THE CURRENT SITUATION IN QUANTUM MECHANICS

ERWIN SCHRÖDINGER (1887-1961)

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1. THE PHYSICS OF MODELS

In the second half of the nineteenth century an Ideal of the exact description of nature was achieved that is the fruit of hundreds of years of research and an aspiration for eons. Built on the kinetic theory of gases and the mechanical theory of heat, it is described as ‘classical,’ and has the following characteristics.

For those natural objects for which one would like to comprehend their observed behavior, one constructs an image, based on experiment but without excluding intuition, that is accurate in all details, actually, in view of practical limits, more exact than obtainable by any possible empirically means. The precision of this image should compare with that of a mathematical construction or a geometrical figure, which, from a limited number of specifications, allows the calculation of all other details. For example, knowing one side and two angles of a triangle is sufficient to calculate the remaining sides and angle, the inscribed circles and their radii, etc. Such an image would differ from geometric figures only in that it exists in time and must be configured in four dimensional space-time as precisely as a geometrical figure in three. That is, the difference is (as is obvious) that such an image evolves in time and can take on various *states;* and, if a state in all details is given at a particlar time, then not only are all other details for this moment to be calculable (as for a triangle), but also all details for later times as well. An integral part of such an image is its intrinsic ability to evolve in a particular way; that is, if left alone, it will evolve in time continuously through a particular progression of states. This is its essential nature, its hypothetical basis the existence of which, as noted above, one presupposes on intuition.

Naturally, no one is so simple minded as to think that a totally faithful image can be achieved for anything in the real world. This implicit reservation is revealed already by the fact that such a fabricated image is called a *picture* or *model*. With the uncompromised clarity not obtainable without certain arbitrary simplifications, one seeks to show only that a particular hypothesis can be verified in detail without introducing even more arbitrary input into the tedious calculations exploring its various consequences. However, because of practical constraints, one actually achieves only what a clever fellow could interpolate directly from empirical data. In any case, it should be clear what input in the hypotheses of the model is arbitrary and is, therefore, to be revised if calculation does not conform to observation—an eventuality that one must be forever prepared to accept. When many experiments confirm the model, it is considered to faithfully represent the reality of the object in all important aspects. On the other hand, if disagreement is found, it is *not* taken as a setback, rather as the fortuitous identification of an inadequacy just where the model is to be improved thereby fostering convergence to an ever more accurate model.

This classical method of achieving precision of a model aims at isolating the unavoidable kernel of arbitrariness in the hypotheses set, like nuclei in cells, ready for modification in the course of the developments spurred on by new empirical data. One might say that this method is based on the belief that *somehow* the initial conditions *actually* determine uniquely time evolution, or that a *complete* model would agree *exactly* with reality thereby permitting calculation of outcomes for all experiments. Perhaps even, it's the other way around, this belief is based on the model. Nevertheless, it is most likely that this process of model development consists of an endless series, and that, the notion of a "complete model" is oxymoronic, similar to the phrase "largest whole number."

The foundation for all that follows is a clear conception of what is meant by the *specifications* of a state in the *model*. It is especially important to distinguish the difference between a particular *model* and a particular *state* within that model. For example, RUTHERFORD's model of the hydrogen atom consists of two point masses. As specifications one could select the six coordinates and six momentum components of the two masses—that makes twelve items altogether. On the other hand, one could alternately take the coordinates of the location and the velocity of the center of mass and also the vector separation of the two masses and its angular orientation with time derivatives; again this makes twelve items. It is not part of RUTHERFORD's model that these twelve items have particular values, which do, however, specify a *particular state*. A clear overview of the totality of possible states—without relationships to one another—constituents "the model" or "the model in an arbitrary state." However, a model is not complete given just a particular state, but must also encompass whatever is needed to specify the time evolution from state to state whenever there are no external influences. (For half of the specifications, the other half provides some information, but that half must be declared.) *This* knowledge is latent in the statement: The two point particles have mass m, and M, and charges $+e$ and $-e$, and are, therefore, attracted together by the force e^2/r^2 if r is their separation.

These parameters, i.e., particular values for m, M and e (but naturally *not* r) belong to the specification of the *model* (not a particular state), and they cannot be called *state specifications*. On the other hand, the value of r is exactly such a state specification. In the second set of specifications given above, however, r does in fact appear seventh on the list; but still in the first set r is not an independent 13th specification, as it can be deduced from the others using

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

The number of specification items (usually called *variables* in distinction to model *parameters*, e.g., m, M, e) is unlimited. Twelve appropriate values are sufficient to specify all others at any given moment, that is, the complete state at that moment. No particular twelve values, however, are privileged to be *the specification*. Examples of other, particularly important variables include: energy, the three components of momentum with respect to the center of mass, and the kinetic energy of the center of mass. This latter choice has the particular characteristic that they are variables that can have different values for different states, but retain their values for that sequence of states actually seen under time evolution, and are known, therefore, as *constants of the motion.*

2. THE STATISTICS OF VARIABLES IN QUANTUM MODELS

At the crux of the current formulation of Quantum Mechanics (QM) there is certain dogma, which may still suffer some modification, but which, I sense, will remain dogma. It consists in the belief that models with specifications capable of precisely determining reality, as were presupposed in classical physics, do not exist.

One might think, that for those accepting this dogma, classical models have exhausted their potential. But this is not so. Rather, *they* are used still, not only to criticize negative aspects of the new dogma, but also to describe the reduced mutual determination that remains in the new theory among the same variables of just those classical models used earlier. This transpires as follows:

A. The classical concept of a *state* is abandoned, insofar as a well chosen *half* of the former set of variables allows full state determination. For the RUTHERFORD model, for example, the six position variables, *or* the momentum components (other choices are possible) suffice. The other half then remains undetermined, i.e., these newly redundant specifications can exhibit various degrees of uncertainty. In general in a full set (for the RUTHERFORD model, twelve in number) all can be known only with uncertainty. The degree of uncertainty can be best determined, with guidance from HAMILTONian mechanics, when care is taken in the choice of variables so that they are ordered *pairwise* as canonical conjugates, for which the most elementary example is one in which the coordinate x is matched with the momentum in the same direction p_x (i.e., mass time velocity). Such pairs mutually limit each other's precision with which they can be determined in that the product of their tolerances or variations (symbolized with Δ) cannot be less than a given constant[1]ie.:

(1) $$\Delta x \Delta p_x \geq h.$$

(The HEISENBERG uncertainty relationship.)

B. If all the variables at a given moment are not determined, then naturally at later times they cannot be determined from former values. One could call this the Principle of Causality, but in view of §A, it is nothing really new. If at no time a classical state can be specified, this situation cannot be changed by force. What does change, and then by compulsion, are the *statistics* or *probabilities*. Individual variables can be precise, but

[1] $h = 1.041 \times 10^{-27}$ erg-sec. and in the literature is denoted usually by the symbol $\hbar$, where h itself is then 2π times this value.

others then made imprecise. All in all it is so, however, that the overall precision of a description does not change through time, which is a consequence of the fact that the limits explicated in ¶A are at all times the same.

