

# Response to Report by Kyrylo Shnizkho

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We received the report by one of the authors of Ref. [1], namely by Kyrylo Shnizkho. We are of course grateful for the time taken by the author to furnish an answer to our Comment. Nevertheless, unfortunately, it is our impression that the arguments put forward in their Report completely miss the point in most cases, adding further confusion to the table. First of all, as an example of the overall confusion, we can point out Figure 1 in their response. That figure does not concern the range of values contemplated in our Comment, which examines the case of  $C$  being of the same order or larger than  $N$ . Therefore, providing a Figure where  $C = 100$  and  $N = 500$ , clearly satisfying  $C \ll N$ , circumvents the problem, as that is not the range of values of our concern. Moreover, in some cases the author even resorts to promoting inaccurate statements in order to convince. For instance, the announcement that the range of validity of the theory is provided in the paragraph between Eq. (34) and Eq. (35) is simply a fallacy. Nowhere in the paper it is explicitly indicated that  $\Gamma$  cannot be of the same order as  $N$ , in other words  $\Gamma/N \approx 1$  is not acceptable, and only  $\Gamma/N \ll 1$  is acceptable for the analytical result to be valid. In addition, as it can be easily verified, nowhere in the paper it is indicated that  $C \ll N$  and  $A \ll N$  for the theory to be valid. These are the limits that concern us, which are not explicitly stated in the paper. The only statement in the paper regarding the range of validity of the theory, provided in the paragraph between Eq. (34) and Eq. (35), indicates  $0 \leq C$  and  $A \in \mathbb{R}$ . Clearly there is no upper limit on  $C$  or  $A$  imposed by the authors, thus allowing those parameters to take values where the analytic result brakes down. Furthermore, shouldn't  $C$  also be  $\in \mathbb{R}$ ? Or can  $C$  be complex? Even if taking the limit  $N \rightarrow \infty$  before taking the limit  $C \rightarrow \infty$  could automatically result in  $C < N$ , in such continuous limit  $C$  and  $N$  are evidently of the same order ( $C/N \approx 1$ ), and the exponential approximation  $(1 + C/N)^N \approx e^C$  cannot be used. Therefore the analytic result is not valid in such limit.

In brief, we believe the range of validity of the theory is completely unclear in the paper. Moreover, from private communications with the author it became apparent that the author himself was not fully aware that the analytic results were not valid for finite- $N$ . To illustrate that the theory brakes down for  $C < N$ ,  $C < N$  but of the same order, or  $C$  larger than  $N$ , we have performed numerical simulations of the geometric phase ( $\chi$ ) vs. polar angle ( $\theta$ ) for  $N = 500$  considering:

