RESPONSE TO REFEREE #1

> Unphysical maps are encountered in various situations: e.g. as entanglement witnesses, in quantum error correction or as reduced dynamics of correlated systems. In such cases, it is of importance to assess the non-physicality of a given map and the possibility of its approximation by quantum channels, or some subset of these. The paper provides a convenient framework, using quite naturally the ideas comming from convex resource theories, where the robustness measures are studied. The measures can be computed by SDP, which leads to some rather easily obtainable lower and upper bounds on these quantities. The relation to the diamond norm provides further insight to the structure of the measures, but also the diamond norm itself. The framework can be also readily adapted to situations where only a subset of physical maps can be used for the simulation, which may be closer to practical applications.
>
> The authors also propose an interesting simulation strategy, where the unphysical map is represented as an application of a channel on the input state tensored with a Hermitian operator of unit trace. The simulation cost, expressed as the smallest trace norm of the Hermitian operator in such a representation, turns out to be the same as in the standard approach, but the present scheme has the advantage that only the input states are changed in each round, while the channel remains fixed. This is a simple but clever idea, that might by also of a practical importance.
>
> The paper is quite well written, the presentation is clear and readable. The background and main results are well explained, references to related literature relevant and adequate.
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> This is a well written paper on interesting applications of robustness measures. I did not find any mistakes or typos.

We thank the Referee for their evaluation of our manuscript.

> As a comment:
> I think that it is a well known fact that the base norm given by the set of channels within the generated subspace is the diamond norm. That is precisely why the diamond norm measures distinguishability of channels, in the same way that the trace norm determines distinguishability of states.

We do realise that these ideas are certainly not extremely surprising to readers familiar with base norms. However, it appears that this was part of the "quantum information folklore" that might seem obvious in hindsight, but was not actually explicitly proved or addressed in other works (except in a general framework of Jenčová in Ref. [37], which we acknowledge). The recent work of Ref. [33] (Jiang et al., arXiv:2012.10959) provides evidence that this fact might not have been as well known as the Referee suggests: the authors of that work spend a large part of the paper rederiving properties of the base norm for trace-preserving maps, not realising that the quantity they defined is simply the diamond norm.

Our aim was to clarify the different base-norm–like quantities in the space of maps which are not completely positive and not even trace preserving, including their relations with the different robustness measures, and also the connection with the diamond norm which is recovered only for trace-preserving maps. We have now clarified this motivation in Sec. 3.

RESPONSE TO REFEREE #2

> I liked the paper very much. It contains a plethora of (mostly non-trivial) results related to quantification of non-CP character of quantum maps. The problem itself is well motivated, due to possible applications in quantification on non-markovianity and error mitigation. Moreover, the paper is generally well-structured and, thanks to the thorough discussion of the relevant literature, can possibly serve as a reference for the researchers working in this field. Below I listed for completeness the main results of the paper
>
> - Relation between diamond norm and non-CP quantifiers based on robustness
> - New operational interpretation of standard robustness against CPTNI maps in terms of the optimal state negativity in the auxiliary register used to simulate a quantum map
> - Relation between robustness of non-CP character of a quantum map and advantage in quantum games (this part follows straightforwardly form the general methodology that relates robustness=based quantifier and quantum games (see e.g. [24])
> - A family of upper and lower bounds bounds on the diamond norm, base norm of CPTNI maps, and robustness measures, via properties of Choi-Jamiolkowski operators associated with the map.
> - Discussion of the diamond norm as the quantifier of non-Markovian character of quantum evolution (when diamond norm is applied to non CP- divisible invertible channels) and to quantify resource costs of error mitigation (diamond norm applied to the inverse of a invertible quantum channel)
> - Computation of the diamond norms for a number of examples: depolarizing channel, generalized dephasing channel, and amplitude damping noise
>
> These results, when taken together constitute a very nice paper. I recommend acceptance after my comments and questions are addressed.

We thank the Referee for the careful reading of our manuscript and for their kind words about the paper.

We are also grateful for the detailed comments, all of which we address below.

> Comments/Question:
>
> My main question concerns the conceptual aspects of using robustness against CPTNI maps to quantify non CP-character of quantum maps. We know that general CPTNI maps correspond to quantum processes that happen only probabilistically. I find it weird that the authors chose this class as the quantifier, especially in the context of quantum error mitigation. The authors should comment how the probabilistic nature of CPTNI maps affect the "actual" physical cost of implementation of such transformation.

