QUANTUM MECHANICS AND "OBJECTUATION"

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Abstract

As long as quantum mechanics is viewed as a merely predictive formalism it raises no conceptual problem, but well-known difficulties appear when we try to express it in an objectivist language, that is, when we try to make it interpretable as a descriptive theory. One of the best known recent proposals in this direction is the Gell-Mann and Hartle Cosmological Quantum Theory, also known as the Consistent Histories Theory. But reasons will be given showing that it also can hardly be expressed in an objectivist language. When all is said and done it therefore seems appropriate to interpret facts as phenomena and physics as an operationalistic tool. However, reasons will be given for still going on considering meaningful the notion of an underlying man-independent reality conceivably reflected to a limited extent in physical laws.

Yesterday two talks at least, the one by Prof. Mittelstaedt and the one by Prof. Aspect reminded us of the enormous magnitude of the changes our familiar conceptualization of the world must undergo due to the advent of quantum mechanics. And both ended up by suggesting or plainly raising questions. No individual exists, Peter Mittelstaedt told us : then, what about we? And as for Alain Aspect he drew up a list of possible ways out but wisely stressed their most conjectural nature.

Such questions are really crucial. In his basic, reference book, *The Investigation of the Physical World* [1], Giuliano Toraldo di Francia introduced the word "objectuation". He defined it as "the activity that consists of dividing reality into objects". Objectuation - he wrote - is a "stage through which we must necessarily pass if we want to investigate, or even just think, reality". As he pointed out: "This is the role of objectuation in classical physics. To enunciate the laws of mechanics, we must consider material objects or bodies" etc..

The use, here, of the word "activity" reminds us that Toraldo, same as all contemporary empiricists, is careful not to indulge in any implicit ontology concerning the said "material objects": we have to make use of this concept, not because material objects "obviously exist out there", but because we could not investigate, or even think, reality without its help. Another way of expressing the same idea is to say that, to enunciate scientific laws, we must use what I call the "objectivist language". By "objectivist language" I mean a language that is basically descriptive instead of just predictive. Otherwise said, a language in which either the objects physics deals with (in a broad sense: particles, fields, etc.) are assumed to exist per se, or we can do as if they so existed, quite independently of us. For a long time - and whatever specific option was taken in philosophy of knowledge - the objectivist language was considered as being, by definition so to speak, the only objective one, that is, the one in which physics had to be couched. Even the Kantians held this view.

Now, to be expressible in the objectivist language any theory must at least yield a possibility of specifying what, within its realm, can be treated as if it were real. And what Einstein demanded and could not get from quantum mechanics was just precisely this. Contrary to what is commonly said, Einstein was not craving for a come-back to old classical concepts. He just asked his colleagues: "in quantum mechanics what algorithm can be considered as corresponding to something real?". And here, in this audience, we all very well know that this is a difficult question! Regarding unaltered quantum mechanics - Schrödinger equation without additional ingredients - we are all aware that, for various reasons, neither the wave function nor the density matrix nor the Heisenberg operators seem able to play such a role: the role of representing what is real.

At this stage, therefore, we are somewhat at a loss how to express quantum mechanics within an objectivist language. However, a remark is here in order. Up to this point I was respectful, as you noticed, of the great guiding lines of modern empiricism: On scanning for theoretical entities that could be viewed as if they corresponded to reality, I only mentioned entities - wave functions, density matrices, Heisenberg operators - that are set by the theory in direct correspondence with observational data. Now, it is a priori conceivable that for that matter the great - but somewhat imprecise - guiding lines in question be too strict. Perhaps we should look for other entities, not directly connected with experimental possibilities but respectable in their own right. There, Toraldo's book again gives us a hint. It draws our attention on a historical fact of great importance, namely the discovery, in the XIXth century, of what he calls "nomological objects", that is, objects, such as atoms, molecules etc. having fixed characteristics. These nomological objects play a prominent role in classical physics. In quantum physics proper only their names appear. But a priori it is possible to conceive of a subquantum world the elements of which would be unobservable - hidden - such nomological objects. These, then, would be the objects of which we could speak as if they were real…. At the price of taking some liberty with "the great guiding lines of empiricism" the possibility of using the objectivist language also in quantum mechanics would be salvaged. This is more or less what Einstein had in mind when he suggested that quantum mechanics is incomplete. And during some decades in the middle of this century it could seem that this was indeed the (philosophical) solution to the problem.

