Reply to Aharonov and Anandan’s “Meaning of the Density Matrix”

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Abstract

Aharonov and Anandan’s claim that the notion of “proper mixture” is improper is shown to be unjustified. The point is made that if a purely operationalist standpoint is taken the three difficulties these authors describe relatively to the conventional interpretation of density matrices in fact vanish. It is noted that nevertheless it is very difficult for us to do without any form of realism, in particular when the quantum measurement problem is considered, and it is stressed that the proper mixture notion comes in precisely at this level. The more general question of the real bearing of Aharonov and Anandan’s ideas on the interpretation of quantum mechanics problem is considered.

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1 Introduction

Aharonov and Anandan recently issued an article entitled *Meaning of the Density Matrix* [1] in the main text of which the ideas of a certain physicist called d’Espagnet are criticized. The first interesting question this paper raises is, of course: “Who is this man?” Admittedly the idea occurred to me that, conceivably, he could be I. But there were tokens of the contrary. The recurrent spelling mistake was admittedly but a minor one. A more serious indication was the following. According to the text, this d’Espagnet had tried to “point to the state of a physical system that is represented by $\rho$ in the sense that measurements on this state would give $\rho$”. Since the authors wrote this, I thought, it must be true. And since personally I never made any such attempt (I do not even understand the authors’ description of it...) Mr. d’Espagnet could not possibly be I. Paradoxically, this (optimistic) impression of mine was enhanced by the fact that, in the text, d’Espagnet is attributed by name a view actually shared, I believe, by, practically, all quantum physicists, namely the idea that, $|\phi_\alpha>$ and $N_\alpha$ being, respectively, the normalized state vector describing an ensemble $E_\alpha$ of physical systems and the number of elements in the latter (all systems being of the same type, and $N = \Sigma_\alpha N_\alpha$) the operator

$$\rho = \Sigma_\alpha |\phi_\alpha| \frac{N_\alpha}{N} < \phi_\alpha|$$

(1)
describes a mixture (incidentally, in the authors’ formula (4), here relabelled (1), the symbol $\Sigma_\alpha$ is missing). The authors refer to some drawbacks they claim this view has and, in fact, I have come to suspect that, at least in some parts of the article, this d’Espagnet is just a convenient symbolic figure representing the large set of the physicists who naively entertain the said view without even noticing its drawbacks. However I well realize that this cannot be the whole story. After all, the name “d’Espagnat” is correctly spelt in both the abstract and the references and... I *did* write the book entitled *Veiled Reality* [2]! So I finally decided that I had to consider the substance of the problem raised. It is the subject matter of this note.

One of my claims concerning it is that, in fact, this problem has much to do - much more than is apparent at first sight - with the difficulty of setting a consistent relationship between two notions, that of *operational definition* (of systems and their
properties) and that of ontological existence. Hence, in Section 2 the point is made that if a purely operationalist standpoint is taken the three difficulties Aharonov and Anandan' Introduction describes in fact vanish. In Section 3 it is noted that nevertheless it is very difficult for us to do without any form of realism, in particular when the quantum measurement problem is considered, but that difficulties then creep in; and it is shown that, in this field at least, contrary to Aharonov and Anandan’s claim, the notion of proper mixtures is justified. In Section 4 the more general question of the bearing of Aharonov and Anandan’s ideas is investigated and in Section 5 some conclusions are drawn.

2 On the virtues of unaltered operationalism

The necessity of only using concepts having a well defined meaning prompts us to systematically use operational definitions but we have a natural trend towards realism and in quantum physics reconciling the two is not always easy. One possible standpoint is to resist this “ontological” trend and take but the operational aspects of quantum physics seriously. It is then considered that quantum mechanics is merely a set of rules correctly predicting what the outcomes of measurements will be. Since some of these rules only yield probabilities we have to consider ensembles. But it must be observed that, to repeat, the standpoint in question removes at one stroke the three drawbacks to the use of ensembles that Aharonov and Anandan mention in their Introduction. This is obvious concerning drawback (1). It is also clear concerning drawback (2) since within this standpoint \( \rho \) is but a predictive tool. That a predictive tool should change abruptly when new information is gathered is quite natural and does not constitute a conceptual problem. And drawback (3) vanishes as well since within the said standpoint introducing the ensemble in question - a Gibbsian one, that is, an abstract concept - only serves to give a meaning to the probabilities of getting such and such measurement results. The formalism simply does not include the rule that the result obtained in one measurement should be relevant for predicting the probabilities of further measurement outcomes. Rather, it stipulates that to this end a new Gibbsian ensemble corresponding to \( \rho \) should be imagined afresh.
3 Charms and Dangers of Realism. Proper mixtures

