. This is the reason that knowledge of the individual systems can decline to the scantiest, even to zero, while knowledge of the combined system remains continually maximal. Best possible knowledge of a whole does *not* include best possible knowledge of its parts - and that is what keeps coming back to haunt us.

Whoever reflectes on this must after all be left fairly thoughtful by the following fact. the conceptual joining of two or more systems into *one* encounters great difficulty as soon as one attempts to introduce the principle of special relativity into Q.M. Already seven years ago P.A.M. Dirac found a startlingly simple and elegant relativistic solution to the problem of a single electron.[10] A series of experimental confirmations, marked by the key terms electron spin, positive electron, and pair creation, can leave no doubt as to the basic correctness of the solution. But in the first place it does nevertheless very strongly transcend the conceptual plan of Q.M. (that which I have attempted to picture *here*),[11] and in the second place one runs into stubborn resistance as soon as one seeks to go forward, according to the prototype of non-relativistic theory, from the Dirac solution to the problem of several electrons. (This shows at once that the solution lies outside the general plan, in which, as mentioned, the combining together of subsystems is extremely simple.) I do not presume to pass judgment on the attempts which have been made in this direction.[12] That they have reached their goal, I must doubt first of all because the authors make no such claim.

Matter stand much the same with another system, the electromagnetic field. Its laws are "relativity personified", a *non*-relatviistic treatment being in general impossible. Yet it was this field, which in terms of the classical model of heat radiation provided the first hurdle for quantum theory, that was the first system to be "quantized". That this could be successfully done with simple means comes about because here one has things a bit easier, in that the photons, the "atoms of light", do not in general interact directly with each other,[13] but only via the charged particles. Today we do not as yet have a truly unexceptionabl quantum theory of the electromagnetic field.[14] One can go a long way with *building up out of subsystems* (Dirac's theory of light[15]), yet without qute reaching the goal.

The simple procedure provided for this by the non-relativistic theory is perhaps after all only a convenient calculational trick, but one that today, as we have seen, has attained influence of unprecedented scope over our basic attitude toward nature.

My warmest thanks to Imperial Chemical Industries, London, for the leisure to write this article.

**Notes**

* Box 79, Route 1, Millington, Md. 21651.

[1] E. Schrödinger, "Die gegenwärtige Situation in der Quantenmechanik", *Naturwissenschaften* 23: pp.807-812; 823-828; 844-849 (1935).

[2] A. Einstein, B. Podolsky, and N. Rosen, Phys. Rev. 47: p.777 (1935).

[3] E. Schrödinger, *Proc. Cambridge Phil. Soc.* 31: p.555 (1935); *ibid.*, 32: p.446 (1936).

[4] h = 1.041 x 10^(-27) erg sec. Usually in the literature the 2.pi-fold of this (6.542 x 10^(-27) erg sec) is designated as h and for *our* h an h with a cross-bar is written. [Transl. Note: In conformity with the now universal usage, hbar is used in the translation in place of h.]

[5] For illustration see Fig. 5 or 6 on p.375 of the 1927 volume of this journal; or Fig. 1, p.734 of the preceding year's volume (1934), though these are proton tracks.

[6] Obviously. We cannot fail to have, for instance, statements on the relation of the two to each other. For that would be, at least one of the two, something in addition to its psi-function. And such there cannot be.

[7] A. Einstein, B. Podolsky, and N. Rosen, *Phys. Rev.* 47: 777 (1935). The appearance of this work motivated the present - shall I say lecture or general confession?

(Paris, 1931); *Cursos de la Universidad Internacional de Verano en Santander*, 1: p.60 (Madrid, Signo, 1935).

[10] *Proc. Roy. Soc. Lond.* A117: p.610 (1928).

[11] P.A.M. Dirac, *The Principles of Quantum Mechanics*, 1st ed., p.239; 2nd ed. p.252. Oxford: Clarendon Press, 1930 or 1935.

[12] Herewith a few of the more important references: G. Breit, *Phys. Rev.* 34: p.553 (1929) and 39: p.616 (1932); C. Mo/ller, *Z. Physik* 70: p.786 (1931); P.A.M. Dirac, *Proc. Roy. Soc. Lond.* A136: p.453 (1932) and *Proc. Cambridge Phil Soc.* 30: p.150 (1934); R. Peierls, *Proc. Roy. Soc. Lond.* A146: p.420 (1934); W. Heisenberg, *Z. Physik* 90: p.209 (1934).

[13] But this holds, probably, only approximately. See M. Born and L. Infled, *Proc. Roy. Soc. Lond.* A144: p.425 and A147: p.522 (1934); A150: p.141 (1935). This is the most recent attempt at a quantum electrodynamics.

[14] Here again the most important works, partially assignable, according to their contents, also according to the penultimate citation: P. Jordan and W. Pauli, *Z. Physik* 47: p.151 (1928); W. Heisenberg and W. Pauli, *Z. Physik* 56: p.1 (1929); 59: p.168 (1930); P.A.M. Dirac, V.A. Fock, and B. Podolsky, ,cite>Physik. Z.

Sowjetunion 6: p.468 (1932); N. Bohr and L. Rosenfeld, *Danske. Videns. Selsk.* (math.-phys.) 12: p.8 (1933).

[15] An excellent reference: E. Fermi, *Rev. Mod. Phys.* 4: p.87 (1932).

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