Let us first address some technical points here. Our original motivation for considering CPTNI maps is simply that we wanted our measures to be able to quantify the non-quantumness of general, non–trace-preserving processes — several measures which are defined by optimising only over CPTP channels are not applicable to such maps, as we discuss in Sec. 2. Importantly, however, when the given non-physical map Φ is trace-preserving itself, then it *does* suffice to consider measures defined with respect to CPTP maps, and all of our measures reduce to quantities defined in this way: this is the content of our Theorem 3.

What this means is that, in physical setting of interest where trace-preserving maps are typically employed — for instance, in describing the inverses Θ^{-1} of quantum channels in error mitigation

— no optimisation over CPTNI maps is required, and the "weirdness" pointed out by the Referee disappears. Most of our discussion concerning error mitigation specifically refers to such maps, and indeed the standard quasiprobability methods of Ref. [17] and similar are usually applied to trace-preserving maps. On the other hand, when a given map is not trace preserving, then we can still apply our Theorem 4, which provides a new method of simulating the map by using only quasiprobability distributions defined over quantum states. In such cases, the use of CPTNI maps cannot be avoided, and additional implementation cost may be incurred due to the probabilistic nature of the CPTNI maps. However, this comes from the non-trace preserving property of the given channel and not from its non-CP character, whereas the robustness measure quantifies the overhead cost from the latter contribution.

> The result concerning the new perspective on simulation cost of a non-CP map is not explained clearly in the abstract. The same remark concerns the introduction. Please clarify.

We have now expanded the explanation. However, it is difficult to talk about the specifics of this result without introducing much of the technical machinery that is given in Sections 2 and 3. We would prefer to avoid overcomplicating the abstract and introduction with any more technical details — we believe that the current presentation suffices to give the readers an overview of the main idea.

> Page 3 - when introducing the robustness for states please add that the set of free states F is assumed to be convex

Added.

> Page 3 : please add more detailed discussion about robustness measures for cones below Eq. 3

Added.

> Page 3 - please provide example of Herm -preserving map that cannot be written as a weighted sum of CPTP maps

We have clarified that it suffices to take any map such that $\mathrm{Tr}_B J_\Phi$ is not proportional to identity.

> Top of page 4 - please explain in detail why R(Phi) is an upper bound for all other quantifiers

Clarified.

> therto - a typo below eq 7

We wrote 'thereto', which is the standard spelling of the word; we are not sure if the Referee had something else in mind here.

> Please elaborate on the relation of Lemma 1 with [36]

We have clarified that [37] (previously [36]) provides an alternative way to derive parts of the proof of Lemma 1 (although the actual statement of our Lemma 1 does not appear in [37]).

> Please elaborate on the full-rank assumption below eq 11

We have clarified the assumption by explicitly restricting the optimisation to full-rank states in the proof.

> The authors claim that results of Lemma 1 offer simplification over [37,38]. Simplification in what sense- conceptual, computational?

We believe that both of these senses apply, which we have now clarified in the manuscript. The conceptual simplification is due to the fact that the optimisation is now expressed in terms of decompositions of Φ into two maps as $J_\Phi = M_+ - M_-$, which leads to a conceptually simpler form than the original SDP of Watrous. There is also a computational improvement (simply due to the lower number of variables to be optimised over), but we do not make any precise claims regarding the extent of the computational speedup of this form of the optimisation program.

> Corollary 2 should be given a more detailed explanation

We have now added a proof of the Corollary.

> Please reformulate Theorem 3- Eq. 15 does not follow directly from 14. It is not a difficult statement to prove but the authors use one paragraph in the proof to justify it.

We have clarified the statement of Theorem 3.

> Page 6 - Reference [42] is concerned with estimating a single outcome probability no the measurement statistics, as the authors suggest

We have corrected the wording.

> Page 8 - please elaborate on how how Eq. 34 follows from Theorem 4

Clarified.

> Please clarify the meaning of last sentence on page 8.

We have expanded the clarification before Corollary 5.

> The proof of proposition 8 seem incomplete to me

We have clarified the proof now.

> Please give the proof of statements below Eq. 58. The phrase "t can be shown" is clearly not a proof.

We did not mean to give a full proof there, but only exhibit a simple example that can be computed numerically and compared with the robustness-based approach. We have clarified this.

> Page 14- Can $\Lambda_{s,0}^{-1}$ be unphysical even though $\Theta_{s,t}$ is physical? The author's narrative seems to exclude such a possibility but I am not fully convinced by this.

Thank you for bringing this to our attention. We have realised that there is indeed no reason to believe that, in general, the unphysicality of $\Lambda_{s,0}^{-1}$ is enough to certify the non-Markovianity of the given dynamics. We have removed these statements from our discussion.

> Page 14- typo in expression for $X = \lambda_i \rho_i$ (summation symbol omitted). This mistake appears also later in the text

Fixed.