Reply to "Comment on 'Wave-function collapse by measurement and its simulation'"

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We discuss and clarify the concept of dephasing in the many-Hilbert-space approach to the quantum measurement problem. We argue that the phase randomization provoked by a detecting macroscopic device is responsible for the loss of quantum coherence, and that the "collapse of the wave function" is to be regarded as a statistical process for the accumulated distribution over many events. In stressing the fundamental differences between our approach and the Copenhagen interpretation, we counter the objections put forward by Johnston [preceding Comment, Phys. Rev. A **48**, 2497 (1993)], whose standpoint is essentially similar to the Copenhagen one.

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In his Comment [1] on our paper [2], Johnston criticizes the many-Hilbert-space (MHS) approach [3] to quantum measurements and argues that our logic is "flawed." We claim that Johnston's argument is based on definitions of *wave-function collapse* and *perfect nondestructive detector* that are completely different from ours, and in fact totally rely upon the so-called Copenhagen interpretation (CI) [4]. The CI is exactly what we have endeavored to counter by our MHS approach. Therefore we do not agree with Johnston's claims and below we present counterarguments.

First of all, let us briefly review the content of Sec. II of Ref. [2], which has been, in our opinion, completely misunderstood. Let the wave function of a particle be decomposed into $\psi=\psi_1+\psi_2$, where ψ_1 and ψ_2 are the two branch waves running through the two spatially separated routes 1 and 2. A detector D is inserted along the second route only. Accordingly, the wave function becomes

$$\psi_2'=T\psi_2 \ , \tag{1}$$

where T is the "transmission coefficient" that fully takes into account the interaction between the "particle" and the (macroscopic) detector. (We shall not address here philosophical issues about the meaning of the word "particle." See [2].) The quantity T is complex, and its modulus is very close to 1 if D is an *ideal nondestructive detector*. Notice that, if D is macroscopic, the phase of T can become completely random. In fact, an *ideal detector* is rigorously defined [see Eq. (46) of Ref. [2]] as a device for which

$$|\overline{T}|^2=\overline{T}=0 \ , \tag{2}$$

where the bar denotes an average over many events in the same experimental run, or, according to our ergodic assumption [2], an ensemble average over the microstates of the macroscopic detector. Observe that the above relation *does not imply at all* that the transmission probability $t=|T|^2$ vanishes *for every single event.* Indeed, an *ideal nondestructive detector* is rigorously defined [see Eq. (47) of Ref. [2]] as an ideal detector for which the additional condition

$$\overline{t}=\overline{|T|^2}=1 \tag{3}$$

is met. Equations (2) and (3) simply mean that *every particle is transmitted with a random phase.* This is the point overlooked by Johnston.

If the two branch waves are recombined, the average probability for observing the particle is

$$\begin{aligned} P=|\psi_1+\psi_2'|^2&=P_1+P_2+2\,\mathrm{Re}(\psi_1^*\psi_2') \\ &=P_1+P_2+2\,\mathrm{Re}(\psi_1^*\overline{T}\psi_2) \ , \end{aligned} \tag{4}$$

where $P_1=|\psi_1|^2$, $P_2=|\psi_2'|^2$. The last addendum on the right-hand side is the interference term and reflects the coherence between the two branch waves. For an ideal detector, by Eq. (2), we get

$$P=P_1+P_2 \ . \tag{5}$$

The right-hand side of the above equation is a sum of probabilities of finding one of two mutually exclusive events 1 and 2. Therefore, this means that once one event (say 1) has happened, the other one (namely, 2) never occurs. This is the *collapse of the wave function.* It is characterized by the lack of the interference term, and is regarded as a *statistical* process for the accumulated distribution over many events:

$$\sum_{\text{accumulated}} \mathrm{Re}(\psi_1^*\psi_2')=0 \ . \tag{6}$$

Notice that under this notion of wave-function collapse we do not require a branch wave to disappear in a single measurement, as the CI and Johnston imply, but assume instead that both branch waves still exist after the interaction with D. The essential ingredient to obtain (6) is *not the disappearance* of one branch wave but the *decoherence* between the two branch waves. Let us now proceed to counter Johnston's objections.

(i) According to our definition, and in contrast with Johnston's, an ideal nondestructive detector is, in essence, a device that transmits every particle with a random phase. It is, therefore, a device that *modifies in an essential way* the wave function of the incoming particles. In the light of this consideration, his claim that "there is no significant change in the wave function associated with the nontriggering of the detector" is incomprehensible to us.

The process described above does not result in "zero detection probability downstream of the detector." On the contrary, as repeatedly emphasized, the wave function is still present after the interaction with a detector. Therefore, there is no "major departure from quantum theory." As stressed at the end of Sec. IV of Ref. [2], all secondary processes, such as countertriggering, following the wave-function collapse must be described through the time evolution of the density matrix of D, as in Eqs. (34) and (35) of Ref. [2]. The D states are suppressed, in Eq. (1), although all the effects of the interaction between the object particle and D are fully taken into account via the coefficient T. The exhaustive density-matrix formulation of the measurement process, expressing the D states in an explicit way, is given in Sec. IV of Ref. [2].