But now, what exactly do the terms: “imprecise, statistics, and probability” actually mean? QM responds as follows. It commandeers any conceivable measurable variable or specification from classical models as directly measurable, even accepting its unlimited precision, with the proviso that only one is selected. If use of a well selected and limited number of measurements maximally determined an object’s state, as per ¶A is possible, then the new theory has the mathematical machinery to specify a *statistical distribution* at the same moment or for any other time for *any* variable. That is, it is able to give the percentage of cases falling within specific ranges of all variables (i.e., probabilities). The general opinion is that these probabilities in fact are those for the relevant variable giving the likely value to be obtained by a measurement. With a single measurement the accuracy of such a *probability forecast* cannot be verified, except partially if the distribution is sharply peaked in a small interval. In order to fully verify these distributions, the whole experiment, including orientation and preparation measurements, must be fully repeated *very* often, and then consider only those cases where the orientation measurements are identical. In those cases, for a specific variable given the same orientation results, statistical predictions will be verified by measurement—such is the popular opinion.

One must take special care in criticizing this opinion, because it is very difficult to parse; a situation which is a consequence of our language. There is, however, another criticism that arises naturally. No physicist in the classical epoch, having conceived a model, would have been so rash as to believe that specifications for objects in nature are measurable directly in fact. Usually only derived consequences from such models actually turned out to be amenable to experiment. Moreover, it would have been anticipated on the basis of much experience, that long before adequate experimental techniques are developed, the model would have been already substantially modified to accommodate newly found empirical facts. — While the new theory declared the classical model incompetent to regulate the *interrelationships among specifications* (for which its originators has intended it), it does anoint itself competent to certify which *measurements* of the object are in principle doable; this, its founders should have considered an insolent usurpation of possible future developments. Now, is it not presumptuous fantasy, to think that researchers from earlier times, who one hears nowadays did not even know what *measurement* actually is, nevertheless have bequeathed to us unintentionally the instrument to judge what is measurable on hydrogen today?

I hope to show below that this reigning opinion was born of necessity. But first I continue with its description.

3. PROBABILISTIC FORECASTS: EXAMPLES

Nowadays as before, all predictions relate to the specifications of a classical model, i.e., to the locations and velocities of mass points, or energy and momentum and the like. The nonclassical aspect is that only probabilities can be predicted. Ideally with QM, it is considered that the task always involves projecting the maximally accurate probabilities that nature allows for results of measurements to be made momentarily or at another future time based on current measurements. But now, what is the actual situation? In important and in typical cases, it is as follows:

If one measures the energy of a PLANCK oscillator, the probability of finding an energy value between E and E' can be different from zero if between E and E' there is one of the values

$$3\pi h\nu,\ 5\pi h\nu,\ 7\pi h\nu,\ 9\pi h\nu,\ \ldots$$

For all intervals not including one of these values, the probability is zero; i.e., other values are excluded. The acceptable values are odd multiples of the model parameter $\pi h\nu$ ($h =$ PLANCK's constant, $\nu =$oscillator frequency). Two things stand out here. One, there is no relation to previous measurements—they are unnecessary. Two, the value does not suffer uncertainty, in fact it is more precise than any possible measurement.

Another typical example is measurement of angular momentum. In Fig. 1 let M be be a mobile mass point, where the arrow depicts both the measure and direction of its momentum (mass $\times$velocity), O is an arbitrary pivot point in space, the origin of the coordinate system, say, that is not a point of any physical significance, just a geometrical landmark. The angular momentum with respect to O in classical mechanics is given by the product of the momentum vector and the length of the perpendicular to its 'line-of-flight' OF.

In QM, analogous to the energy of an oscillator, angular momentum is quantized; the probability of its values falling in intervals not including the values

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

is zero again; in other words, only one of these values can result from measurement. Once again, it is not dependant on previous measurements. One can well imagine just how substantial this precise prediction is, *much* more so than knowledge of which value or the probability for each permissable value in any particular case. It is also notable that there is no mention of the orientation point O; wherever it is put, the series of admissible values, (2), is the same. Thus, in a model, this point is irrelevant, since the lever arm OF changes continuously as the point O is displaced while the momentum vector remains constant. This example illustrates how QM uses a model to read off which variables can be measured, while it still must designate which interrelationships among these variables that are valid simultaneously.

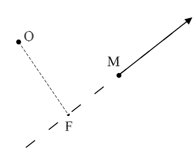

Fig. 1

Does one not get the feeling in both cases that the essential content of what is to be said, can be shoe-horned only with great effort into a prediction of the probability to encounter this or that measurement result of a variable of the classical model? Does one not have the impression that the issue here is the fundamental characteristic of *new* groups of indications having no more than a name in common with classical counterparts? This unease concerns not just exceptional cases, even truly important predictions have this character. While there are actually some tasks approaching the sort for which this means of expression is tailor made, they do not have nearly the same importance. All the more those that one naively fabricates as didactic examples, that have absolutely no importance really. "Given the location of an electron in hydrogen at time $t = 0$; one construes what the probability of its location is at a later time." Who cares?

Literally taken, such predictions do concern the model. But the useful conclusions are not imaginable; and, what is imaginable, is virtually useless.

4. IS AN ENSEMBLE INTERPRETATION TENABLE?

In QM a classical model plays a protean role. Each of its specifications can be, depending on circumstances, an item of interest and acquire a certain reality. But never all together—now it is these, then it is those, and always only *half* of a full compliment needed for a clear determination of the momentary state. What happened to the rest of them? Do *they* not have any reality? Maybe (see below) in just a vague, fuzzy way? Or, do all of them have unique meanings, but such that *knowledge* of only half is possible?

This second supposition is commodious for statistical theory of the sort developed in the latter half of the previous century; especially so while the new theory was born *from it* in analysis by PLANCK from December 1899 on the theory of heat radiation. The essence of this mode of thought is found in the fact that one practically never knows all specifications for a system, but only a few. For the description of a realistic body at a given moment one does not call on *a* state of the model, rather a so-called GIBBSian ensemble, which is an ideal, merely imagined collection of states corresponding to the actual knowledge in hand. The body is then considered to execute just *one of the states* contained in the ensemble. This viewpoint has enjoyed great success. Its greatest triumphs were there, where each of the options cannot be found pertaining to a single member of the ensemble. The body then really does behave erratically, exactly corresponding to probabilistic expectations (think: thermodynamic fluctuations). Thus, one is tempted to attribute quantum uncertainty to the plurality of such possibilities—but such that just which one is realized remains unknown.

Unfortunately, the example given above involving angular momentum shows that this surmise is not tenable. Consider an ensemble in which each member is related to a different point O and with different linear momenta. Then one can chose the length of the momentum vector with respect to any pivot point such that any of the admissible values, (2), is obtained. But then for any other pivot point, O', the values arising might not be among the admissible ones. Transplanting the issue to an ensemble does not resolve this discrepancy. Another example is that of the energy of an oscillator. Consider the case in which its energy has a sharp value, e.g., the lowest one, $3\pi h\nu$, say. The separation of the two mass points (that constitute the oscillator) is then very *imprecise.* In order to be able to transfer these facts to an ensemble, the statistics of the separations should be limited from above so that the potential energy never exceeds $3\pi h\nu$. But this is not what happens in fact, arbitrarily large separations are also included, albeit with diminishing probability. Moreover, this defect is not an ancillary calculational quirk to be corrected without affecting the essential kernel of the theory. Among many other effects, this feature is the basis of GAMOV's quantum explanation for radioactive decay. Such examples are to be found without limit. Note that in all of this there is no mention of 'time.' Nor would it help at all to accept models incorporating "nonclassical" features, say instantaneous "jumping." Even at a single instant, that would not work; there does not exist a collection of classical model momentary states which would adequately cover the totality of quantum predictions. This can also be expressed as follows: if I for any instant attribute a particular (but unknown to me) state to a model system, or equivalently fix *all* values (again, just not known to me) for the model's specifications, there still is no *conceivable* prediction which is not in conflict with a portion of the quantum conclusions.

