IL NUOVO CIMENTO VOL. I, N. 1 1° Gennaio 1955

The Philosophy of Experiment.

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(ricevuto il 2 Luglio 1954)

Summary. — The accepted outlook in quantum mechanics (q.m.) is based entirely on its theory of measurement. Quantitative results of observations are regarded as the only accessible reality, our only aim is to predict them as well as possible from other observations already made on the same physical system. This pattern is patently taken over from the positional astronomer, after whose grand analytical tool (analytical mechanics) q.m. itself has been modelled. But the laboratory experiment hardly ever follows the astronomical pattern. The astronomer can do nothing but observe his objects, while the physicist can interfere with his in many ways, and does so elaborately. In astronomy the time-order of *states* is not only of paramount practical interest (e.g. for navigation), but it was and is the only method of discovering the *law*, known by now in its general features (NEWTON). The physicist is nearly always still out for discovering the *law* (technically speaking; a Hamiltonian); this he rarely, if ever, attempts by following a single system in the time-succession of its states, which in themselves are of no interest. The accepted foundation of q.m. claims to be intimately linked with experimental science. But actually it is based on a scheme of measurement which, because it is entirely antiquated, is hardly fit to describe any relevant experiment that is actually carried out, but a host of such as are for ever confined to the imagination of their inventors.

1. – The Accepted Scheme.

In quantum theory (as used at present in thinking about experimental investigations) the following conceptions prevail: some physical *system* with which we are concerned, not necessarily isolated but possessing an individuality and more or less clearly demarcated from other parts of the physical world; the *nature* of this system and of its interaction with the surrounding which includes the experimenter and his measuring appliances; the *state* in which the system finds itself (some prefer to say: is found) at a given moment; *measurements* performed on the system.

The objective of physical science according to the most reserved and cautious group is to foretell what might be called the « orbit » of the state of the system, its development in time. The means both for making the prophecy and for checking it are measurements. Hence it amounts to forecasting the results of later measurements from those previously performed. Though the forecast is usually not precise but of probability, there is an unambiguous representative of the *state*, the state-vector or state-function, which is supposed to change between measurements in a precisely known fashion (if the nature of the system is known) and to determine precisely the probability forecast for any measurement at any given moment.

It is to be noted here that the terms « prophecy », « forecast », « previously », « later » must be understood to include in the limit the case of time difference zero between the two measurements, the one from which and the one for which the forecast is made. This limiting case is not trivial and not at all simple, since many different pairs of measurement can be performed on the same system in immediate succession, and not even then does the result of the first as a rule permit a unique forecast on that of the second, but only of probability.

The *nature* of the system is described by first indicating the variables on which its state-function depends and then the so-called Hamiltonian operator, which determines the partial differential equation according to which the state-function changes while undisturbed by the observer. The nature of the interaction between the system and the observer's appliances is described by a particular operator, said to be associated with any particular measuring device; it is required for making the forecast.

Except in the limiting case of time difference zero, mentioned just before, the nature of the system must be completely known, if the result of a measurement is to serve for pronouncing on the probable results of a later measurement, that is to say if it is to serve any purpose at all. For unless the Hamiltonian is known one does not know how the state of the system has changed in the meantime. There may, of course, be « constants of the motion » i.e. measuring devices for which the prediction does not change with time. They are those whose associated operator *commutes* with the Hamiltonian. But to tell whether it does the Hamiltonian must be known.

In the limiting case this knowledge is irrelevant. But this limiting case applies only to a handful of basic kinematic concepts, mostly such as played already a prominent part in dynamics ever since it exists, long before the advent of quantum theory. The prediction is in these cases based on the mutual commutation relations between the associated operators. A well known example is the cartesian coordinates of the mass centre and the components of its velocity. A precise determination of one of the latter makes any value of the corresponding coordinate equally probable. More generally, the same holds for any observable parameter and its canonically conjugate; e.g. for a so-called angle variable and its corresponding action variable (in a conditionally periodic system). A different instance of considerable interest is the total angular momentum and its three cartesian components. If the former and one of the latter be determined with precision (which can be done, because their associated operators commute), then the absolute value (but not the direction) of the component orthogonal to the one that has been measured can be indicated with precision, while for the component in any particular direction in that orthogonal plane the exact probability distribution can be computed from the mutual commutation relations between the associated operators. It is not necessary to know the analytical expressions of the latter in terms of the variables on which the state function of the system depends, indeed it is not necessary to know anything about these variables, nor any details about the nature of the system. But these are exceptional cases, and positively restricted to the limit « time difference zero » (physicists dub them quantum kinematics as against quantum dynamics). In this paper we shall be concerned with the general case.