But well… it does not work. More precisely it works only under conditions that are so unpalatable that they make the proposed solution quite unconvincing. This negative result follows from the Bell theorem [2]. The reason is that this extremely powerful theorem extends nonseparability to any theory that can be expressed in the objectivist language and yields the same observable - and observed! - predictions as standard quantum mechanics.

Incidentally, this is a proper place to recall to you the indirect but important role Giuliano Toraldo di Francia had in this. In the late sixties I was, I dare say, one of the very, very few who took interest in the conceptual foundations of quantum mechanics and - consequently - in the, just appeared, Bell inequalities. It, alas, then seemed crystal clear to me that no established scientific organization would ever consider taking such problems seriously. Hence I was surprized - and delighted - when I got from the President of the Italian Physical Society an invitation to organize, in the Varenna International Summer School, a session on the foundations of quantum mechanics. As you perhaps guessed, this President was no other than Giuliano Toraldo di Francia. I hastened to invite, among prominent people, John Bell, Wigner of course, and also Abner Shimony who, together with three colleagues, had just published the theoretical paper [3] that was to make experiments in this field possible. And I think it can be claimed that this meeting, which took place in 1970, was what most contributed giving impetus to the experimental work in this domain. Thanks to Giuliano!

Now, can we and should we strive to salvage the objectivist language? These are two separate questions. Let me start with the first one and let me expand it in the form: "can we salvage the objectivist language and at what price?" There is a global answer to this question. It is: "yes we can, but the price is: we must accept nonseparability (or nonlocality which is almost synonymous) and this price is very heavy." This answer of course raises the question of the "how?", and to answer this last one we should turn to the specific models that were put forward. The main ones are well-known. They are: the Louis de Broglie "pilot wave" model [4], greatly generalized by David Bohm [5] and nowadays known as the Bohm model; the Ghirardi, Rimini and Weber model [6]; its variant the CSL model of the same plus Pearle; and the so-called "modal model" of Healey [7], Dieks [8], Bas van Fraassen [9] and a few others. Unquestionably they all work. On the other hand they all are essentially nonrelativistic and - in contradistinction with standard quantum mechanics - there are serious difficulties in making them relativistic; a fact which of course has to do with nonlocality. My own feeling about them is that determining which one - if any - is "right" seems hopeless; which of course does not mean I find them uninteresting. Quite on the contrary I consider that it is by exploring, as their upholders do, all possible vistas, that we can get a well-balanced view of the overall situation concerning this problem of the objectivist language.

Up to this point I mentioned models that work. Let me now comment a little on models that, interesting as they are, don't work as regards the specific program the feasibility of which we are now investigating, that of enunciating physics in terms of the objectivist language.

Those I have in mind are the so-called consistent histories theories, and particularly the Gell-Mann and Hartle theory [10]. To begin with, let me note that, as explained in the original papers where these theories were put forth, the search for them was, in several cases, motivated by an alledged shortcoming of the Copehagen interpretation, consisting in that this interpretation crucially relies on the notion of outside observers. Now, within the realm of a merely predictive approach to science this criticism would obviously be unoperative since within such a standpoint the only legitimate purpose of science is just precisely to provide consistent accounts of what the human observers perceive. It becomes a valid objection only when we adopt the philosophical standpoint that science must be expressed in an objectivist language. This seems to imply that the "consistent historians" are upholders of this standpoint. And indeed their introductions, conclusions etc. are expressed in terms of standard realism. Then however we necessarily fall back on the difficulty we just discussed, namely nonseparability and more specifically the violation of Bell's "local causality" [2].