The above conclusion is clear and, I think, uncontroversial. Wherefrom does it then come that Aharonov and Anandan found the conventional meaning given to density matrices has the drawbacks they state? This question admits of but one answer. It is because just as all “normal” people, they do not strictly cling to “pure operationalism and nothing more”. Implicitly or not, they instill in their views some admixture of realism.

This, of course, is fully rational and understandable. To appreciate how natural it is the best way is to consider a problem different from theirs, namely the quantum measurement problem. Suppose we have to do with an ensemble of generalized, Schrödinger-cat-like measurements, made with instruments having scales with $\mu$ intervals and taking place before time $t$. If we want to strictly remain within the realm of a purely operationalistic description we must be very cautious concerning the way we describe the state of affairs after time $t$. We must do this just by stating that if we look at the pointers after that time we shall get the “feeling” that some of them lie in scale interval 1, some of them lie in scale interval 2 etc., the corresponding proportions being derivable from knowledge of the pointer ensemble density matrix (obtained by partial tracing from the density matrix of the overall ensemble of systems plus instruments). In a “strictly scientific” sense this information incorporates everything that we need to know. A more “realistic” description of the pointers, supposing that it can be given, would add nothing to our predictive powers concerning the phenomenon of interest. But still, most people find it very difficult to believe that this operational description is the “whole story”. Quite naturally they would like to be able to interpret the ensemble $E$ of all the pointers “realistically”, that is, as composed of $\mu$ subensembles $E_1, \ldots, E_\alpha, \ldots, E_\mu$, the components of each $E_\alpha$ being pointers really lying in one definite interval, the one labelled $\alpha$. This shows that, at least in some situations, it is hardly possible to resist our natural inclination towards the philosophical standpoint called realism. What differentiates this realistic description from the foregoing, purely operational one is that we now consider that on every member of each $E_\alpha$ the result of measuring in which interval it lies is predetermined. This assumption cannot be translated in a purely operationalistic language since it is impossible
to operationally ascertain it. To be sure we can measure in what interval this pointer is but nothing proves that it is not this very measurement act that sets it in this interval. Nevertheless the realist considers the assumption in question as meaningful an he finds it so natural that he deems it to be correct.

But then, as is well known, a problem arises. As soon as, following most of the theorists who tried to build up quantum measurement theories, we decide to describe quantum mechanically - that is, either by state-vectors or by density matrices - each one of these $E_\alpha$ “as it really is” (i.e. taking into account the fact that each one of its components is in scale interval $\alpha$ and nowhere else), we automatically get that since $E$ must be the addition of all the $E_\alpha$, it is of the nature of what, by definition, I called a “proper mixture”, describable by a $\rho$ of the form (1) or a trivial generalization thereof. This simply follows from the fact that, obviously, under the conditions stated the quantum description of any one of these $E_\alpha$ must differ from those of the others. Note that this conclusion is of a general nature: the observation that macroscopic systems such as pointers are not isolated from the environment does not alter it since we can, by thought, incorporate environment within the system. For completeness sake let it here be briefly recalled that the conclusion in question may be viewed as being at the source of the “measurement problem”, that, within conventional realism, the problem in question has no satisfactory solution, and that this is considered by some as compelling us to give up the said realism. This analysis, however, fall outside our present subject.

Concerning the present debate two points, I think, emerge from the above. One is that physicists should more thoroughly investigate what they intuitively mean when they, explicitly or implicitly, deal with realistic notions. Let this question be deferred to Section 4. The other point is that, at least as it is used in my book Veiled Reality, the notion of “proper mixture” is not in the least “improper”, contrary to Aharonov and Anandan’s claim. The reason is that, as explained in Remark 5, Section 7.3 of Veiled Reality, the chapters of this book that Aharonov and Anandan refer to were written just for the purpose of making clear the difficulty realism leads to. The whole argumentation in these chapters is, in this respect, negative. In substance, it amounts to exclaim: “Look here: in general ensembles produced this way are not proper mixtures. You cannot think of them as you would think of balls distributed among different vessels”. Considered in
this light, that is, as essentially linked to an interpretation problem - and more precisely to attempts at building up an interpretation couched in realist, or “classical”, terms - the notion of proper mixture far from being “improper” is trivial. It is no more improper that the just introduced notion of balls distributed among different vessels.