2. – The Accepted Scheme Claims Philosophical Purity.

The point I wish to make is this. The method of forecasting that I have outlined above (without the analytical details, which the physicist knows well enough, while they would bother the non-mathematician) forms the mainstay of the accepted quantum theory; it may or may not be appropriate where it applies. But whether or no it is so, we ought to consider if its claim is justified to be an accomplished theory of measurement that applies, in principle, to all cases. I hope to show by a brief anaylsis that it is very far from doing so. To say the least, the vast majority of measurements actually performed in the laboratory have an entirely different character and simply do not fall under the adopted scheme. The question whether there are any that do is comparatively of minor importance and may be touched upon later. For if only my first contention is true, it characterizes the scheme as a mere ingenious thinking-device, a scheme of the writing desk. This in itself is no degradation. Indeed the ingredients from which the great theories of the XIXth and XXth centuries were formed (Maxwell's, Gibb's, Boltzmann's Lorentz', Planck's, Einstein's) were all of this kind — pictures in the mind from which only after elaborate theoretical reasoning results, testable by experiment, can be deduced. But the present case is different. Quantum mechanics claims that it deals ultimately and directly with nothing but actual observations, since they are the only real thing, the only source of information, which is only about *them.* The theory of measurement is carefully phrased so as to make it epistemologically unassailable. There is no question ever of what *is* or *is not* at a given instant, only of what we should *find* if we made this or that measurement; and the theory is only about the functional connexion between some group of such findings and some other group. But what is all this epistemological fuss for, if we have not to do with actual, real findings « in the flesh », only with imagined findings? And worse still, is not the whole epistemology of the scheme exploded, if there are any measurements at all, valuable sources of information, that do not fall under the scheme?

3. - The Laboratory Pattern is Different.

In the physical laboratory (as against the astronomical observatory) we are not very often interested in the future history of the body or system on which we have made a measurement. In the vast category of measurements concerned with some constant of the material (as density, compressibility, Young's modulus, specific heat, electric or thermic conductivity, surface tension, viscosity, etc.) the physical object is just a sample that may afterwards be thrown into the dustbin. The results are used on a hundred later occasions, but not usually for predicting the future behaviour of the sample. When a motion or, more generally, a change with time becomes relevant, it is more often that of a measuring instrument (the needle of a galvanometer or electrometer, the cathode ray pencil of an oscillograph) than that of the object under examination. These remarks refer not only to old fashioned routine, but also to provinces very relevant to quantum theory: blackbody-radiation, spectrometry, mass-spectrometry, nuclear magnetism, etc..

It behoves me to mention examples to the contrary: the direct determination of radioactive decay, or the observation of slow chemical reaction rates, when samples are taken and analysed from hour to hour or from day to day. The closest similarity to the scheme of quantum mechanics, to my mind, obtains in synthetic chemical manufacture of drugs. Here we actually perform some carefully prescribed preparatory operations, including a host of measurements, with the exclusive scope of producing a substance whose chemical properties we can foretell. This is a wide and important, still a very special branch of physical science. Ought one perhaps to put the manufacture of a scientific instrument on the same level? By a certain handling of raw material we produce a system — the instrument — with very special, closely predictable properties. I will not decide this at the moment and beg to regard it as a side remark.

How is it now that there are, at any rate, hosts of actual measuring devices which are continually applied and seem to fit so badly into the quantum theory of measurement? Is that really so, or could they be looked upon at an other angle and would then fit into the scheme? No. This is really so and it is not difficult to tell the reason, and even to phrase it according to quantum mechanics' own concepts and terminology.

4. – Astronomy - The Prototype of Physical Theory.