This is not entirely what one expects when one hears that the results of the new theory, in comparison to classical theory, are never sharp.

5. ARE VARIABLES REALLY INDISTINCT?

The alternative would be to ascribes reality to whatever variables are distinct and sharp; or. more generally put: to attribute to variables exactly that degree of reality corresponding to the sharpness which quantum theory allows them.

That it is not impossible to express in just *one* completely *clear* construct, the degree and sort of indistinctness of all variables, is manifested by the fact that QM has and uses just such an instrument, namely the so-called wave or ψ-function, or sometimes called the system vector. We shall have much more to say about it below. Such functions, being abstract mathematical entities without visualizable content, as is oft noted with respect to the new mode of thought in general, is devoid of consequence. In any case it is a conceptual entity encompassing the indistinctness of all variables at any given instant as clearly and exactly as a classical model does their exactitude with numbers. In addition, its law of time evolution is in no way less clear and precise, for isolated systems, than that for a classical model. Consequently, ψ-functions are appropriate for situations where indistinctness is limited to the scale of unverifiable atomic dimensions. In fact, one has extracted from such functions quite imaginable and commodious notions, for example, the "negative charge cloud" surrounding an atomic nucleus etc. Serious reservations arise, however, whenever this indistinctness should pertain to macroscopic objects for which the is simply misplaced. The state of a radioactive nucleus has presumably just such a degree and type of indistinctness in that neither the time nor direction of a decay of an α-particle are fixed. Confined to the interior of a nucleus, this does not disturb us. The ejected particle can be described, if one seeks an image, as a spherical wave expanding away from the nucleus such that it would impact a spherical detector centered on the nucleus over is whole surface. Such detectors, however, do not respond this way, but flashes at *single* spots, or, to fully respect truth, flashes occur arbitrarily here and there as it is not possible to carry out this exercise with a single radioactive atom. If instead of a spherical detector a space filling ionizable gas is used, the tracks seen depict linear trajectories[2], departing from the α-emitting nucleus (i.e., WILSON cloud chamber tracks of condensation on ions engendered by the expelled particles).

One might even consider a burlesque illustration. Suppose a cat is confined in a box together with, but isolated from, a devilish gadget consisting of a GEIGER-counter to trigger a hammer to smash a vile of lethal prussic acid. Further suppose the counter is set before a miniscule amount of radioactive material which within an hour has an equal likelihood of, or of not, one α-decay. Now, a ψ-function for the total system, cat and gadget, according QM principles, after one hour is a mix of live and dead cat!

The significant point here is that what originally was limited to the microscopic atomic domain, is transfered to a macroscopic arena available to direct observation. This illustration strongly throttles a naive understanding of any "indistinct model" as a representation of reality. In and of itself, there is nothing unclear or contradictory here. It simply illustrates the difference between an out-of-focus photograph and one of a cloud or fog bank.

6. EPISTEMOLOGICAL REFORM

In §4 we saw that it is not possible simply to adapt the classical model and ascribe precise values to indistinct or unknown variables. In §5 we saw that indeterminacy is not just indistinctness, as there are cases where an easily made observation compensates any indeterminacy. So, what remains? To escape this dilemma, epistemology is called to the rescue. Often we are assured that there is no real difference between the state of a natural object and what one knows about this object, or rather what one could know with sufficient effort. *Reality*— it is posited—is actually just that, what one perceives, observes or measures. Thus, if I have at any moment the best possible information on an object allowed by nature's laws, then I am entitled to reject all further questions about that object as *vacuous*, at least insofar as I am convinced that no additional observation could increase

[2]For examples on the pages of this journal, see Figs. 5 & 6, p. 375 of the year 1927 and p. 734 of last year, 1934, where however, the tracks were made by hydrogen nuclei.

my knowledge—without, however, destroying already held information, e.g., by changing its state.

This throws some light on the genesis of the remark made at the end of §2, that I described as rather far reaching: namely that all of a model's quantities in principle are measurable. This, as an article of faith with universal dominion, simply cannot be disregarded if one finds oneself compelled to call upon a rescuing omnipotence for aid in physics methodology.

Reality resists mental imitation via models. So one grants naive realism free reign and falls back directly on the indubitable thesis that in the end *reality* (for a physicist) is only observation or measurement. In which case all ruminations on physics problems are based on and pertain to the results of feasible measurements, as though all other sorts of reality and models thereof are impotent. All numerical quantities that arise in physics calculations then must be designated measurement results. But, as we were not born yesterday so as to be starting afresh today to do science, and already posses a well developed quantum calculus, which now, after substantial successes, we do not wish to abandon, we seem to feel obliged to "dictate from the desk" just which measurements are possible in principle, i..e, which must exist so as to verify our calculus. Since this allows a sharp value for every variable of the model separately (if for only a "half set"), each separately must be precisely measurable. We must allow ourselves no less, having lost our naive realist innocence. We have nothing better than our calculus to show us where Nature has drawn the ignoramus-border, that is, what constitutes the best possible knowledge of an object. Should our calculus be inadequate, then our measurement-reality would depend on the conscientiousness of experimenters to garner information. We must instruct them on just how far they may go, if clever enough. Otherwise, it is to be feared that just there, where further questioning is forbidden, something useful might turn up.

7. WAVE FUNCTIONS AS EXPECTATION CATALOGUES

In the continuation of the explication of orthodoxy, let us turn again to the ψ or wave function. It is the instrument of prediction for probabilities of measurement results. Within it is to be found the totality of valid future expectations, a catalogue as it were. It provides the bridge of relationships and conditions between measurements and subsequent measurements, similar to the model and its current state in classical theory, which is something it has in common with wave functions. It can be, in principle, uniquely determined by a suitable selection of measurements on the object, half as many as would be necessary in the classical case. In this way, the catalogue of expectations is compiled. Thereafter it changes with time, just like the state in a model in classical theory, inevitably and unambiguously ("causally")—the evolution of a ψ-function is governed by a partial differential equation (first order in time and solved from $\partial\psi/\partial t$). This corresponds to an unperturbed motion in classical theory. But this proceeds only so long as no measurement is made. At each measurement, one is required to attribute to a ψ-function an abrupt modification *depending on the numerical results of the measurement* that itself is *not predictable*. All of which says that this secondary sort of change has absolutely nothing to do with the orderly evolution of a ψ-function between measurements. The abrupt change occasioned by measurement relates closely to the matters considered in §5, which are the most interesting aspects of the theory. It constitutes exactly *the* point that requires a break with naive realism, and which *precludes* equating a ψ-function with a state in classical theory, not because unpredictable abrupt alterations cannot be attributed to ontological objects or encompassed in a model, but because an observation of a natural process is as much a part of nature as the object itself.