Let us turn, then, to Aharonov and Anandan’s objections. First of all, it is true, of course, that picking up one ball in one definite vessel changes the probabilities concerning further pickings that might be done. But then what? Does this make the notion of “balls distributed among vessels” inconsistent, illogical, “improper”? Obviously not. The same holds true here. What must be stressed in this respect is that the notion of balls distributed among vessels (or pointers distributed among scale intervals) is one that we, originally, have; that, as we saw, we intuitively tend to use it, at least in some instances, for giving a realistic meaning to partial trace density matrices; and that, therefore, it was necessary to show that it is not, in general, appropriate for this purpose. Otherwise said, Aharonov and Anandan’s remark about the “memory” of proper mixtures is correct but irrelevant. It does not constitute an objection to the concept of such mixtures since, as shown above, the concept in question is necessary for discussing interpretations of quantum measurement theories that seem natural, not to say “obvious”.

Hence their objection must boil down to one of a semantical nature; one of the type: “the concept d’Espagnet calls ‘proper mixture’ is a valid one but the ensemble it refers to are not mixtures”. Since names always are, to some extent, conventional such semantical discussions are not, as a rule, of much interest. But anyhow, my opinion on this is that the arguments in favor of calling such ensemble “mixtures” are at least as cogent as those for abstaining of doing so. One of them is, of course, that the word “mixture” is an element of our commonsense, everyday language and that, conceptually, proper mixtures are considerably more similar to such “everyday life” mixtures than are the mixtures I call “improper” (that is, the ones for which Aharonov and Anandan insist on saving up the word ‘mixture’). Another and even more significant argument is that, for all purely operational purposes such as those described here in Section 2, proper mixtures can be described by density matrices, that is, are indistinguishable from the ensembles for which Aharonov and Anandan save up the word presently under discussion (indeed, it is only when ‘proper’ mixtures are given some kind of an ontological interpretation, as collections of physical systems, that differences with ‘improper’ mixtures appear, see
below and Ref. [2]).

As for the other criticisms that Aharonov and Anandan try to develop in their Section 4, I think they are unsubstantial as well. In the paragraph in which these authors let \( N \) tend to infinity they observe that the density matrix cannot be any one of “this sequence”. Unfortunately, in their wording it is not clear what the expression “this sequence” actually means. They also observe, as previously noted, that so long as only usual measurements are performed on the system “we cannot point to the state of a physical system that is represented by \( \rho \) in the sense that measurements on this state would give \( \rho \)”. But in what sense is this a criticism? It seems that Aharonov and Anandan have here in mind their own view that a density matrix should be attached to a single system, not realizing that it is not necessarily the view other physicists have in mind.

By contrast, their analysis of the differences I had noted between differently prepared proper mixtures described by the same density matrix is basically sound. In substance, however, it is a mere rewording of what I had already stated in Section 8.3 of Ref. [2]. The points I made there were (i) that the argument “these two mixtures are different since they have been produced differently” is not a universally convincing one; (ii) that while observable differences (the ones Aharonov and Anandan mention) can indeed be produced between these mixtures, the differences in question are actually observable - hence significant - only if these “ensembles” are treated as what, after all, they physically are, that is, as systems of (noninteracting) particles; and finally (iii) that concerning the question whether or not we can define a state by means of a non-pure-case density matrix the answer is yes but only at a (heavy) price. This price, as I explained there, consists in deciding that the ensemble concerning which quantum mechanics yields predictions (concerning nonprotective measurement results, of course!) are essentially abstract constructs, very useful indeed for predicting observation outcomes but not to be considered as composite physical systems (i.e. not to be considered as one would like to consider ensembles of, e.g., pointers, see above). And, correlatively, that - at least in quantum measurement theory - the notion “state” is but a predictive concept.

At first sight this last point seems to contradict Aharonov and Anandan’s views since these authors consider with favor the concept of an “objective reality that could be described by the density matrix”. It is clear, however, that in their approach such an
objective reality is defined only relatively to protective measurements, and it remains to
be seen whether and to what extent such a partial definition of reality is truly satisfactory.
This is the purpose of the next section.

