Review 2

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Review of “Extension of the Alberti-Ulhmann criterion beyond qubit dichotomies”

for *Quantum*, completed on Nov 29, 2019

Author will not see the reviewer's name or the date completed.

Overview questions

only editors will see response

Overall rating

★★★★★

For this manuscript I recommend...

Revise and resubmit

Comments to editor

The paper presents a small contribution to an important problem of comparison and simulability of families of qubit states and measurements. It is not very involved mathematically, based on well-known facts and simple linear algebra, which, on the other hand, may be seen as an advantage. I am inclined to the opinion that the contribution is significant enough, but not entirely sure.

Open response questions

Note: if you prefer to submit a free-form review instead of filling in this form, simply reply to the invitation email with your report as an attachment. Due to Scholastica's limitations, please make sure that you send it from the same address at which you received the invitation. If that's not possible, you can email the review to info@quantum-journal.org . Otherwise, just write "ok" in the reply to this question and proceed to the rest of the form.

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ok

Summary: what are the main questions posed by the manuscript and how does it answer them?

intended for the author

Given two indexed families of quantum states, is there a quantum channel transforming one to the other? The Alberti-Uhlmann theorem gives a criterion for the case of pairs of qubit states, based solely on outcome probabilities of tests. The authors present some conditions under which this criterion can be extended to larger families of qubit states (the target states are sometimes allowed to be qutrits). A similar question is solved for qubit measurements. The results are then applied to semi device independent simulability of families of states and measurements, which can be described as the problem of certifying that some set of states (measurements) can be simulated by another set (measurement) of known dimension, based solely on the input-output correlations obtained by applying an unknown device to the latter set (measurement).

What is your assessment of the paper? If you recommend acceptance, make a case that this work does indeed make a significant technical or conceptual contribution to scholarship (including experimental methods and/or mathematical tools).

intended for the author

An extension of the Alberti-Uhlmann theorem to higher dimensions and/or larger families of states was studied by many authors from various perspectives, but the simple criterion is in general not sufficient already for qutrits or triples of states, and more complicated conditions have to be checked, which can become very difficult. The paper contributes to this study in the quite particular case of real qubit families. If the target states are also assumed to be qubits, this statement is rather straightforward: it can be seen from the fact that the linear span of a real family of qubits has dimension at most 2, which allows us to apply the Alberti-Uhlmann criterion to any pair that spans it and extend the resulting map over the rest. The rest of the results is, besides the decomposability properties of positive maps on qubits, based on simple linear algebra: namely the well known fact that a surjective map has a one-sided inverse. Nevertheless, to my knowledge, this extension has not been observed before and is an interesting contribution to the important problem of comparison and simulability for sets of quantum states and measurements.
However, I had some difficulties in reading the paper and a careful revision

is needed.

To what extent have you checked the technical correctness of the paper?

intended for the author

I have checked all the proofs and found them correct.

Comment on the presentation of the paper. Is it well written? Are the main results clearly laid out? Does the manuscript clearly describe assumptions and limitations? Is the literature review adequate?

intended for the author

The paper is not very well written. The main results are quite well explained, but the proofs are in a Supplemental material which uses a setting that is not explicitly related to the studied problem, and are, in my opinion, not as clear and efficient as they should be. There are also some confusing notations, repetitions, careless copy-pasting, etc., see the comments below. The authors could also make a stronger case for the significance of their results in the introduction. The bibliography seems adequate.

If the submission includes numerical or physical experiments, does it provide sufficient details such that they could be reproduced by readers? This includes for example source code, documentation, experimental data, experimental setup specifications, etc.

intended for the author

The paper is entirely theoretical, no experiments are included.

Suggested changes, corrections, and general comments.

intended for the author

1. I personally dislike the habit of just stating the theorems in the main text and banishing the proofs into a ''Supplemental material''. Without proofs, there are no theorems, and it can be annoying for the readers to go back and forth reading the statements and searching for the proofs. This can be justified for some really technical parts, but this is not the case here.

2. The setting of the Supplemental material is not clearly related to the problem studied in the main text. Better spend a short paragraph or two

to explain this. Also notations such as S^+ are not explained. I would suggest to put the results into the general setting in the main text and include the proofs, leaving perhaps some technicalities about linear maps for the SM.

3. The proofs should be made clearer and more efficient:
   For example, the result of Thm. 1 (proved in Lemmas 3 and 4) follows by a straightforward argument (described above). This argument is in fact used here, but it is not clear from the exposition of the two lemmas. Also, both Lemma 2 and Lemma 5 are based on simple facts about linear maps with including ranges, which form a large part of both proofs. I think this simplicity is one of the appealing aspects of the paper and the authors should emphasise it.

4. confusing notations: for example, the vectors p_y with entries indexed by states, and p_x with entries indexed by effects are designed to get confused (in fact, I think it should be p_y in Coro. 1).

5. Further remarks:

- p. 3, column 1, ''Also the convex hull...'' there is something wrong with this sentence
- on p.3, column 2, instead of showing that the free energy is monotone in the area of the range, the argument here seems to show that both the free energy and the area are monotone in the variable a
- p. 3, column 2: what is \mathcal E here?
- p. 6, column 2, paragraph before Lemma 2 (some copy-pasting?)
- proof of Lemma 2: M_0=M_1C ... M_i instead of S_i

Would you be willing to referee an updated version of this work before a final decision is made?

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Yes.

If this work is accepted, would you be willing to write a short Perspective (opinion piece similar to a viewpoint or editorial) based on your report? For examples, see: http://quantum-journal.org/category/Editorial,Perspective/

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No, I am sorry, but my time is limited.

How was your reviewing experience? Is there anything you would like us to improve?

only editors will see response

Fine.