Measurements according to "Consistent Quantum Theory"

Elias Okon

Instituto de Investigaciones Filosóficas, Universidad Nacional Autónoma de México, México City, México.
E-mail: `eokon@filosoficas.unam.mx`

Daniel Sudarsky

Instituto de Ciencias Nucleares, Universidad Nacional Autónoma de México, México City, México.
E-mail: `sudarsky@nucleares.unam.mx`

Abstract:

We critically evaluate the treatment of the notion of measurement in the Consistent Histories approach to quantum mechanics. We find such treatment unsatisfactory because it relies, often implicitly, on elements external to the provided formalism. In particular, when dealing with measurement scenarios, the formalism, in order to be informative, needs to assume that after measurements measuring apparatuses are always in states of well defined pointer positions. The problem is that there is nothing in the formalism to justify this assumption. We conclude that the Consistent Histories approach, contrary to what is claimed by its proponents, fails to provide a truly satisfactory resolution to the measurement problem of quantum mechanics.

1   Introduction

The measurement problem, as commonly understood, is the fact that, even though the standard formulation of quantum mechanics depends crucially on the notion of measurement, such notion is never formally defined within the theory. Then, in order to apply the formalism, one needs to know, by means external to quantum mechanics, what constitutes a measurement, when a measurement is taking place, and what is it that one is measuring.

Bohr's famous response to this issue involves taking a purely operational stance. That is, it involves holding that quantum mechanics is a theory that only talks about probabilities for outcomes of measurements performed by observers, and nothing more. The problem is that such position is untenable if one wants to think about the theory as the basis for a complete description of a physical world that has ontological

independence from us and lays beyond ourselves and our brains.

A more detailed way of presenting the measurement problem is as the inability, within orthodox quantum mechanics, of answering the question of when to use the U process (evolution according to Schrödinger equation) and when the R process (reduction postulate). In the Copenhagen interpretation, the answer is seemingly straightforward: for a given micro system S, use U except at those times when that macro world of measuring devices and observers interacts with S. The issue of course is unsatisfactory if one believes that quantum theory should apply to everything and that there is no fundamental wall separating the macro-world from the micro-world. Clearly, the issue becomes even more serious if one wants to apply quantum mechanics to cosmology (see [1]).

The Consistent Histories (CH) approach to quantum mechanics (also known as Decoherent Histories or Consistent Quantum Theory) is regarded by its proponents as an alternative interpretation of quantum theory that overcomes the measurement problem. It was first introduced in 1984 by Griffiths through [3], and developed in the subsequent years by himself, Omnès, Gell–Mann and Hartle in [4, 5, 13, 14, 2, 12]. Contemporary presentations of the formalism include [6, 7, 11, 8, 9], ref. [6] being regarded as the most detailed and complete discussion of the conceptual foundations of CH available.

In fact, CH recognizes not one but two different measurement problems (see [9]). The first corresponds to the one we mentioned above, namely, the fact that if a measurement apparatus is treated quantum mechanically, then the Schrödinger evolution will typically drive it into a superposition of macroscopic states and no definite outcomes will be obtained. In [6], for example, the orthodox solution to the measurement problem is criticized as follows:

> "it seems somewhat arbitrary to abandon the state... obtained by unitary time evolution,... without providing some better reason than the fact that a measurement occurs; after all, what is special about a quantum measurement? All real measurement apparatus is constructed out of aggregates of particles to which the laws of quantum mechanics apply, so the apparatus ought to be described by those laws, and not used to provide an excuse for their breakdown." ([6, p. 214])

The second measurement problem has to do with the impossibility, within the standard theory, of relating the pointer positions of a measurement apparatus with the state of

the measured microscopic system *before* a measurement take place. That is, with the fact that, in standard quantum mechanics, measurements typically disturb the measured system and so the values of the measured properties before and after the measurement are not related.

According to its proponents, CH constitutes a *realist* approach to quantum theory that solves the measurement problem (actually both of them), as well as a whole range of quantum paradoxes. The claim is that the formalism assigns probabilities for all systems, microscopic or macroscopic, using the same machinery and without any reference to measurements. Therefore, actual laboratory experiments can be analyzed in purely quantum terms, using the same principles that govern any quantum system. Consequently, CH is held not to be based on measurement and not to require an artificial separation between the classical and quantum domains. For instance, in [10, p. 3] one reads: "In this formulation, it is the internal consistency of probability sum rules that determines the sets of alternatives of the closed system for which probabilities are predicted rather than any external notion of 'measurement'."