As a last remark concerning proper mixtures let me stress once more that, at the
start, two very general standpoints are conceivable, namely (i) the purely operational,
purely predictive one and (ii) the descriptive, tentatively realistic one. A priori either one
of these two standpoints is worth consideration but we have to choose between them. If
we choose the first one, or if our investigations lead us to consider that it is the only a
posteriori tenable one, then it is clear from the above that there is no point in considering
proper mixtures. But if we choose the second one - or before we have finally discovered this
second one is untenable - I cannot really see why a general notion of proper mixtures (not
restricted to objects we would like to think of as classical) should be an inconsistent one.
The point is that within this second standpoint we cannot cling to the rule - call it Rule
R - that by definition two systems or ensemble of systems that cannot be distinguished
from one another by any measurement are, by definition, identical. This rule R is specific
to standpoint (i). If, in the spirit of standpoint (ii), we impart by thought some reality
to the quantum states there is then no reason why we should not consider ensembles of
systems of the same type lying in different quantum states. As we saw above on the
particular example of pointers, there are cogent reasons to describe such ensembles by
density matrices such as (I) and call them mixtures. And it is easily seen that those of
the objections raised by Aharonov and Anandan that are valid can be disproved, just by
observing that, implicitly, they are based on Rule R and that, in standpoint (ii), Rule R
is not present.

4 Invariants and Reality

As we saw in Section 2, it is a fact that the purely operational standpoint works. On
the other hand, as pointed out in Section 3, it is also a fact that most people are expecting
more from physics. Indeed, many of us consider that the real purpose of physics is to
describe in detail the physical world as it really is. And the very wording of quantum
physics - with such terms as “particle” and “state” - illustrates how powerful this ideal
is. Needless to say, it is a respectable one. It motivates most of the inquiries concerning interpretation of quantum physics and in particular the tentative quantum measurement theories. Concerning wave functions and density matrices it explicitly constitutes the substance of Aharonov and Anandan’s aim. In my view the ideal in question (call it, say, the “realist” one) is not to be dismissed on the basis of purely philosophical arguments. Indeed, it is one that, in the high times of classical physics, was considered by most scientists as being very much within reach and it is reasonable that contemporary physicists with such a realist turn of mind should do their best to preserve it. However, in Section 3 we also touched upon several points that have been quite extensively developed by many authors (see e.g.[2]) and show that, when the phenomenon of quantum measurements is duly taken into account, the ideal in question generates considerable difficulties. Hence the question really arises: the quantum formalism being what it is, is it actually possible to salvage, within it, at least the main elements of a “normal kind” of realism? This, to repeat, is the aim not only of the many authors of quantum measurement theories but also, as it seems, that of Aharonov and Anandan as well.

To study this question it is appropriate to proceed more or less as philosophers would. When discussing such matters, philosophers use to begin by introducing the notion of **invariants**, a substantive referring to invariance with respect to varied modes of apprehension and/or description. In ordinary life we have - they note - visual etc. impressions that change when we move around: but our mind is able to build up the notion of a set of **things** and **properties of things** (such as shape, color etc.) that, we posit, do not really change under these conditions; that are “invariants” with respect to them. Correlatively - they go on noting - our mind spontaneously builds up a theoretical model making it possible to consistently explain its impressions by referring to the invariants in question. And, they claim, it is such invariants that, in ordinary life, we call real. Note in this respect that, in this context, “to call” is more than just to introduce a semantical convention. To call these invariants **real** means hypostatizing them to a kind of special level. It means instinctively attributing to them an “absoluteness” that, as we already noted, cannot be defined operationally (I mean: noncounterfactually) but that somehow makes them differ from a host of other concepts, taste, equations, probabilities, etc. that are not viewed as “physically real”. 

Hypostatizing concepts goes together with hypostatizing the models that are based
on them. We then say, or would like to say, that the models in question are true descriptions of the World as it really is. At this stage, however, an important point must be stressed. It is that this whole hypostatizing procedure is really satisfactory only if the validity of the hypostatized concepts and model is not limited to the description of phenomena taking place within some limited experimental context. For further reference let us call this condition, *Condition A*. When classical physics was in its apex Condition A seemed to be met. The World then appeared as composed of particles and fields and the nature of each one of these constituents could be specified quite independently of the particular phenomenon or experimental procedure the physicists choosed to discuss. In other words, the corresponding invariants were totally noncontextualistic. It was not necessary to strictly associate any one of them with some restricted class of impressions. In that sense, they could be termed universal.