Both forms of quantum mechanics (the matrix- and the wave-form) originated from *analytical mechanics* (a.m.). They both leaned against the great central theorems, due to Hamilton and Jacobi, of this most accomplished and highly architectural theory in physics. Let us note, by the way, that though both groups of discoverers used this architecture as a guide for initiating a new science, they did this in so entirely different ways that it was a great surprise to find them willy-nilly running into the same mathematical construct. The earlier form (HEISENBERG, BORN) led very directly to, nay it consisted in, adopting an axiom (now usually called a theorem) of dangerously fascinating beauty: the equations of motion must be taken over from a.m. *au pied de la lettre*, but the variables whose change in time they control and whose numerical values at any moment of time would in a.m. indicate the instantaneous state of the system must now be looked upon as something entirely different. They are not ordinary numbers; the product of any two of them depends in general on the order of the factors; their « commutation relations » are of outstanding importance. They are momentously *contributory* to our knowledge of the state of the system; however alone by themselves, even when completely known, they tell us absolutely nothing about the *state* (not even by probability), but only about the *nature* of the system, about *possibilities* (see Section 1 for the distinction between state and nature). That is why I called this axiom-theorem dangerously fascinating. Its apparent simplicity — the same equations of motion between quantities, habitually given the same names and represented by the same symbols — seduces us to underrate the change that has taken place. And that the more, because the analogy with a.m. goes even further. In the early stages of matrix mechanics the state function (alluded to in Section 1) was missing; it was supplied by wave-mechanics. Now, if it be given for any time, e.g. for $t=0$, then those non-commuting quantities controlled by the equations of motion do give us full information about the state at any other time. Thus the knowledge of the state-function for one moment is apparently the analogue of the initial conditions (or integration constants) in a.m.. Moreover, just as in a.m., interesting information of a general kind can be obtained from the equations of motion alone: e.g. when they assert than the non-commuting representative of a quantity does not change with time, this tells us that any information we might have or obtain about this quantity (whether precise or of probability) will not change with time. (But patently this general information concerning constancy is about the nature, not the state.)

I must apologize for going into these details perhaps more than necessary, thereby deterring non-mathematical readers. I return to the main argument. A.m. has descended from celestial mechanics, initiated by NEWTON. The marvellous precision with which the motions of the heavenly bodies are predicted from Newton's laws — a precision unparalleled in any other branch of knowledge up to the present day — has made mechanics the prototype of exact physical science. Newton's pattern was closely followed in all the attempts of constructing models of the material world in order to account for its behaviour. It was followed not only as long as the hope or tendency prevailed to explain everything mechanically, but far beyond. For it does not really matter in principle (though the mathematical methods vary considerably), whether I give myself the initial positions and velocities of a number of particles that attract or repel each other by forces, known or assumed known, and ask myself what aspect will they offer at a given later time, — or whether my system includes field variables, distributed continuously throughout space and governed by laws that relate them to each other and to the motion of the particles. The close proximity to the Newtonian pattern consists in the peremptory demand that the said laws should, from a given initial state of particles and field, entail a definite state of the same at any later time, a definite orbit, as it were, of the whole system (notwithstanding the utter impossibility of actually checking the infinitely many data implied by even one such state).

5. - But Not of Physical Experimenting.

So this ideal of exactitude in physical science was inherited from astronomy: for any theory we think out the touchstone shall be that it enables us to predict the observable features of a physical system at any later time from sufficiently accurate observations made on the same system at an earlier time. This seems to be a sound basis for *thinking* about physical events, and I dare say the only sound one that has been conceived till now. If in our time it has been found out that nature is not such as to make accurate prediction possible in all cases, but sometimes only of probability, this is decidely of very great interest, but it does not change the pattern of thought fundamentally, provided the probabilities are predicted with accuracy (as is universally agreed that they are). At any rate this is *not* the point I wish to analyse here.

But the great difference between (positional) astronomy and physical science in general is this. Is astronomy, both before and after its fundamental law had been discovered by NEWTON, the actual observations were and are of precisely the type of the ideal pattern (which, as I said, has been modelled after them). Several positions of a planet are observed — not just one, since its velocity is required, moreover only two angles are observable, while the third space coordinate must be inferred somehow. From these data later positions are computed and compared with observation. In this we assume Newton's law to be known. But even before it was known, the actual observations were of exactly the same kind. Only no very reliable prediction was possible. But it is known how KEPLER's genius succeeded in determining from a vast number of positional observations the actual facts, known as Keepler's laws, from which NEWTON read off, as it were, both the general law of motion and that of gravity, making thenceforward accurate prediction possible.

In physical science, however, as it has developed since, while this same pattern of *thought* has been copied and retained, it is found much too narrow, indeed mostly quite inadequate, to cover the actual observations. They are of entirely different, indeed of extremely multifarious types (as explained before in Section **3**). Not only are we usually not in the post-Newtonian position of knowing the laws and testing them by prediction, but in the position of KEPLER. Our quest is after the *nature* of the system not after its *state*. Moreover for finding out what we want to know we do not follow the method of Kepler. We are not, or hardly ever, faced with a system that moves or changes its state of its own in a way that we would find out by carefully registering its observable features as functions of the time, as the positional astronomer does. I once had the good luck of having to supervise for three years an advanced practical (measuring) course in physics. Except for Atwood's machine (which was rather on an elementary side-track) and, perhaps, observations on a pendulum and the like, I do not remember a single experiment that followed these lines, but many, many along different lines. Now, this was in the early teens of this century; but I do not think the situation has changed since, neither in the courses of practical exercises, nor in the research laboratories.