In broad terms, the formalism that (allegedly) accomplishes the above, works as follows. One starts with the notion of a *history*, which is a sequence of properties of a quantum system. Such properties are represented by projection operators on the Hilbert space at successive times. Then, one introduces sets of histories and specifies rules that assign probabilities to the various elements of the set. However, not all sets or families of histories allow for probabilities to be assigned. This assignment can be done provided that the sum of probabilities of the histories in the family equals one and that all pairs of histories within the family are orthogonal (in the sense that there is no overlap between them). Families satisfying these two conditions are called *frameworks*, or *realms*, and represent the only sets of histories for which probabilities can be consistently defined. Frameworks, then, constitute the quantum counterpart of sample spaces in ordinary probability theory.

There is however an apparent complication; given a quantum system, multiple incompatible frameworks can be constructed. In order to avoid inconsistencies, the following rules or principles must be enforced (see for example [6]):

- **Single-Framework Rule (SFR):** probabilistic reasoning is invalid unless it is carried out using a single framework.

Therefore, any prediction of the theory must be constrained to a single framework and statement belonging to incompatible frameworks cannot be compared in any way.

- **Principle of Liberty**: one can use whatever framework one chooses in order to describe a system.

- **Principle of Equality**: all frameworks are equally acceptable in terms of fundamental quantum mechanics.

- **Principle of Utility**: not all frameworks are equally useful in answering particular questions of physical interest.

As a result, the CH formalism violates the **Principle of Unicity**, meaning that, according to the theory, no single framework suffices to characterize completely a quantum system. Therefore, quantum systems in particular, and reality in general, are characterized by various alternative and incompatible descriptions.

As we remarked above, and as can be noted from our description of the formalism, *measurements* play no special role within CH. Standard, laboratory measurements are then just another type of physical process which can, as any other process, be described within the theory. In order to do so, an appropriate framework must be selected from which probabilities, identical to those given by applying Born's rule, can be calculated. Therefore, even though measurements play no fundamental role within the theory, the CH formalism appears to be capable of describing them consistently, yielding the same results as the orthodox interpretation.

All of the above sounds very appealing; the CH formalism seems to avoid the notion of measurement and to give rules that are unified in the treatment of micro and macro systems. Now, the question is whether the reference to measurement as a special and independent notion, which was so bothersome within the Copenhagen interpretation, has really been removed from the formalism. Our claim is that the answer to the previous question is negative, that the reference to measurements remains hidden in the unspoken rules that indicate what framework should one use when considering any specific measurement situation.

Our point of view is that issues related to measurements of all sorts are rather opaque in the general CH setting. The main problem is that applications of the formalism to actual experiments, as discussed, for example, in [6], require in order to really work, the incorporation of elements external to the CH framework. Furthermore, these external notions, in essence, bring back in a hidden manner the basics of the Copenhagen measurement ideas (i.e., the notion that after a measurement, a measuring apparatus is always in a state of well defined pointer position). Therefore,

when discussing measurement situations, there is a reliance on an implicit rule, often presented as "common sense," whereby those states of the measuring devices characterized by superpositions of states of different values of the pointer are simply not allowed as final states. The problem is that there is nothing in the formalism to justify this assumption. We hold, therefore, that contrary to what is claimed, the formalism offered simply fails to address the measurement problem. Furthermore, given that, after all, measurements are our only way to verify the validity of any theory, the inability of the CH formalism to deal with them represents a serious complication. In the next section we explain in detail what we find unsatisfactory about the treatment of measurements in CH.

2   Measurements in Consistent Quantum Theory

As we mentioned in the opening paragraph, the measurement problem consists of the fact that the notion of measurement is essential, but never formally defined, within standard quantum mechanics. Therefore, in order to apply the formalism, one needs to know, by means external to quantum mechanics, what constitutes a measurement, when a measurement is taking place, and what is it that one is measuring. A solution to the measurement problem, then, would be a quantum theory that can be used, i.e., that is able to make predictions, without requiring to introduce elements external to the theory. What we expect from such a theory is to inform us on the possible outcomes of experiments, along with their corresponding probabilities, and not only to provide us with probabilities for a list of outcomes that must be provided "by hand."