8. Measurement theory-I

Rejecting realism has logical consequences. A variable *has* in general no particular value preceeding its measurement; that is, measuring it does *not* just reveal the value that it *had* a priori. What does it mean, really? There must exist a criterion for whether a measurement is right or wrong, a method good or bad, accurate or inaccurate—whether it deserves the title 'measurement' or not. Every machination with a meter in the vicinity of an object, from which one at some point takes a reading, cannot really constitute a measurement of this object. Thus, it is rather clear, if reality does not determine measured values, then measured values at least determine reality, which *must* be available *after* a measurement in *the* sense certified by our epistemology, i.e., as measured values. In other words, the sought criterion can be only: reiteration of a measurement must yield the same measured value. With such serial measurement repetition then, the accuracy of the measuring process can be checked and thereby demonstrated that it is not just empty theatrics. It is comforting, that this instruction coincides with the experimentalist's procedure for whom a "true value" is unknown in advance. We capture the essence of this point as follows:

The intentional interaction of two systems (measured object and measurement instrument) is called a measurement of the first system if some diagnostic indicator of the second system (measurement instrument reading) for an immediate repetition of this interaction (where no intervening influence on the measured system is allowed) always yields, within given tolerances, identical results.

This definition surely needs refinement, it is not without defects. The ontic is always more complex than its mathematical representation and difficult to formulate in smooth prose.

Before the first measurement any quantum theoretical forecast *can* be valid. *After* such a measurement *in all cases*: further measurement results must fall within relevant tolerances. That is, the catalogue of predictions (the ψ-function) is altered with respect to the measured variable by measurement. When the measurement procedure is known to be *reliable*, then the first measurement reduces the theoretical pallet of expectations to be within the tolerances, regardless of its previous extent. This constitutes the abrupt alteration of ψ-functions mentioned above. But the fact is that not only the measured variable's catalogue of expectations is unpredictably altered, but also that for other variables, in particular for 'conjugate variables.' If a rather sharp prediction for a particle's momentum prevails before its location is measured beyond that allowed by Eq. (1), then the forecast for the momentum must also have been altered. The quantum calculus automatically takes this effect into account; there exists no ψ-function from which with appropriate methods an expectation not in accord with Eq. (1) can be obtained.

In that the expectation catalogue is altered by measurement, the object itself then becomes no longer useful for checking the complete catalogue of probabilities of the incoming state, in particular for the measured variable which retains its first revealed value. In order to check the whole palette, in other words to check the experiment *that* is the rational behind the prescription in §2, namely to check the probability catalogues encompassed in a wave function, the measurement procedure must be faithfully repeated *ab ovo*. One must prepare identical objects with ψ-functions identical to the one valid for the first measurement. (Note, this repetition, being of the experiment not just of a measurement, is fundamentally different from that mentioned above!) This must be done not just twice, but very often. Thusly, forecast statistics can be verified—per current opinion.

The difference between error limits and statistical scatter *of measurements* on one hand, and theoretical expectation statistics on the other must be kept in mind. They are unrelated, they engage two types of *repetition*, as was just emphasized.

At this point notions regarding constraints on *measurement* can be refined. There are measurement apparatus which hold their readings after use; or, what's worse, the pointer might get stuck accidentally. Reruns then yield the same result repeatedly; and, the experimenter might conclude, following the above principle, that his measurement is somehow special and particularly accurate. But this is a result pertaining not to the object, but to the instrument. In fact, therefore, the above principle is incomplete in an important respect, one that earlier could not be considered easily, namely by neglecting the essential difference between the object and measuring instrument (that only the latter delivers readings, is a superficiality). For example, often an instrument must be reset, as we have just noted, to its initial condition before reuse—as is well known to experimenters. Theoretically this matter is dealt with best by proscribing that all instruments be recalibrated so that for each repetition the same ψ-function pertains when re-exposed to the instrument. In addition, any deliberate perturbing interaction with the object itself must be prevented during "control measurements" i.e., "repetition of the first sort" (leading to measurement error statistics). That is a characteristic difference between object and instrument. For a "repetition of the second sort" (to check quantum forecasts) the object-instrument distinction vanishes; i.e., it is really quite insignificant.

From this we learn that for a second measurement an identically constructed and prepared instrument may be used, it need not be the *identical* instrument; in fact, sometimes instrument exchanges are made just to check the original one. Even totally different types of instruments can be used for sequential measurements (repetition of the first sort) if they yield compatible results, i.e., they measure essentially the same variable and are mutually calibrated.

9. ψ-FUNCTIONS AS STATE DESCRIPTIONS

Rejecting realism engenders certain responsibilities. From a point of view based on classical models, ψ-functions are, in terms of their content, incomplete, they include, seemingly, only 50% of a total description. From the QM stand point, however, they must be complete for reasons that were mentioned at the end of §6. It must be impossible to attach still more valid forecasts to them or else one loses the right to disregard all further demands for more specificity, as pointless.

Therefrom it follows that two different catalogues for the same system under different conditions or at different times, can overlap partially, but never so that one is completely contained in the other. Were it otherwise, it would be possible to enlarge one with additional information, namely the difference of the overlap. The theory takes this automatically into account; there exists no ψ-function yielding all the same expectations as some other, but also additional ones.

Therefore, whenever a ψ-function is altered, be it from within by itself, or from without by measurement, the revamped function always yields some different expectations not extractable from the unaltered version. That is, the catalogue will have not only new predictions, but will also have lost some previous ones. Now, information can be *obtained*, but not thereafter *lost*. The loss of expectations can only mean then that what was correct to begin with, becomes false thereafter. A correct pronouncement can become false, if its *essence* changes. I find the following conclusion in this regard faultless:

THEOREM I: *If there are different ψ-functions for a given system, then the system is in different states.*

If the matter concerns systems for which there exists only one ψ-function, then the inverse obtains:

THEOREM II: *Given identical ψ-functions, the system is in the same state.*

This inverse does not follow directly from the first without also assuming completion. Were one to hold that a difference for the same catalogues as possible, this would admit that it still does not give answers to all questions. The language used by nearly all authors reflects this point, in that they are essentially fabricating a new sort of reality, and I believe fully legitimately. Such language is, moreover, not tautological, not simply a definition for the word "state." Without the assumption of catalogue completeness, alterations to ψ-functions would be effected simply by acquisition of new information.

There is still one other objection to THEOREM I that must be assessed. It could be said, that each individual statement of conviction involved here, the actual object of concern, is really no more than a probability statement which in the catalogue *rightly* or *wrongly* does not concern an individual item but only the collective, that arises in that one has prepared the same system a thousand times (so as to carry out identical measurements; see §8). That is true, but still all members of this collective must be verified as absolutely identical, because for each the identical ψ-function obtains, i.e., they have the identical catalogue of expectations and differences not already in the catalogue are inadmissible (see the reasoning for THEOREM II). This collective consists, then, of identical single items. If an expectation for *it* is false, then each single item must have been altered, otherwise the collective would be false too.