If we set aside the so called “ontologically interpretable models” such as the Broglie-Bohm model - which raise difficult problems of their own, would call for a separate discussion (see e.g.[2]) and lie anyhow outside the realm of the present debate - we must acknowledge that the advent of quantum theory dramatically altered the above picture. Within Bohr’s approach, for example, complementarity means that the validity of any one of the ordinary language invariants is, in the microscopic domain, limited to the description of phenomena taking place within some well-defined experimental context, that is, to a restricted class of observables. In other words, these invariants do not meet Condition A.

Did more recent advances in the field substantially modify the situation? I do not think so. Take, say, the concepts of *events* and *histories*. It is true that the theories of Griffiths, Gell-Mann and Hartle and Omnes partly succeeded in restoring the meaningfulness of these notions within the microscopic domain. It is true that, for example, within a given consistent history branch an event is, in these theories, independent in principle of whether it is observed or not that is, it qualifies for being considered as an invariant in this respect. However, it is not an invariant with respect to the adjunction - or, better to say, the “taking into account” - of some possible future events. In other words, it is an invariant only with respect to quite a limited class of possible experimental procedures: a class that is very far from incorporating all the experimental procedures that are easily available. Hence it is impossible to hypostatize it to the level of an element of reality as
classical realists did concerning the invariants they thought were universal. It does not fulfill Condition A.

Now, since this article is motivated by the Aharonov and Anandan paper, it is worthwhile to point out that, same as in the foregoing example, the Aharonov and Anandan concepts of wave functions and density matrices attached to one system only are, as stressed for example by Bitbol [3, 4], merely partial invariants. They are invariant with respect to protective measurements but only with respect to them. Hence the above conclusion also holds good concerning them. These invariants do not fulfill Condition A. Admittedly we may, if we like, take up the convention of calling them real. Viewed as a mere convention this one may be useful (see below). On the other hand, since protective measurements constitute a highly limited class of possible experimental procedures, when trying to predict ordinary measurement outcomes we shall obviously continue being in need of considering probabilities, statistical ensembles and so on. In this domain, density matrices attached to one system only will be of no help. Willy-nilly we shall have to go on interpreting them as referring to the ensembles in question.

5 Conclusion

There is no doubt that the protective measurements concept and the possibility it yields of - in some conceptual contexts - interpreting density matrices as descriptions of one system “states” are quite interesting indeed. This is shown for example by the content of Section 3 of Aharonov and Anandan’s paper. Of course, we have known for a long time that as long as we decided to forget about such things as the measurement problem and the Born probability rule, the remaining quantum rules could be stated as genuine physical laws, that is, without referring to human actions and observations: otherwise said, in a strongly objective language parallelling the strongly objective language of classical physics. With this proviso we could say: “there exist wave functions and/or density matrices that have such and such properties, obey such and such equations etc.”, much as had been done in classical physics concerning material objects and fields. But there remained the difference that in classical physics the entities thus said to exist (positions of objects, field strengths etc.) could also be measured, a circumstance that contributed very much
to remove any suspicion that their postulated existence was “unwarranted metaphysics”. This is a difference that the new ideas brought in by Aharonov and Anandan remove. And in this sense it must be granted that these ideas do bring quantum mechanics somewhat closer to a realistic world view that many physicists consider as being the only acceptable one.

But on the other hand we should keep in mind that all this holds good only as long as the proviso is accepted of forgetting about the measurement problem while, of course, we cannot really forget about it. It is a fact that the great majority of the measurement we perform are not “protective” ones, that they yield definite values according to definite probability laws etc.. and it seems clear that, as stressed at the end of the foregoing section, the existence of these effects will oblige us to continue using probabilities, ensembles and density matrices in the usual way. Now, in this field all the analyses that have been made, along the years, of the quantum mechanical rules and the measurement problem (including the fact that improper ensembles are not to be identified with proper ones!) remain valid. All taken together, they show that the strongly objective language of the main parts of classical physics cannot be consistently used in quantum physics. Finally therefore, as shown in [2], quantum physics as a whole can only be expressed in a weakly objective language, in which objectivity is identified with universal intersubjectivity.
References

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