In this section we critically examine various aspects of the treatment of measurement in CH, particularly as developed in [6, 11, 9]. Our main objection on such treatment stems from the fact that the procedure needed to apply the formalism to measurement situations seems to require, in order to be useful, elements external to the explicit CH formalism. That is, CH does not really constitute a satisfactory solution to the measurement problem as described above. Let us explain in detail why we believe this to be the case.

First of all, in the description of the procedure presented in [6], there are statements that indicate rather explicitly the dependence of the formalism on external input. For instance, in discussing the Stern-Gerlach experiment in section 17.2, the book states (all emphasis in bold is ours):

"The unitary history... cannot be used to describe the measuring process, because the measurement outcomes... are **clearly incompatible** with the final state [of the unitary history]... A quantum mechanical description of a measurement with particular outcomes must, **obviously**, employ a framework in which these outcomes are represented by appropriate projectors..." ([6, p. 202])

That is, one must know, by means external to the formalism, what are the possible outcomes of the experiment. It is then necessary to adapt the selection of the framework according to that knowledge. But where does such knowledge come from? It seems that it comes, on a case-by-case basis, from some of our laboratory experiences. The application of the formalism, is, in a sense, relying implicitly on the practical fact that in laboratory situations we know what is being measured, which part of the system is to be considered as an apparatus, and that under those conditions the possible results are those indicated by the Copenhagen interpretation. We must, then, use our experience with macroscopic apparatuses as part of the input for the analysis. That means that the theory does not seem to account for those experiences but needs them as extra input. This seems to defeat the purpose of going beyond the Copenhagen interpretation. What we need, instead, as we remarked above, is an approach that removes the requirement to know a priory i) what part of the system is to be considered a "macroscopic apparatus," as those should in principle be *treated within the quantum formalism* and ii) what are their possible final states, as those should in principle be *provided by the formalism.*

There are various other examples of such kind of statements appearing in the analysis presented in [6]. For instance:

"Another way of stating this is that whatever the initial apparatus state, unitary time evolution will inevitably lead to an MQS [macroscopic quantum superposition] state in which the pointer positions **have no meaning**. Hence it is **essential to employ** a non-unitary history in order to discuss the measurement process" ([6, p. 207])

The issue is then: why, precisely, is it that the state characterized by the superposition of pointer positions, has no meaning? It seems that the answer is not provided by the formalism and must be extracted from experience. However, the purpose of a formal theory should be precisely to account for such experience. Other examples are:

"It is customary to use the term *pointer basis* for an orthonormal basis, or more generally a decomposition of the identity such as employed in the generalized Born rule... that allows one to discuss the outcomes of a measurement **in a sensible way**." ([6, p. 115])

or

"While quantum calculations which are to be compared with experiments usually employ a pointer basis for calculating probabilities, **for obvious reasons**, there is no fundamental principle of quantum theory which restricts the Born rule to bases of this type." ([6, p. 115])

The problem, again, is that this "sensible way" of choosing the basis constitutes an element extraneous to the theory. It represents information not present in the theory itself, crucially required in order for the theory to deliver an accurate description of what in fact we observe in experiments. In other words, the formalism is unable to inform us about the possible outcomes of a given measurement, this information must be put in "by hand" while choosing a "reasonable" basis.

A common response of CH proponents to the type of objections raised above is that the selection of a framework for measurement situations is not put in "by hand" but that it is chosen in order to "model the particular experimental situation designed by the experimentalist." Accordingly one reads

"There is, to be sure, an alternative unitary framework in which the fearsome cat... is present..., and physicists, philosophers and science fiction writers are at Liberty to contemplate it as long as they keep in mind its incompatibility with (and thus irrelevance to) the sorts of descriptions commonly employed by competent experimental physicists when describing work carried out in their laboratories." ([9, p. 105])

or even more explicitly,

"at the time... just before the measurement $|\psi_0\rangle$ is incompatible with the... [projective decomposition] corresponding to **the properties... the apparatus has been designed to measure**, a framework in which $|\psi_0\rangle$ makes sense will not be useful for discussing the measurement *as a measurement*, i.e., as measuring *something*, so the physicist interested in that aspect of things must use something else." ([9, p. 105])