10. MEASUREMENT THEORY-II

Discussion (§7) and clarification (§8) above led to the notion that *measurement* suspends the law of continuous ψ-function time evolution, interrupting it with a lawless alteration dictated by the measurement result. However, during measurement some other unnatural law cannot reign; measurement is as natural a process as any other, and therefore unable to violate natural law. As ψ-functions are in fact interrupted, they (as was noted in §7) cannot be considered candidate depictions of objective events as with classical models. Exactly this is the idea crystallized in the previous section.

Let's try, in outline, to draw contrasts: 1. The collapse of the expectation catalogue is *unavoidable*; if measurement is to make sense then a good measurement result *must* be no less than its numerical outcome. 2. The abrupt change *cannot* be regulated by the time evolution law, as it depends on the numerical result, which arises first at the moment of measurement. 3. This change includes (because of completeness) loss of knowledge, and as knowledge as such is indestructible, this means that the item itself has changed—also abruptly and unpredictably, in contrast to normal evolution.

Does this make sense? The situation is not at all simple; and concerns the most difficult and interesting aspect of the theory. To begin, we must attempt to capture the essence of the interaction of object and instrument. To do so, first some abstract technicalities need be explicated.

The crux is this. If one has two fully separated bodies, or better said, for each a separate expectation catalogue (i.e., maximal knowledge), then naturally one also has complete knowledge for the pair together, that is, considering them as a totality, not separate units, whose combined future is of interest[3].

But now, the inverse is not true. *Maximal knowledge of the pair does not include maximal knowledge of the parts, even when they at the moment are widely separated and do not interact.* It can be, however, that a portion of what one knows pertains to the interrelationship or conditionality between the pair (we limit ourselves here to two items) thusly: if a particular measurement gives a *particular* result for one, then there will be particular expectation statistics for the other; but if the first result were something else, then other, different expectation statistics obtain for the second. Or further, if a third result is obtained on the first, then still other expectation statistics obtain for the other, and so forth. This structure resembles a complete disjunction of all numerical results that the considered measurement of the first variable can deliver. In this way the measurement process (or what is the same, a variable of the second system) is coupled to an as yet undetermined value of the first variable, and of course, visa versa. When that is the case, i.e., when such conditional probabilities are in the total system catalogue, *then it cannot be maximal for the component systems.* Thus, when the content of two maximal single catalogues should suffice by itself as a total catalogue, these conditional probabilities are inadmissible.

Such qualified predictions are, moreover, not something new popping up here. They exist in every expectation catalogue. If one knows the ψ-function and makes a measurement giving a particular result, then one still knows the ψ-function, *voilà tout*. But in the case at hand, as the whole system consists of two fully separated parts, the situation is quite different. Here it makes sense to differentiate between a measurement on one part or on the other. This in turn justifies the desire for a complete catalogue for each individually; otherwise, it could be possible that a portion of that information on the mutual conditional aspects is wasted, so-to-speak, and leaves the wish for separate catalogues unfulfilled—despite the fact that the catalogue for the whole is complete, that is, its ψ-function is known.

Let us dwell here for a moment. The just made remark in all its abstraction actually says everything. The best possible knowledge for a total system does not included necessarily the same for its parts. Translated into the terminology of §9: The total system is in a particular state, but the parts by themselves not so.

-How so? Any system must be in *some* state.

=No! State equals ψ-function, and is maximal knowledge—this does not imply that actually I have gained possession of such knowledge. Maybe I'm lazy, in which case the system would be in no state at all!

-Fine, then the agnostic question-prohibition is not yet ratified and I can consider that for each part there is a state (i.e., ψ-function) , and, just don't know which.

=Not so! Saying "I just don't know" is unacceptable. After all, maximal information for the total system is indeed at hand.

The insufficience of a ψ-function as an ersatz for a model, results exclusively from the fact, that one does not always have it. Once one does have it, it surely is a good and trusty description of the state. But, actually one doesn't really have it in some cases where one expects to have it. In which case one may not assert that "actually there is one, but I just don't know it;" the chosen underlying epistemology forbids that. "It" is namely a sum of knowledge, and knowledge known to nobody is no knowledge at all.

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[3] Naturally, information on the relationship of the two to each other cannot be missing. If it were, then it could enter into one or the other of the ψ-functions; and that is precluded by definition.

We continue. That a portion of the knowledge in the form of disjoint conditionalities *between* the parts hangs in the blue, cannot be true for the case where the parts are assembled from opposite ends of the universe and have had no interaction, so much so that the parts would be totally 'unaware' of each other. Measuring one cannot reveal anything about the other, its future, its fate or whatever. Whenever in fact there is an "entanglement of predictions," obviously it can arise only if the parts at some time in the past constituted a *single* system, that is, interacted so as to leave traces thereof. If two separate bodies which separately are maximally known come to a situation in which they affect each other, and then move apart again, a conditional predictably arrises that I have called *entanglement* of our knowledge of the parts. The common expectation catalogue logically consists originally out of the sum of the catalogues of the parts; but during interaction, catalogues evolve or develop according to well known laws (of course, here measurement is not involved). Knowledge remains maximal, but in the end, as the parts move apart the catalogue is not restored to the sum of those for the parts. What remains *therefrom*, is, maybe even severely, less than maximal. Notice the contrast here to classical model theory, where naturally the initial state and the known outcomes or final states are all individually known.

The measurement process described in §8 falls directly under this scheme, *if* we apply it to the total object complex and instruments. In constructing an objective picture of this process, as would be done for any other process, we might hope to clarify the weird jumping associated with ψ-functions, maybe even banish it altogether. Moreover, one body is now the object, while the other is the instrument. In order to preclude all external influences, the setup should be so imagined that the measuring instrument has built into it an automatic control mechanism so it can sneak up on the object, measure, and later sneak away. The actual reading of the meter shall be postponed because first we wish to determine just what "objectively" transpired; but we have arranged that the measurement result itself be recorded and read out later, as is often done in fact nowadays.

So, what does this automatic measurement amount to? Again, we start with a maximal expectation catalogue; but, the read-out is, of course, not in it. With respect to the instrument, the catalogue is quite incomplete, and does not even tell us where the recording pen did its writing. (Recall the poisoned cat.) So one gets the idea that our knowledge has sublimated certain conditionalities: *if* the pen reaches filer mark 1, *then* for the object probability is this or that, *if* filer mark 2, *then* another probability, *if* 3, *then* a third, etc. Has the ψ-function of the object made a jump? Or has it, on the other hand, evolved according to the appropriate law (the differential equation)? Neither, actually; it ceased to exist. It has, according to inviolable law for the *total ψ-function,* gotten tangled with that of the instrument. *The expectation catalogue for the object has split into a conditional disjunction of expectation catalogues*, like an artful folding map. In each fold one finds the probability of its own occurrence—transfered from the original catalogue for the object. But the issue now is: which fold of the map is relevant? The answer is given by the filer mark.

What happens if it is not read? Say, it is recorded on photographic paper that is accidentally over-exposed before development, or just lost. Then this unfortunate measurement not only teaches us nothing new, but actually has reduced our knowledge. Knowledge that one almost had earlier, is lost forever. One has to now carefully reorganize everything so as to regain what went astray.

