COMMENTS

Comments are short papers which criticize or correct papers of other authors previously published in the **Physical Review**. *Each Comment should state clearly to which paper it refers and must be accompanied by a brief abstract. The same publication schedule as for regular articles is followed, and page proofs are sent to authors.*

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

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Namiki and Pascazio [Phys. Rev. A **44**, 39 (1991)] propose a model of wave-function collapse associated with the nonfiring of a detector in one of two paths for which a particle is known to be present, and argue that the collapse has the same status as the collapse associated with a measurement involving an actual detection. I question this proposal on the following grounds: (i) it is based on a shift in detection probabilities in the two paths while there is no shift in wave-function amplitudes, and (ii) Namiki and Pascazio use an insufficient measure, loss of interference, as an indicator of collapse.

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This is a criticism of the paper by Namiki and Pascazio [1], which is based on the many-Hilbert-space (MHS) theory of quantum measurements [2]. The many-Hilbert-space theory proposes that a "negative measurement result," e.g., the absence of a measurement result at a perfect, nondestructive detector in one of two paths in which an incident particle is known to exist, causes a collapse of the wave function that is equivalent to the collapse caused by an actual measurement result. A double-slit interferometer is used as a theoretical test of their hypothesis of collapse. The logical justification of their theory in this situation is questionable.

(i) Their theory starts with the assumption of a perfect, nondestructive detector, something which is unrealizable within quantum theory. "Perfect" means that if the detector does not trigger, "the particle is determined to be in the other path," or more precisely, not in the path with the detector. "Nondestructive," in the context of their paper, means that if the detector does not trigger, there is no significant change in the amplitude of the wave function associated with the nontriggering of the detector (but there is a random-phase shift introduced by the detector). This combination of perfect and nondestructive results in zero detection probability downstream of the detector while there is still nonzero amplitude of the wave function. This represents a major departure from quantum theory in which detection probability is related to the squared amplitude of the wave function.

(ii) The use of loss of interference in an interferometer as an indicator of wave-function collapse is faulty because loss of interference is a necessary but not sufficient condition (result) of collapse. It is possible to destroy an interference pattern by introducing phase shifts—which has nothing to do with loss of amplitude in one of the channels. Their conclusion that the loss of interference caused by random phase shifting in their perfect detector is related to collapse as originally indicated (determination that the particle is in the other path) is not valid.

The concept of a perfect detector is one for which, when a particle is known to be incident, a nonfiring of the detector is sufficient to determine that the particle is in the other path. This was made clear early in their paper; it is essential to the situation they are addressing. One can imagine putting a 100% effective "subsequent" detector (SD_2) just downstream of the first detector (in their path II), and another one (SD_1) in the equivalent position in the other path (without a prior detector). In the case that the first detector (path II) does not trigger, then just downstream, SD_2 must have zero probability of detecting the particle: a detection by SD_2 would demonstrate imperfection in the first detector. And, in the other path, SD_1 has 100% probability of detecting the particle. This represents a direct empirical verification of the statement "the particle is determined to be in the other path."

This discrete shift in detection probability is the basis of the concept of collapse applied to wave functions associated with individual events. If their detector is perfect in this sense, then the assumption of nonzero amplitude of the wave function downstream of their "perfect" detector is incompatible with the zero probability of detection of a particle there—regardless of any statements made about the inner workings of the detector. If their detector is not perfect in this sense, their concept of perfect has changed from the sense in which they introduced it; in that case it would not be clear what they meant by "perfect," or what relevance that would have. A detector that does not affect detection probabilities when nontriggering is irrelevant to the problem originally

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posed. If there is not a significant shift in detection probabilities associated with the nontriggering of the first detector, no information is gained by the nontriggering, and there is no reason to consider collapse of the wave function.

Their redefinition of collapse in terms of loss of interference is not consistent with their original premises. They start out with the usual concept of collapse—associated with a discrete shift in detection probabilities in the two paths. They equate this with the necessary *but not sufficient* condition of loss of interference. Then they associate collapse with a detector that causes no change in wave-function amplitude downstream of the detector, but does induce random phase shifting sufficient to cause loss of interference. This loss of interference is clearly *not* associated with a shift of detection probabilities or collapse in the original sense, since they maintain that there is no significant change in wave-function amplitude downstream of their detector. It cannot in any way contribute to the determination that the particle is in the other path; the probability of finding the particle downstream of it is essentially the same as just upstream of it.

These criticisms and a discussion of the nature of the shift in detection probabilities associated with nondetection in a near-perfect detector will be presented elsewhere [3]. (It is argued that the shift in detection probabilities associated with nontriggering is of a purely epistemological nature.)

[1] Mikio Namiki and Saverio Pascazio, Phys. Rev. A **44**, 39 (1991).

[2] Mikio Namiki, Found. Phys. **18**, 29 (1988). See also Mikio Namiki, Yoshie Otake, and Hiroshi Soshi, Prog. Theor. Phys. **77**, 508 (1987); Shigeru Machida and Mikio Namiki, *ibid.* **63**, 1457 (1980).

[3] James R. Johnston (unpublished).