But, what is the connection between what we normally call a measurement and the prescription for the particular framework one needs to use in a specific situation? If the answer is: we know in practice what is a measurement, then all that has been achieved is moving or transforming the "measurement problem" into the "framework choice problem." In other words, when faced with a specific measurement situation, how is it that the characterization of apparatuses in a fully quantum language provides the recipe of how the framework is to be chosen? This question does not seem to be addressed at a fundamental level, but only in a case-by-case fashion, and it is done using extraneous considerations (i.e., notions that are not part of the axioms of the CH approach to quantum theory). For instance, in the treatment of the Stern-Gerlach experiment in [6] referred to above, the unitary history is discarded simply because "the measurement outcomes $|\omega^+\rangle$ and $|\omega^-\rangle$ are clearly incompatible with [it]." That is, we must know by experience what are the possible results obtained by an experimentalist, and thus adapt the application of the theory accordingly. In other words, one must choose the framework by using what one knows about the functioning of a measurement. It seems then that one must use the recipe of the Copenhagen approach, known to work "for all practical purposes," in order to select the framework. The reliance on the notion of what is "clearly incompatible" has no other basis than that, and (as far as we can see) is not specified a priori by the postulates of the CH approach.

Another typical response regarding framework selection for measurements is to claim that the choice must be made with utilitary purposes in mind. That is, the framework must be chosen "according to the questions one is interested in answering." Therefore, one finds

> "a measurement of S_x, for example, needs to be interpreted in the x framework. It will not be informative if analyzed in the z framework." ([11, p. 2838])

or, regarding unitary evolution

> "This is a valid prediction of quantum mechanics, but unless the final state is the eigenstate of some **straightforward observable** it is also not a particularly useful prediction since it contains no accessible physical information." ([11, p. 2841])

or

> "It is true, of course, that in the unitary framework... the wave function constitutes a valid history, but that history is, in general, **not useful for answering physical questions** about the system..." ([11, p. 2843])

The trouble that we have with the utilitary answer is that it hides the fact that the formalism does not offer any clear characterization regarding the exact relation between the experimental setup, the framework one needs to use in order to get useful information out and the questions one wants to answer. The most detailed answer to our question appears in [11, p. 2838] where, when discussion the measurement of a spin, it is stated that

> "in the x framework any state has two disjoint possibilities or properties $[x^+]$ and $[x^-]$, so for a system prepared in the state $|z^+\rangle$ a measurement of S_x, when viewed in the x framework, will reveal one or the other of these properties, each with probability $\frac{1}{2}$. In the z framework, on the other hand, I am unable to interpret an S_x measurement."

A few questions come up with respect to the description given above. First, how does the formalism establish the relation between the framework we decide to use and what we in fact observe when we perform the experiment? So, for example, how would we explain what we *in fact observe* if we decide to use the z framework when performing a measurement of S_x. The framework is supposed to be chosen according to the questions one wants to answer. However, how these questions are tied to experiments is left unspecified. In fact it becomes clear, when facing some concrete situations, that the problem is that there are fundamental questions which CH is simply incapable of answering. For example, if one is given a new measurement equipment, described entirely in quantum terms (via a Hamiltonian, an initial state, etc.), CH is unable to answer which are going to be its possible final states when we use it in the laboratory. That is, in a given situation, described by providing the complete physical setup in terms of the initial state of the closed system and the Hamiltonian, it is unclear what framework one must chose. CH indicates that the choice of framework depends on "the questions one asks" but how do these correlate with the experimental setup as characterized by the Hamiltonian of the whole system, including measuring apparatus and probe (which cannot be subject to different rules if one wants to avoid the standard Copenhagen quantum/classical divide)? In short, by relying only on the formalism, one cannot decide what is the relation between experiments performed by humans,

and the different sets offered by the formalism. Furthermore, according to [6], given an experimental set up there is, among all the sets offered by the formalism, only one that correctly describes the outcomes (and, as we remarked above, one must use information external to the formalism in order to determine which one is the right one). If this is the case, then it seems to us not only that the formalism is incomplete but that the other frameworks play no role whatsoever in the application of the formalism to particular situations. That is, if the point of view taken is that there is one world, (a fact that must humans seem to agree about) with one space-time (at least there is one to which we have access to), then that is the one that we need to describe, not multiple ones.