6. – The Blind Spot in Quantum Mechanics.

Quantum mechanics (q.m.) has been shaped after analytical mechanics (a.m.), which in turn has descended from astronomy. Right at the outset the fascinating and intriguing novel feature presented itself, that the predictions of q.m. must not be regarded as unique but only as of probability. So much keen interest and honest work was spent on elaborating a scheme which fitted the new situation and yet remained close enough to its prototype (a.m.) for availing itself of its benefits, that no time or strength or inclination was left for noticing how far the methods of experimental laboratory research had drawn away from those of astronomy — to which they were never very close. Or was it believed that the new scheme (q.m.), that is a.m. readjusted so as to make only probability forecasts, was now equipped to apply *directly* to actual laboratory measurements (which a.m. never claimed except for simple cases as Atwood's machine or a pendulum)?

Anyhow, the claim *is* made. The new science (q.m.) arrogates the right to bully our whole philosophical outlook. It is pretended that refined measurements which lend themselves to easy discussion by the quantum-mechanical formalism could actually be made. They could not. (I am alluding to the gamma-ray-microscope, to the location of the electron in a « given » hydrogen atom, and the sort). Actual measurements on single individual systems are never discussed in this fundamental way, because the theory is not fit for it. This in itself is no blame. What is objectionable is the philosophical presumption, which claims reality for anything the quantum theorist chooses to imagine as measurable, while he closes his eyes to the fact that few, if any, actual measuring devices are amenable to discussion under his scheme.

One can certainly make a case for the view that the sum total of all observations which have been and ever will be made is after all the only reality, the only thing that physical science is concerned with. This view is not self-evident, but it is worth discussing. However to maintain the same about all observations that some school of theoreticians fancies, while in actual fact such observations are not made and differ in bulk from those that have been made and on which physical science is based, such a view is not founded on reason and cannot pretend to passing for serious philosophy. In using such plain language I hate to give offence to those of my friends who adhere to this kind of view (without realising that it is of this kind). But I wish to make it clear, that I shoulder now and ever after the full responsibility for my refractoriness. I am moving against the stream. But the tide will change.

7. - Our Objective is the General Laws.

At the end of Section **3** I promised to express in q.m.'s own language why most actual laboratory devices do not fit into its scheme of prophecy. When their right place within the accepted theory is pointed out, it becomes perfectly clear that and why they do not fit into the wrong place.

The situation is fairly obvious. The prophecy scheme (in all but a few outstanding exceptional instances, see Section **1**) deals with measurements on systems whose *nature* is known. Experimental research is nearly always concerned with *finding out* the nature of the system under examination. It has its place earlier, by a well marked step, than the prophecy scheme. Its task ranges with that of KEPLER, not with that of the astronomers after NEWTON. (Let it be mentioned, by the way, that they too have questions about the *nature* of their system left to them: the masses of the planets; the inertial frame; the appropriate time variable, since the rotation of the earth is not uniform.) To put it briefly: experimental research is interested in general laws, not in accidental *states*.

So is astronomy. But here it happens that the accidental state of the planetary system is of paramount practical importance for geography and for navigation. And, secondly, it so happens that a painstaking record of the time sequence of *states* is the only appropriate means for answering questions of *nature*, whether pre-Newtonian, as in the work of KEPLER, or post-Newtonian, as e.g. in ascertaining the tidal retardation of the rotating earth. The reason for this being so is that the astronomer has no means of interfering with his system: he can do nothing but observe it.

It might, of course, be the case, that in experimental physics the method for establishing general laws were the same as in astronomy. If this were so, the quantum mechanical theory of measurement might be all right. But it is not so. And that is small wonder. The physicist has full liberty to interfere with his object and to set the conditions of experiment at will. This empowers him to invent methods widely different from, and largely superior to, the placid observation of the astronomer. It is not astonishing that the strictly astronomical scheme of quantum mechanical prophecy is too narrow to embrace them.

In quantum mechanical language I would say, that the physicist's experiment is usually not aimed at finding out the state-function of his physical object, but at discovering characteristic features of its Hamiltonian (very often: its eigenvalues). For the Hamiltonian is the representative of the nature of the system, of the general laws that govern it in any state. Now I must repeat myself. It is perfectly thinkable that a good way of finding out about the Hamiltonian were the inversion of the prophecy scheme: you measure initial and final values many times and ask what Hamiltonian will correlate them correctly. If this were so (as it is in astronomy), the quantum mechanical theory of measurement might be all right. But it is not so. The fact that from a known Hamiltonian the prediction is only of probability makes the inverse problem exceedingly involved, as everybody who has an insight into the mathematics of the subject will admit. It is small wonder that the experimenter hardly ever follows this course. The most interesting questions are those about the discrete eigenvalues of some physical variable (mostly: the energy) or about some other matrix-elements of some such quantity (mostly: perturbation energy). These questions are sometimes answered by producing suitable experimental conditions repeatedly, never by following an individual system through a long course of its orbit, because this is not possible. Repeated short period observations on similar systems are then put together and taken to form virtually the potential history of one and the same system.