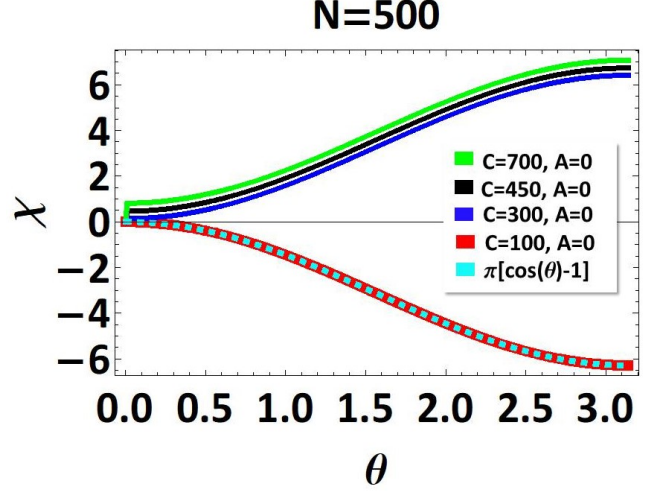


FIG. 1. Geometric Phase ( $\chi$ ) vs. polar angle ( $\theta$ ) for  $N = 500$  considering:  $C > N$ , with  $C = 700, A = 0$  (green curve),  $C < N$ , with  $C = 300, A = 0$  (blue curve),  $C < N$  but of the same order as  $N$  ( $C/N \approx 1$ ), with  $C = 450, A = 0$  (black curve),  $C \ll N$ , with  $C = 100, A = 0$  (red curve), and projective measurement  $\pi(\cos \theta - 1)$  (cyan dashed curve). Only for  $C \ll N$  the numerical and analytic results coincides with the predicted Pancharatnam phase. For  $C > N$ ,  $C < N$ , or  $C < N$  but of the same order as  $N$ , the analytic result cannot be used. The latter range of values corresponds to the limit  $N \rightarrow \infty$  and  $C \rightarrow \infty$ , namely  $C$  and  $N$  are of the same order ( $C/N \approx 1$ ), where the exponential approximation  $(1 + C/N)^N \approx e^C$  brakes down. The range of validity of the theory is not explicitly indicated in the paper. Further details are in the text.

$C > N$ , with  $C = 700, A = 0$  (green curve),  $C < N$  with  $C = 300, A = 0$  (blue curve),  $C < N$  but of the same order as  $N$  ( $C/N \approx 1$ ), with  $C = 450, A = 0$  (black curve),  $C \ll N$ , with  $C = 100, A = 0$  (red curve), and projective measurement  $\pi(\cos \theta - 1)$  (cyan dashed curve). Only for  $C \ll N$  the numerical and analytic results coincides with the predicted Pancharatnam phase. For  $C > N$ ,  $C < N$ , or  $C < N$  but of the same order as  $N$ , the analytic result cannot be used. The latter range of values corresponds to the limit  $N \rightarrow \infty$  and  $C \rightarrow \infty$ , namely  $C$  and  $N$  are of the same order ( $C/N \approx 1$ ), where the exponential approximation  $(1 + C/N)^N \approx e^C$  brakes down. Numerical results are displayed in Figure 1, clearly demonstrating that the analytic result brakes down for this range of values, which are well within the limits considered in the paper. This issue requires clarification: We believe that Eq. (38) and Eq. (39) in