One last possible answer to our objections that we will consider is the claim that the proposed formalism is capable of making predictions for all experiments or observations suitably specified and that that is enough for physics, regardless of the conceptual issues we might have problems understanding. We, however, do not believe that the CH framework, without the aid of external input, is capable of making specific predictions. As we showed above, in order for the formalism to do so for a given experiment, one must "put by hand" what are the possible outcomes for the experiment, a fact that one knows from experience and not from using the formalism. Without such external information, the formalism is unable to correctly describe what we in fact observe in the laboratory. Another way to say this is that in order to make predictions, one must specify the observables to be measured and the times of such measurements. The problem is that at such point the measurement problem resurfaces because we need to know what constitutes a measurement, something not specified within CH. On the other hand, if one wants to simply specify the physical systems and their Hamiltonian (including within the system the quantum description of the measuring devices and the interactions of those and the system), then no predictions emerge.

Let us try explain once again what we mean when we say that CH is incapable of making predictions without the aid of external input. A CH proponent may say: "given that one has identified appropriate initial states, states of the system to be measured, and apparatus states after the measurement is complete, CH is not only capable of making predictions, it makes precisely the same Born rule predictions given in textbooks". The trouble that we have with that description is the bit about having to provide "apparatus states after the measurement is complete," which are exactly what you want a good physical theory to provide. Especially a theory like CH that allows for different frameworks, many of which contain final states for the measuring

10

apparatuses different than what in fact we observe in the laboratory. In other words, without a selection of a framework, no predictions are possible, and the selection or identification of the appropriate framework relies on extraneous notions (i.e., notions not specified in the Hamiltonian or in the initial state of the system) and not identified by the general rules of the CH approach.

To illustrate this, consider again the Stern-Gerlach example of sec. 17.2 of [6], quoted above. Without the external information about what are the correct "measurement outcomes" it is not possible to argue that some framework, containing a given final state, is not acceptable. Therefore, without that extraneous input, one cannot select the framework, and without it one cannot assign probabilities. The notion of "measurement outcomes" is extraneous in the sense that it is not part of the explicit CH prescription. It is not encoded in the system or in the Hamiltonian. In a sense one uses the fact that one "knows what is a measurement" and thus claims to bypass the measurement problem. Finally, regarding CH making predictions equivalent to those given by the Born rule, we agree that that is the case, but only after the external information about the possible final states for the measuring apparatuses is introduced. However, we do not find that surprising because by introducing such external input the standard Born rule itself is being inadvertently introduced!

Another point we would like to question is the claim within CH regarding the non-existence of physical collapses. In this respect one reads:

> "If the Hamiltonian of the system is such that a measurement of A is carried out at the time t_n, then this framework embodies the orthodox notion that in all histories the system is described by $|\psi(t)\rangle$ from $t = t_0$ to $t = t_{n-1}$ and that the system then "divides" into the different eigenfunctions of A when a measurement of A is made at $t = t_n$. In CQT this collapse framework is a valid representation of the system, but by no means the only one and it does not signify that any physical collapse has taken place." ([11, p. 2842])

The question is, if no collapse actually occurs, why would one decide to use such framework?, how did it occur to anyone to use it?

To conclude this section, we would like to make one last comment about what in [9] is referred to as "the second measurement problem," which is the fact that the standard theory is incapable of relating the pointer positions of a measurement apparatus with the state of the measured microscopic system *before* measurements

11

take place. CH is supposed to be able to solve the problem because, for example, in a EPR setting, one can meaningfully talk about "particles possessing definite spin in advance of measurements." So, for example, in discussing EPR [6] explains

> "Even stronger results can be obtained using the family... in which the stochastic split takes place at an earlier time. In this family it is possible to view the measurement of S_{az} as revealing a pre-existing property of particle a at a time before the measurement took place, a value which was already the opposite of S_{bz}." ([6, p. 282])

It seems then that one can say that some particle had a definite property before a measurement took place only if one chooses a specific framework where this is so. But, if according to CH, all families are equally valid, it is not clear why the description according to this one family should be taken that seriously. That is, the problem is solved in only one of a infinite number of possible frameworks.