Gell-Mann and Hartle denied this difficulty. But I must say I do not understand their argument. Bell's theorem is a theorem and if you accept its premisses, namely: use of objectivist language and locality, you must accept its conclusion. Apparently, Gell-Mann and Hartle thought they could wipe off the difficulty just by stating that quantum mechanics describes alternative decohering histories and stressing that one cannot assign reality simultaneously to different alternatives. This is true but it must not be forgotten that realism (and the same holds true concerning the use of the objectivist language) has implications reaching beyond pure actuality. In particular, it implies counterfactuality. What I mean here by "counterfactuality" is very simple: In a case in which there is a table in the next room counterfactuality means that the statement "if I raised my hand [which, as you observe, I do not] there would be a table in the next room" is meaningful… with the corollary that it is certainly true if it is moreover assumed that no influence is propagated from this room to the next room. A "realism" without counterfactuality would hardly have any point in common with standard realism as everyone understands it. However the theories of the authors in question unquestionably violate counterfactuality so that it is difficult to understand that when they write popular books (I am thinking here of Murray Gell-Mann's "The Quark and the Jaguar"[11]) they definitely tend to suggest that full-fledged "standard realism" holds good and that their own theory agrees with it.

A clearcut example of this is where Gell-Mann discusses EPR correlations at a distance on the example of two photons in a state of zero total spin. He states that on one history branch the situation is not different from what it is in the classical case: because - he writes - if the polarization of one photon is measured the polarization of the other photon "is specified" with certainty. Now, "is specified" means "has some value", and by focusing on one history branch Gell-Mann avoided considering the crucial question: "would this polarization of the right-hand-side photon be what it is if the measurement on the left-hand-side photon had not been made (or if some other polarization component had been measured on it instead)?". But this is a perfectly legitimate question (remember the statement about the table in the next room), to which any normal theory and particularly any alledgedly "realist" one should have some answer [12]. To avoid it is not tantamount to answering it. Of course, in the theory in question the proper answer would be "no" (wheras, of course, in a realist, local theory it would be "yes"). More precisely the proper answer would be that when the left-hand-side measurement is actually made, the statement that the polarization of the right-hand-side photon has a definite value is - in Omnès' language - a "merely reliable" one, not a "true" one. And that local causality is granted by the theory only concerning "true" statements. Indeed, in the specific case at hand it seems impossible not to consider that the "reliability" of the statement in question is induced at a distance by the measurement performed on the left-hand-side photon. To sum up, contrary to the apparent claim of its authors the Gell-Mann and Hartle theory is not a realist, local one, which means, in particular, that it violates the implications for partial systems of the objectivist language.

Well, the outcome of all this is that salvaging the use of the objectivist language in physics seems, after all, a very tricky endeavour. So, it is now time that we consider the second of the broad questions I mentioned: *should* we strive to salvage it?

Clearly, an alternative exists. It consists in turning to a purely operationalistic conception of science. Moreover, this alternative is favored by the fact that the basic laws of quantum physics are, actually, but predictive rules. As an analysis of, say, the Stern-Gerlach experiment clearly shows, the Born probability rule cannot be interpreted as yielding the probability that the quantity of interest really takes up such or such value, but merely as yielding the probability that, if we look for this or that value, we get a positive answer.

Now, I am fully aware that there are objections to this purely operationalistic approach. I see them as being of two kinds. First there are objections of an epistemological nature, that were developed by quite a number of philosophers. I think this is not the proper place to engage in a long discussion of them. Let me just say that what I know about them could not convince me that they are absolutely decisive. But then, there is an objection of a more general philosophical nature, which is that we cannot deny that thought exists (either in its own rights or as a mere "product of matter" as some materialists would have it). Even transcendental idealism, which claims that the phenomenological self is no more of an absolute than the phenomena, has to take seriously the concept of an impersonal transcendental self that is prior to everything. The dilemma that radical operationalism sets us in then is: either we believe in a kind of Berkeleyan idealism or irrealism: only we exist, and we create the phenomena and all that exists; or science merely lets us know the impressions we shall have and tells us nothing certain about what "truly exists" (although great basic equations may disclose something about its structure).