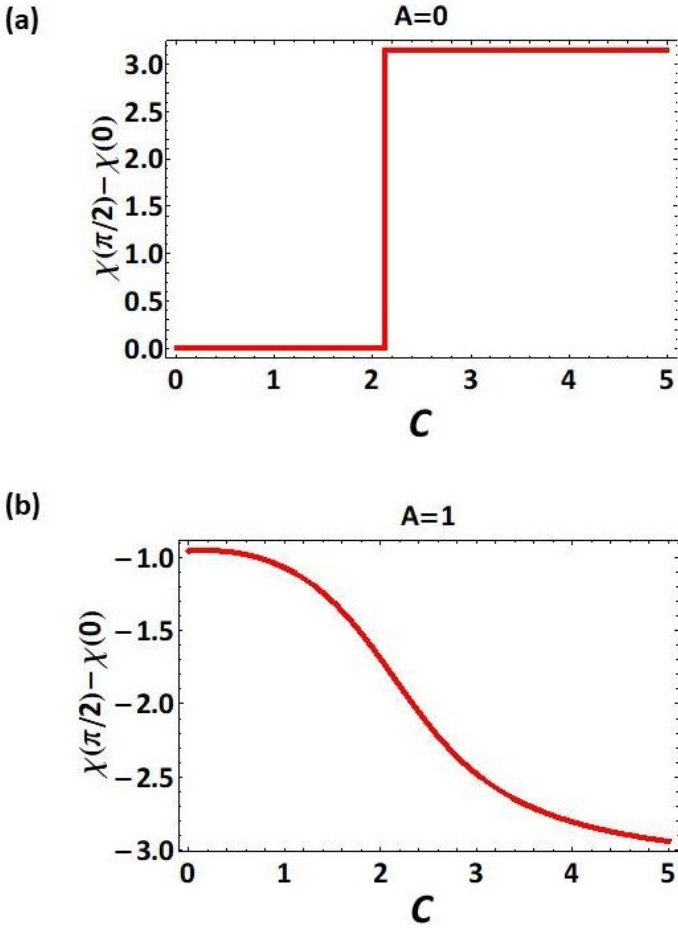


FIG. 2. Topological Invariant given by the difference  $\chi(\pi/2) - \chi(0)$  for: (a) non-Hermitian backaction  $A = 0$ , and (b) non-Hermitian backaction  $A = 1$ . Clearly, only for the case  $A = 0$  the topological invariant is quantized, and there is a discontinuous jump at a critical  $C$ -value, indicating the onset of a topological phase transition. For  $A \neq 0$  the claim of topological phase transition in Ref. [1] appears unsubstantiated. Further details are in the text.

Ref. [1] are not valid, since they are based in the analytic result which cannot be used for  $\Gamma/N \approx 1$ . Moreover, nowhere in the paper it is stated  $\Gamma/N \ll 1$  for the exponential approximation and the analytic result to be valid.

Second, the contradictions with Ref. [2]. It now became apparent that not only the claims in Ref. [1] and Ref. [2], by the same authors, are in contradiction, but also the definitions of topological invariants are in contradiction. In our opinion, these issues need to be clarified for the reader. In addition, even if the author is humble enough to accept their error, some of the arguments

they put forward are somewhat irrelevant, adding only further confusion to the table. This is also the case with the phrase copied from Ref. [2], in our opinion that phrase does not shed any light into the issue at hand. Moreover, we have performed numerical simulations considering the topological invariant introduced in previous publications, as acknowledged by the author. Namely,  $\chi(\pi/2) - \chi(0)$  vs. measurement strength  $C$ , both for the case  $A = 0$ , meaning no non-Hermitian backaction (as considered in Ref. [2]), and for the case  $A = 1$  (as considered in Ref. [1]). This is presented in Figure 2. The difference is striking: While for the case  $A = 0$  the topological invariant is quantized between  $\{0, \pi\}$ , and makes a discrete jump at a critical value of  $C$ , thus indicating a topological phase transition at such critical value, for the case  $A = 1$  the topological invariant is not quantized, nor does it make any jump at any critical  $C$ -value, therefore the claim of topological phase transition for  $A \neq 0$  is unsubstantiated. These discrepancies are quite serious and need attention. Even if by considering other topological invariants the authors can prove some topological behaviour, the signatures of topological protection should be apparent for *all* topological invariants put to the test.

Regarding the two remaining minor issues, the author again provides a response which in our opinion misses the point. Concerning the dynamical phase, we obviously agree with the author that there can be dynamical phases of multiple origins in any given setup. That was not our point. We only pointed out that, in contrast to the “surprising” dynamical phase introduced in Ref. [1], we believe it is *not surprising at all* to find dynamical phases in any given setup, and we backed up our statement by simply citing Ref. [2], where the authors themselves readily introduce a dynamical phase by hand. Next, concerning the experimental implementation, the examples of experimental devices provided by the author are certainly very helpful and they should be included in the actual paper. Nevertheless, we would like to point out that in both examples the parameters  $C$  and  $A$  are controlled by the same device, therefore they are not independent, fully supporting our claim on this matter. Finally, concerning the experimental results reported in Ref. [3] and Ref. [4], while these experiments are of course laudable, we would like to point out that all such experiments strongly require the non-Hermitian backaction  $A = 0$ , therefore they do not add any experimental proof of the validity of the theory for  $A \neq 0$ , as reported in Ref. [1]. More specific, all experimental results reported are fully contemplated within the theory originally presented in Ref. [2], therefore they do not serve as experimental evidence supporting Ref. [1], which is the main object of criticism in our Comment.

[1] Kyrylo Snizhko, Nihal Rao, Parveen Kumar, and Yuval Gefen, *Weak-measurement-induced phases and dephasing:*

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