The latter remark refers mainly to the tracing of the orbits of individual particles and of the events produced by them (as nuclear disintegrations) in the cloud chamber and in photographic emulsions. In these experiments we are in a similar position to that of the astronomer with regard to our not being able to influence the event. Yet the situation is not quite as bad, for we prescribe the medium in which the events take place (the nature and pressure of the gas or the composition of the photographic emulsion), and we can apply a magnetic field of known strength, which gives valuable information by curving the paths.

8. – Conclusion.

There is a habit in some quarters to answer objections of the kind raised here by saying that they are a matter of philosophical taste and not relevant to any question physics is really concerned with. This attitude is an instance of the fact that scientists are inclined to take their own outlook for the natural way of looking at things, while the outlooks of others, in asmuch as they differ from theirs, are adulterated by preconceived and unwarranted philosophical tenets, which unprejudiced science must avoid.

The ingegnious new-comer to quantum mechanics asks many inconvenient questions from which, in the considered opinion of the adepts, he must be weaned. He asks for instance whether the state-transitions in the atom that accompany the emission of a light-quant are instantaneous or whether they take time and pass through intermediate states. He is told that this question is meaningless and cannot be answered. Meaning is only attached to the value we find for the energy if we measure it, this can (by axiom) only be either the value of the initial state or that of the final state, the probability of finding the latter rather than the former increases with time continuously in a way that the theory foretells.

Another example: our bright disciple may find out for himself, that according to his theoretical instructions nothing prevents the velocity of a particle being measured by the time-honoured method which is practiced on the race-course and by the police (to trace offenders against the speed-limits), viz. by recording the time taken by the particle to cover a known distance; and he is perturbed in noticing that nothing is in the way of carrying the accuracy of this measurement far beyond the limit imposed by the Uncertainty Principle. The answer he gets from the initiates is, that this is indeed so, but causes no worry, since the conflicting data refer to a bygone moment and cannot be used for predicting the future.

These examples could be multiplied. The answers are intriguing; they appear to be unassailable, for they seem to rest on the simple and safe principle that sound and sober reality, for the purposes of science, coincides with what is (or might be) observed. But actually this is not the whole story. We are also supposed to admit that the extent of what is, or might be, observed coincides exactly with what quantum mechanics is pleased to call observable. I have endeavoured to adumbrate here that it does not. And my point is that this is not an irrelevant issue of philosophical taste; it will compel us to recast the conceptual schema of quantum mechanics.

RIASSUNTO

L'interpretazione più comune della meccanica quantistica (m.q.) è interamente fondata sulla sua teoria della misura. Come sola realtà accessibile si considerano i risultati quantitativi delle osservazioni, il nostro unico fine essendo quello di predirli per quanto possibile a partire da altre osservazioni già fatte sullo stesso sistema fisico. Questo schema è interamente suggerito dalla astronomia di posizione, sul cui grande strumento analitico (la meccanica analitica) è stata modellata la stessa m.q.. Ma le esperienze di laboratorio ben di rado seguono lo schema dell'astronomia. L'astronomo non può che osservare i suoi oggetti, mentre il fisico può influenzare i propri in molte maniere, e anzi lo fa in modo elaborato. In astronomia la sequenza temporale degli *stati* è non solo di enorme interesse pratico (per esempio per la navigazione), ma è stata ed è il solo modo di scoprire la *legge*, che si è finito per conoscere nei suoi aspetti generali (NEWTON). Il fisico ancora oggi si propone di scoprire la legge (in terminologia tecnica: una Hamiltoniana); ma raramente, o mai, egli cerca di raggiungere lo scopo seguendo la successione temporale degli stati di un singolo sistema, che non sono di per sè di interesse fisico. L'interpretazione più comune della m.q. si vanta di essere intimamente legata alla scienza sperimentale. Ma in realtà è basata su uno schema di misura che, essendo interamente antiquato, è ben poco adatto a descrivere qualunque esperienza che venga realmente eseguita, ma piuttosto una schiera di esperienze per sempre limitate alla immaginazione dei loro inventori.