At first sight both alternatives seem equally unpalatable. But I think [13] this is but an appearance. I think that the last one is more flexible than the first one - the one I just called "Berkeleyan" - and that it can be accomodated so as to become acceptable and even rather inspiring. I shall not dwell here on its "inspiring" character because finding something "inspiring" is a matter of taste rather than rationality. But I would like to just briefly mention two points. The first one pleads in favor of the operationalistic approach without implying a choice between the two alternatives. It is that, thanks to recent developments [14], we now see in detail that the predictive rules of classical physics do follow from those of quantum physics.

This implies that the operationalistic approach does, after all, have something in it akin to an explaining power.

The second (and last) point is specifically in favor of the second alternative I mentioned: the one consisting in not reducing existence to reference, and in, therefore, taking seriously the notion of an unknowable reality. The point is that, as long as we only consider the quantum mechanical predictive rules, and even if we consider decoherence and so on, we are confronted with the "and-or" problem, also called the problem of unicity: "Why does only one of the the possible measurement outcomes materialize?" Now it is important, I think, to note that this problem does not appear in the "realistic" models that I mentioned a moment ago. For example, it does not appear - or, better to say, it is solved - both in the Bohm model and in the Ghirardi, Rimini, Weber model. So, what I say is: concerning independent reality perhaps one of these models - or some not yet discovered model - is right. We do not know and we shall never know. But the mere possibility that one is right obviously suffices to remove the difficulty.

To conclude, let me point out that the structure of this argument is old. When Epicurus decided to free the Greeks from the terror of the gods he built up a world view in which there were no gods; but then he said (in substance): "my model is just a model. It is quite possible that other models exist, that also involve no ominous gods. I do not know which one is right. But it does not matter. What matters is that we now know we can have a world view free from ominous gods". To those of you who like Epicurus' philosophy I hope this remark will make my argument acceptable…

To sum up: I consider that basic physics is essentially predictive and only metaphorically descriptive. However I do not rule out the idea that basic laws such as the Maxwell equations do dimly reflect something of the great structures of "what exists".

REFERENCES

1 G.Toraldo di Francia, *The Investigation of the Physical World*, Cambridge University Press, (1981).

2 J.S.Bell,Physics 1, 195 (1964); *Speakable and unspeakable in Quantum Mechanics*, Cambridge University Press 1987.

3 J.F.Clauser, M.A.Horne, A.Shimony and R.A.Holt, Phys.Rev.Lett. 23, 880 (1969).

4 L.de Broglie, *La nouvelle dynammique des quanta,* Rapports et discussions du cinquième conseil de physique Solvay, H.A.Lorentz ed. Gauthier-Villars, Paris 1928.

5 D.Bohm, Phys.Rev. 85, 165,180 (1952)

6 G.C.Ghirardi, A.Rimini and T.Weber, Phys.Rev.D 34, 470 (1986).

7 R.Healey, *Philosophy of Quantum Mechanics, An Interactive Interpretation*, Cambridge U.Press 1989.

8 D.Dieks, Synthese 82, 127 (1990).

9 B. van Fraassen, in Current Issues in Quantum Logic, E.Beltramelli and B.van Fraassen eds. Plenum, New York, 1981.

10 M.Gell-Mann and J.B.Hartle, *Quantum Mechanics in the Light of Quantum*

Cosmology, Proceedings of the Santa Fe Institute Workshop on Complexity, Entropy and the Physics of Information, May 1989.
11 M.Gell-Mann, *The Quark and the Jaguar,* Little Brown, USA (1994).
12 B.d'Espagnat, *Influences, Histories and Reality,* Found. Phys.,26, 919 (1996).
13 B.d'Espagnat, *Veiled Reality,* Addison-Wesley Publ. Co. Reading, USA, (1995).
14 R.Omnès, *The Interpretation of Quantum Mechanics,* Princeton University Press (1994)