So, what has this analysis brought us? *First,* insight into the disjunctive split of expectation catalogues, which transpires quite continuously via embedding into the common catalogue for object and instrument. From such multiplicity the object can be liberated finally only when a mortal experimenter finally reads the instrument. At some time that must happen, if the enterprise is really to constitute a measurement—since mortals conscientiously always work with maximal objectivity. That is the *second* bit of insight: only *after reading the meter*, which resolves the multiplicity, does something discontinuous or jump-like, occur. One is tempted to perceive it as a *mental* act, since the object itself has in fact already moved on and is no longer physically involved, whatever else happens. But still it would not be totally correct to say that a ψ-function for the object, *otherwise* always evolving according to a differential equation independent of an observer, changes *now* jumpwise as a consequence of a mental act. There for an instant, it existed no more. What isn't, can't change. Then suddenly it is resurrected, resuscitated out of the entangled knowledge held by an observer by a process that has no physical effect on the object. From the former ψ-function to the new one, there is no continuous path as it passes through annihilation. But by fore-and-after contrasts, it appears jump-like. In fact there is important developments in between: namely the effect of the two parts on one another, during which the object possessed no private catalogue, and had no right to one, because it did not exist independently.

11. Entanglement resolution, an intervention of consciousness

Let us return to the general issue of "entanglement" without focusing on measurement. Consider the expectation catalogues for two objects, *A* and *B*, entangled by cause of temporary interaction. Now let the two be separated. Then one can select one, *B* say, and his under-maximal knowledge of it and boost it by means of measurements up to maximal knowledge. It can be said, that as soon as that is accomplished, but not sooner, first of all the entanglement will have been resolved, and secondly, by exploitation of the conditionalities, the best maximal knowledge of *A* will have been gained also.

In the first place knowledge of the whole system remains maximal always, because good and accurate measurements never would expunge knowledge. Secondly, conditional statements of the form: "if at $A\cdots$, then at $B\cdots$," can no long be valid as soon as a maximal catalogue for *B* is compiled, which would not be *conditioned* and so, no more information concerning *B* can be invested into it. Third, conditional statements in the opposite direction ("if at $B\cdots$, then at $A\cdots$") can be converted into statements only on *A*, because all probabilities for *B* are completely known in advance. The entanglement is thereby totally resolved, and as knowledge of the whole system remains, it must be attributed to the maximal catalogue for *B*, as well as another one for *A*.

It cannot happen that *A* indirectly through measurements on *B*, becomes maximally known, before even *B* does. If it did, then all arguments would work in reverse, that is, for *B* also. The systems become maximally known mutually. Incidentally, we note that, that would be so also if measurement were not focused on just one of the two parts. The curious factor here is, that focus *can* be limited to one part and still permit getting good results.

Which measurements and in which order they are made is up to the discretion of the experimenter. He need not select a particular variable in order to exploit the conditionalities. He may confidently make a plan bringing him maximal knowledge of *B*, even when he knew nothing of *B*. No damage is thereby done; even if he, wondering after each step whether he is finished, just checks so as to spare useless further effort.

Which *A*-catalogue of this type indirectly arises, depends naturally on the numerical measurement results derived from *B* (before the entanglement is completely resolved, but not by later measurements, in case further superfluous ones are made). Suppose now, I had found this sort of *A*-catalogue in a particular case. Then I can contemplate and consider whether I would have found a different *A*-catalogue has I used *another* plan to measure *B*. Because I have not had contact with the system *A* really, nor would I have had it in the contemplated alternative, so the predictions of the other catalogue, whatever it could have been, are all *also* correct. They must all be in the first catalogue also, as it was maximal. That was true too for the second; so they must be identical.

Peculiarly the mathematical formalism does not automatically satisfy this requirement. Even further, examples can be constructed where this requirement is violated necessarily. While for each attempt only *one* scheme of measurements (always on *B*) can be executed in that as soon as it is, the entanglement is resolved and one finds, via further measurements on *B*, no more knowledge over *A*. But there are cases of entanglement, in which for measurements on *B*, there are *two distinct programs*, for which 1.) the entanglement must be resolved, and 2.) that lead to an *A*-catalogue to which the other absolutely cannot lead—whatever the numerical measurement result in this or that experiment. It is simply the case, that two *rows* of *A*-catalogues, that for one or the other program could be employed, are perfectly distinct and have no elements in common.

There are particularly critical cases, in which this situation arises often. In general one must consider matters more carefully. If two programs for the measurement of *B* are then available, and also two rows of *A*-catalogues to which they could lead, then it is not enough that the two rows have one or more common elements in order to be able to say: 'well, one or the other can always be used,' and thereby put aside the above requirement as "presumably fulfilled." That is not enough since one *knows* the probability of every measurement on *B*, seen as a measurement on the total system, and by multiple ab-ovo-repetitions each must enter with its attributed frequency. The two rows of *A*-catalogues must therefore agree element for element and further the probabilities in each must be the same. And that not only for the two programs, rather for each of endlessly many conceivable variants. But this is by no means the issue here. The demand that the *A*-catalogue, which one gets, must be the same regardless of which measurements are expected to be made on *B*, is never in any way accomplished.

Let us consider now a "peculiar" example.

12. An Example-I[4]

For the sake of simplicity, let us consider two systems with only *one* degree of freedom. That is, each shall be characterized by only one variable q and its canonically conjugate partner p. The classical image is that of a mass point confined to one dimensional motion like a bead in an abacus. p is the product of mass and velocity. For the second system we use the use large Q and P. Although we do not insist on having them mounted on a wire, if they are we nevertheless do not insist that the origin for both variables is the same, so that $q = Q$ does not imply that they are coincident. The two systems can be completely separated.

In the cited paper the two possibly entangled systems, for which *at a particular instant, all that follows relates,* are succinctly described by the equations

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

That is: *I know* if a measurement of q an the first system yields a particular value, the same value will result for Q-measurement on the second, and visa versa; *and I know* that if a

[4]A. Einstein, B. Podolsky and N. Rosen, *Phys. Rev.* **47**, 777 (1935). Their paper stimulated me to write this—should I call it screed or testimony?

p-measurement made on the first gives a particular value, a P-measure yields the opposite value, and visa-versa.

A single measurement of *q or p or Q or P* resolves the entanglement and renders *both* systems known. A second measurement on the same system modifies then a forecast on *it*, but says nothing about the other. That is, both equations can not be verified in one experiment. But one can endless repeat it *ab ovo;* duplicating the entanglement; and then as one wishes, test one or the other equation, to find it confirmed. We consider this herein to have been done.

If after many runs one decides to measure q on the first system and P on the second, and gets:

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

may one doubt that

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

would be a correct prediction for the first system; or

$$Q = 4, \quad P = 7,$$

correct for the second system? Quantum predictions are never fully testable in a single experiment, but correct nevertheless because whoever has them in hand is in no danger of later disappointment no matter which half he does in fact test.

This is beyond doubt: Each and every measurement on a system is the first measurement on it. There can be no direct influences from other measurements on separated systems, a phenomenon which it existed would be pure magic. Neither can it be just random coincidence as thousands of measurements on virginal systems do coincide.

The forecast catalogue $q = 4$ and $p = -7$ would be, naturally, maximal.