In our view, the collapse of the wave function, regarded as a *statistical dephasing* process, should be *strictly distinguished* by any discharge phenomena taking place in D. The latter are just secondary processes, which are set in D in order to *display* the result of the measurement. The presence or the absence of such "display processes" should be regarded on an equal footing [3,2].

Johnston also claims that an ideal nondestructive detector is unrealizable within quantum theory. However, for example, in neutron interferometry the simplest realization of an ideal nondestructive detector is the following: Take a "bad" phase shifter, with a very rough surface. Different neutrons will impinge on different parts of this surface, and acquire different phases. The phase randomization can even be made bigger by increasing the temperature of the phase shifter. An analogous situation has been mimicked in Fig. 9(b) of Ref. [2].

(ii) The objection put forward in point 2 of Ref. [1] is also not understandable. We have *never* stated that a loss of amplitude is equivalent to collapse. On the contrary, a careful analysis of the values of the decoherence parameter (Table I of Ref. [2]) shows that even though the amplitude of one of the two branch waves may be reduced by the presence of an absorber, this effect has nothing to do with the loss of quantum-mechanical coherence (collapse). This is discussed at great length on pages 41 and 49–50 of Ref. [2]. Johnston's starting point is incorrect, and this is the cause for our alleged "illogical conclusions." Incidentally, we agree with Johnston that loss of interference is by no means equivalent to collapse as was shown in Ref. [5]. Several examples of this sort have been discussed in the literature [6]. What Johnston does not realize is that in some cases, although there appears to be no interference, quantum-mechanical coherence can be *fully preserved,* because the evolutions are unitary and it is always possible, in principle, to restore a beautiful interference pattern [5]. The above-mentioned case is not the object of the present discussion. On the other hand, if Eq. (2) holds, quantum coherence is lost, and cannot be recovered. In this case, the phase randomization is *irreversible* and *complete dephasing* occurs: This is the collapse of the wave function.

We emphasize that the loss of quantum-mechanical coherence (i.e., the vanishing of the off-diagonal terms of the density matrix) is shown explicitly, in the MHS approach, and is not to be simply ascribed to the orthogonality of the apparatus wave functions and/or to be postulated, like in the original theory of von Neumann, in which the wave function is collapsed due to the intervention of an external observer that is supposed to provoke an *acausal* change of the wave function by simply observing the quantum system. No external observer is needed in our case, in order to explain the collapse of the wave function, for the evolution from a pure state to a mixture is simply a statistical effect due to the macroscopicity of the detector.

We have emphasized the statistical nature of quantum measurements, and have stressed that it is meaningless, in our approach, to speak of wave-function collapse for single events. The statistical nature of our theory is based on the fact that quantum mechanics itself can never give a definite prediction for a single measurement on a single system, but can only predict a definite result on the accumulated distribution given by many independent experiments performed on many dynamical systems, each of which is described by the same wave function. However, as was repeatedly remarked, this fundamental nature of quantum mechanics does not exclude the special case in which we get a definite result in a single experiment, when each dynamical system is in an eigenstate of the observable to be measured. Of course, our theory can tackle such a situation. In this case, the expected statistical fluctuations become extremely small on the accumulated distribution.

It is not cogent to demand, as Johnston does, that our theory yield general predictions for the outcome of *single* detection events. We do not see why our approach should go beyond the *probabilistic* predictions of quantum mechanics. In fact, that would be a "major departure from quantum theory."

Johnston, in the first part of his comment, endeavors to clarify "the concept of a perfect detector" and "the concept of collapse applied to wave functions associated with individual events." We wish to stress, however, that the concepts he puts forward are probably in agreement with the CI, but not with what we proposed. In fact, as repeatedly emphasized, one of our main objectives is to dispense with the CI.

As a concluding remark, we would like to stress that one of the most important results of our approach is the introduction of a sort of order parameter (named decoherence parameter ϵ) that enables us to judge whether an instrument can work well or not as a detector, in order to yield the "wave-function collapse." Our theory can deal, by means of ϵ, with a wide class of phenomena, including interference, perfect, and imperfect measurements, while the CI cannot do the same; in particular, it cannot formulate the notion of imperfect measurement. Nowadays, in our opinion, the task of a satisfactory theory of quantum measurement is not only to describe the "wave-function collapse," but also to analyze concretely the most recent foundational experiments, and to disclose new physical situations. Our present efforts [7] are concentrated on the analysis of real experiments [8], by means of ϵ and within the framework of the MHS theory, and our theoretical results are fully consistent with the experimental data.

We think that a measurement theory should be liable to experimental test, and not just confine itself to the realm of philosophy and academic discussions. Johnston does not suggest any *experiment* to show that our theory is flawed, and does not base his criticisms on cogent arguments. We shall discuss our theory again in the near future.

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