3 Alternatives

So far, we have presented our objections against the tratment of measurements in CH. An advocate of such theory could reply that it would be better if we could propose instead an alternative formulation of quantum mechanics of comparable scope and clarity that has the features we seem to require. Then we could compare the two to see if they are distinguished by experiment. So, do we think we already have a fully satisfactory version/modification of quantum theory that is free of inconstancies and agrees with observations? The short answer is: no. However, let us give a more appropriate, although much longer, answer by saying the following:

The first thing is that the two questions (about an evaluation of CH and about the development of an alternative) should be considered quite separately. The issue of whether or not one theory works is quite independent of whether one has or has not a better alternative. If, for analogy, one is supposed to be providing a proof for a certain theorem, the issue of its validity as a proof is independent of whether there are other proofs available or not. Of course, once one has a valid proof, one might want to evaluate if a second proof is better, (say in the sense of being shorter and still being valid, or perhaps easier to follow), than the first one. However, one cannot argue that a given proof is valid simply because there are no others available.

Now, if one is in a situation where no good proofs are available, one might want to evaluate if a certain path towards a proof seems promising or not. One might even enter in a debate of whether a certain second path is more or less promising than the first one. These two situations, the one without and the one with a valid proof, are quite different. In the first case, it might be justified to consider, in the context of a search for a proof, some rather complicated paths and elaborated constructions that one might find esthetically or metaphysically unappealing. In a situation where a perfectly sound proof does exist, anyone taking as a line of research the unappealing path might reasonably be considered to be wasting his/her time.

In that sense, our long answer to the above question is:

1. We are quite convinced that the CH approach does not offer a fully satisfactory resolution of the measurement problem, (i.e., it's not a good proof).

2. We do not claim to have at this point another proposal that is shown to be fully satisfactory (i.e., we do not claim to have a valid proof either).

3. We believe that both Bohmian mechanics and objective collapse models (along the lines of GRW or CSL) do offer (not yet wholly satisfactory but) promising paths. We might of course be proven wrong here.

Moreover, we believe that it is possible to empirically test objective collapse theories by combining then with models of inflationary cosmology and by comparing the predictions to the precise data from the CMB and large-scale structure. We also believe that it is possible to set bounds on the parameters of such models with the aid of laboratory experiments. However, as such enterprise involves a radical idea (i.e., modifying quantum theory) it is important to make sure that something like that is really needed. That is, that there is a problem that the exiting proposals fail to resolve in a fully satisfactory way. In this sense, we do see the CH approach, as well as the proposals of interpretational adjustments based on decoherence, as the most powerful attempts to avoid the conclusion that quantum theory needs to be modified. Unfortunately, we do not believe they succeed.

4   Conclusions

In this manuscript we have critically analysed a key element of the CH proposal to solve the measurement problem of quantum theory, namely its treatment of the notion

of measurement (a notion that CH does not take to be fundamental).

We have started by identifying the essential difficulty involved in the measurement problem and by clearly stating what would constitute a satisfactory solution. In this regard, we stress the need to provide a unified treatment for all physical systems where no privileged status should be given to any subsystem simply because it is identified as an observer or as a measuring device. Therefore, the treatment of such subsystems should not involve special and distinct rules, over and above those which are suppose to rule the quantum mechanical characterization of the behavior of any other subsystem.

We have seen that the CH formalism fails to provide a truly satisfactory resolution of the problem precisely because, contrary to what is claimed by its proponents, when treating measurements it cannot avoid relying, often in an implicit way, and other times by disguising them as "common sense," on elements external to the formalism.

We hope that this work contributes to research on the foundations of quantum mechanics by helping to focus attention on the deficiencies of what seems, at first sight, to be a very promising path toward the resolution of this most serious conundrum within one of the must successful theoretical constructs of the twentieth century. Moreover, we hope that this kind of study will encourage others to search for alternative ideas that might lead us to the proverbial "light at the end of the tunnel."

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