13. Example-II: all possible measurements are entangled

Actually a *forecast* with this degree of comprehensiveness, according to QM principles examined herein to their last consequence, is not even possible. Many of my friends are thereby comforted and assert that what a system would have told an experimenter *if* $\cdots$ —has nothing to do with real measurements and is not derived from our epistemological standpoint.

Let us again be completely clear. Focusing on the system with variables p, q (let us call it the ‘small’ one), I can pose via direct measurements, *one* of two questions, either what is q or p. Before I do so, if I like I could have measured a fully separate system (to be considered an ancilla) to answer either of these questions, or I might just intend to do so later. My small system, like an examined student, *can know neither* whether I have done so *nor* which I have done; or intend to do later. From many previous runs I know, that, as it were, this student always gets the first question right. That implies that he must know the answer to *both*. That the very process of questing so upsets the student that all subsequent questions he doesn’t get right, changes nothing on this conclusion. No responsible school principle would conclude otherwise, no matter how much he would wonder what the cause of this strange behavior is. In any case, it would not occur to him that because the examiner checked his notebook that the student answered correctly, or even that the notebook was altered after-the-fact so as to conform to the given answer.

Thus, the small system keeps handy an answer for whichever question comes first. This answer-in-reserve cannot be ‘fixed’ by measuring Q on the ancillary system (in the analogy: that the teacher checks one of the questions in his notebook and thereby the *page* on which the answer to the other question is found, is render illegible). The QM practitioner asserts that after a Q measurement on the ancilla, my small system gets a ψ-function in which "q is fully sharp, but p is completely diffuse." But still no 'fix' changed my small system so that for it the p question holds a precise answer ready, and that it is just that one found earlier.

The situation is really worse. The student has an answer not only for the q- and p-question, but also for a thousand others although I have no idea how he does it. p and q are not the only variables that can be measured; any combination, e.g.,

$$p^2 + q^2$$

corresponds, according to QM principles, to a particular measurement also. It turns out[5] that for this combination also there exists a measurement on the ancilla to be made to get the "small" answer, namely on $P^2 - Q^2$, and here again the answers is identical. According to general QM rules the result must be one of

$$h, \quad 3h, \quad 5h, \quad 7h, \quad 9h, \quad \cdots.$$

The answer for the $(p^2 + q^2)$-question (when this is the first measurement to which it is subject) that the small system has ready must be from this series. Likewise for the measurement of

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

where a is an arbitrary positive constant. In this case, per QM, the result must be one of

$$ah, \quad 3ah, \quad 5ah, \quad 7ah, \quad \cdots.$$

For each value of a there is a new question, and for each the small system has an answer from the above series ready (with the appropriate value of a).

Now, the astounding fact is: these answers can be inconsistent with each other! As, let q' be the answer, which for the q-question, p' the answer, which for the p-question is held ready, then it is not possible that

$$\frac{p'^2 + a^2 q'^2}{ah} = \text{an odd whole number},$$

holds for any values q' and p' and *for any positive number* a'. This is not just a machination with dreamed up numbers having only formal meaning. Two of the measured results one can get, e.g., q' and p', the one by direct, the other indirect measurement. And then one can convince oneself (see below), that the above expression with q' and p' and the arbitrary a, is not an odd number.

The deficit of insight into the interrelationships among the already held answers (using the student's memory technique) is quite extensive; the gaps do not arise from a new QM algebra. The deficit is even more strange in that one can prove: the entanglement is already uniquely specified by the requirements by $q = Q$ and $p = -P$. If we know that the coordinates are equal and the momentum opposed, then there exists a *particular and unambiguous* quantum attribution for *all possible* measurements. For *any* measurement on the small system, one can get a corresponding one on the "large" system, and each such latter measurement orients simultaneously on results that a particular measurement on the small yielded. (Naturally in the same sense as above: only a virginal measurement on each system is valid.) As soon as we have brought the two systems together, such that (briefly said) then their position and momentum take on a correspondence and do all other variables also.

[5] E. SCHRÖDINGER, *Proc. Camb. Phil. Soc.* (in press).

But just how the values among themselves of all these variable interrelate, we know nothing, although the system for each must have an answer ready, as it can be determined by direct measurements on the ancilla.

Should one think, that because we know nothing about the relationships among the variables of *one* system, that no such relationships exist, or that even arbitrary combinations can arise? That would mean that such a system with *one* degree of freedom uses not just two variables, as does classical mechanics, for its specification, but many more, perhaps infinity many. But then it would be remarkable, that *two* systems agree always over *all* variables, if they agree on any two. One might think that this is so because of our lack of finesse, that we are unable to bring two systems together as a single one with agreement regarding two variables without *nolens volens* getting agreement for the other variables, although it would not in itself be necessary. These two assumptions must be made in order not to experience embarrassment for the deficit on insight into the interrelationships among the variables within a system.

14. Time variation of entanglement; status of time

It is perhaps not idle to recall that all in §12 and §13 pertain to a single instant. But entanglement is not time constant. It persists uniquely as an entanglement of *all* variables, but its attribution among them changes. That is to say, at a later time t one can surely learn by measurement on the ancilla the momentary value of q or p, but the measurements to do so on the ancilla are *different.* What they should be is easily seen in simple cases. It depends naturally on whatever forces are in play between the systems. Let us assume that there are no such forces. The masses, for simplicity's sake, are set equal and denoted m. Thus in a classical model the momenta p and P remain constant, as they are just mass times velocity; and, the coordinates at time t (distinguished with subscript "t") (q_t, Q_t) can be found from the initial ones by

$$q_t = q + pt/m,$$
$$Q_t = Q + Pt/m.$$

Considering first the small system, the most natural way to describe classically it at time t is by giving coordinates and momenta *at this time* by q_t and p. But, there are also alternatives. Instead of q_t one, in analogy to personal data, can give q corresponding to age (48 in my case), or q_t corresponding to birth date (1887 in my case). Now, from above one gets:

$$q = q_t - \frac{p}{m}t;$$

and likewise for the large system. Thus, for specifications we may take

$$\text{for small system} \quad q_t - pt/m \quad \text{and} \quad p$$
$$\text{for large system} \quad Q_t - Pt/m \quad \text{and} \quad P.$$

The advantage is that between these, *entanglement remains invariant*, namely:

$$q_t - pt/m = Q_t - Pt/m,$$
$$p = -P;$$

or

$$q_t = Q_t - 2tP/m,$$
$$p = -P.$$

What changes with time then is that the coordinates for the small system can not be obtained as a coordinate measurement on the large system, but rather by a measurement of the aggregate

$$Q_t - \frac{2t}{m}P.$$

One should not imagine that one measures Q_t and P separately; that does not work. Rather one must, as is always the case in QM, recall that there is a direct measurement for the aggregate. Otherwise, all that was said in §12 and §13 still holds, for each moment there is the one unique entanglement of *all* variables with their usual consequences.

It is precisely the same when forces between the systems are in play, but then q and p are entangled with variables more complicated that just sums of Q_t and P.

This by way of preparation for the following. Time variation of entanglement is cause for pause. Must all considered measurements transpire instantaneously to procure their inevitable consequences? Can the exotic conclusions be precluded by calling on the finitude of the duration of measurement? Not at all. One needs for each single experiment only *one* measurement; only the virginal one is admissible, others are simply beside the point. Measurement duration can be ignored because no follow-on measurement is intended. The two virginal measurements must be so executable that they give values for the same preselected *instant*, known in advance because we must measure a pair known to be entangled at the chosen instant.

-Is it always possible to do so?

=Maybe not. I suspect not in fact. Still, *at present* QM must require it; QM is so formulated that its forecasts are made for a particular *instant.* As these forecasts concern numerical results, they would be pointless were the measurements not doable at a particular instant, regardless of how long or short the procedure itself takes.

Just when the experimenter finally *learns* concretely what the result is, makes no difference, however. Theoretically it is as immaterial as the fact that it takes a month to integrate the equations predicting the weather for a single day. The farfetched example of the examination actually, while relevant in spirit, in fact is not faultlessly germane. The expression "the system *knows*" may not mean that the result derived from an instantaneous event, but actually came from a succession spread over an extended interval. But this need not concern us so long as the system somehow revealed the result without external intervention, except that (via experimental procedures) it is told *which* question is to be answered and when the result can be attributed to a particular instant, which per QM, for better or worse must be assumed or the forecast rendered empty.

Here in this discussion we stumble on a new possibility, namely, if it makes sense that a quantum forecast relates never or seldom to a sharp instant, then it need not be required of the numerical results either. This, as the entangled variables exchange places in the course of time, would substantially impede emergence of an antinomy.

That a temporally sharp forecast is a mistake, is probable for other reasons also. Any numerical time reading is also an observational result. May one admit exceptional status for a clock reading? Should it not, as with all others, relate to a variable, having in general no sharp value and, in any case, simultaneously can not have one with *every* other variable? If forecasts of the value for some other variable at a particular instant, need one not fear that neither can be known sharply? Within QM, this issue cannot be investigated, because time is considered *a priori* as precisely known, although it must be kept in mind that any sort of clock-reading disturbs the clock itself to some uncontrollable degree.

I emphasize, we do not now have a formulation of QM from which forecasts *do not* pertain to a particular instant. It seems to me that this deficit is made manifest by the antinomy noted above. By which I do not want to say, however, that this is the only manifested deficit.

15. A law of nature or just an algorithm

That "sharp time" is an inconsistency within QM, and independently that the special status of time encumbers the application of the principle of relativity to QM, are points I have emphasized repeatedly in recent times, regrettably without being able to suggest the shadow of a remedy.[6] The overview of the whole situation, as attempted herein, leads to a remark of another sort pertaining to the voraciously sought, but still not truly achieved, "relativization" of QM.

The contrived theory of measurement, the apparent jumps of a ψ-function and finally the "antinomy of entanglement" all originate by cause of the simplicity of the means by which the quantum calculus allows two separate systems to meld into a unity, for which it seems artificially tailored. When two systems enter into interaction, it is not their ψ-functions that enter into interaction, rather these functions immediately cease to exist and a new one for the combined system reified. It consists, briefly put, to begin simply as the *product* of the two individual functions; for which one depends on quite different variations than does the other, and it is a function of all these variations "in a region of much higher dimensionality" than for the individual functions. As soon as the systems start to influence each other, the total function ceases to be a product; and later when the subsystems separate it does not split up into the factors that can be attributed to the individual subsystems. Thus, as precursor (until the entanglement is resolved by observation) one has only a *common* description of the two in the higher dimensional zone. That is the reason knowledge of the individual systems is reduced to a minimum, even totally destroyed, while the common knowledge remains maximal. The best possible knowledge of the combined system does not include the best possible knowledge of the parts—and that is the whole mystery.

Whoever reflects on it, must thoughtfully cogitate over the following fact. The conceptual melding of two or more systems into *one* encounters great difficulties as soon as one seeks to introduce Special Relativity in QM. The single electron problem was solved seven years ago by P. A. M. DIRAC[7] in an astoundingly simple manner. A series of experimental verifications ornamented with the technical jargon "electron spin, pair production, positron," etc., leave no room for doubt in the rectitude of his solution. But still it steps smartly away from the paradigmatic scheme of QM (which I tried to describe herein)[8]; moreover, one encounters sever problems as soon as an attempt is made, following the example of classical theory, to develop a relativistic many-body electron theory. (That a solution to this problem must be outside the conventional scheme, is revealed by the fact that this problem exists already for the combination of the simplest sort of subsystems.) In this effort I presume to make no evaluation of attempts in this direction that have been made[9]. That they have had the last word I doubt, however, because even they themselves do not make such a claim.

The situation with respect to the electromagnetic field is just as problematic. Its laws are indeed "the very embodiment of relativity;" a nonrelativistic treatment is simply impossible

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[6] E. SCHRÖDINGER, *Berl. Ber.* (16 Ap 1931); *Ann. de l'Institue* H. POINCARÉ, p. 269 (1931); 'Cursos de la universidad international de verano en Santander **I**' (Signo, Madrid, 1935) p. 60.

[7] P. A. M. DIRAC, *Proc. Roy. Soc. Lond. A* **117**, 610 (1928).

[8] P. M. A. DIRAC, 'The principles of quantum mechanics,' (Clarendon Press, Oxford, 1930) p. 239; and 2nd Ed. (1935), p. 252.

[9] Some of the more important citations: G. BREIT, *Phys. Rev.* **34**, 553 (1929) and **37**, 616 (1932); C. MØLLER, *Z. Phys.* **70**, 786 (1931); P. M. A. DIRAC, *Proc. Roy. Soc. Lond. A* **136**, 453 (1932) and *Proc. Cambridge Phil. Soc.*, **30**, 150 (1934); R. PEIERLS, *Proc. Roy. Soc. Lond.*, **146**, 420 (1934) ; W. HEISENBERG, *Z. Phys.* **90**, 209 (1934).

in spite of the historical fact that this field, in its classical avatar as heat radiation, provided the impetus for QM, i.e., it was the first system to be "quantized." This was achieved with simple means because photons, as "light atoms," do not interact with each other[10], rather only with the intermediary of electric charge. Thus, we still do not have a really faultless quantum theory of the electromagnetic field[11] One get quite far with the *construction of a composite system* (DIRAC's theory of light[12]*)*, but not to the goal.

Perhaps the elementary methods offered by the nonrelativistic quantum theory are no more than convenient algorithms, which having deep influence, have strongly prejudiced our whole approach to nature.

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[10]This may be only approximately true. See: M. BORN and I. INFELD, *Proc. Roy. Soc. A*, **144**, 425 and **147**, *ibid.,* 522 (1934); *ibid.,* **150**, 141 (1935). This is the most recent attempt at formulating Quantum Electrodynamics.

[11]Some further important citations, some of which belong to the last but one footnote. P. JORDAN and W. PAULI, *Z. Phys.* **47**, 151 (1928); W. HEISENBERG and W. PAULI, *Z. Phys.* **56**, 1 (1929), *ivid.* **59**, 168 (1930); P. A. M. DIRAC, V. A. FOCK and B. PODOLSKY, *Phys. Z. der Sov. Uni.* **6**, 468 (1932); N. BOHR and L. ROSENFELD, *Danske Videnskaberne Selskab. math-phys. Mitt.* **12**, 87 (1933).

[12]An excellent citation: E. FERMI, *Rev. Mod. Phys.* **4**, 87 (1932).