Dear Referees,

We would like to thank you for your time and effort spent reviewing our paper, as well as for the comments that have helped us to improve our manuscript. Below, we present detailed responses to all of your comments and add an additional remark describing an additional correction to the manuscript that we implemented. We also provide an additional file with all the changes made to the manuscript marked in red. We believe that we have addressed all the issues raised by the Referees and we hope that our manuscript is now suitable for a publication in PRX Quantum.

Reply to the Referee A

I noticed the following typos:

- *Second column of the first page, first paragraph: “Since the processes that underline...”. Here “underline” should be “underlie”.*
- *Just below Eq. (140): “Again, for convenient going forward...”. Here, “convenient” should be “convenience”.*
- *On the left-hand side of Eq. (53) I believe $V(\rho1|\gamma1)$ should be written as $V(\rho1 \| \gamma1)$.*

Thank you for spotting these typos, we have now made the necessary corrections.

Reply to the Referee B

However, the paper in the present form is not very well written. While the basic ideas are clearly explained and the main results are well described, the actual proofs are quite difficult to read. This is partly due to the fact that the arguments are sometimes given in a confusing and descriptive way, and partly due to a relatively high number of mistakes. These are mostly just typos, but since most of the proofs consist of complicated manipulations, this makes reading exceedingly difficult.

Thank you for pointing that out. We have now carefully checked all our calculations and made the necessary corrections (see below for details).

Some of these issues are listed in the specific comments below. I suggest that the authors carefully read the manuscript, identifying also other places where some confusion may occur. Some further suggestions are added at the end, that the authors might find useful.

Some specific comments

- *p. 15, Eq. (70a): ϵ should be $\epsilon -2\delta$ (?)*

  Yes, fixed [currently it is Eq. (74a) on p. 16].

- *p. 15, Eq. (73): the assumption $\lambda>0$ is needed*

  Yes, fixed.

- *p. 15, paragraph above Eq. (74): $\gamma_\lambda(\rho\|\sigma)$: λ or x?*

  Yes, x, fixed.

- *p.16-17, Lemma 17: In the results in [106,107] and [108,109] used in the proof, the role of type-I and type-II errors are exchanged, this should reflect in the exchange $\rho\leftrightarrow \sigma$ in the expressions of the lemma (the bounds for λ seem to be given correctly).*

  Indeed the results [106-109] are given with the roles of the type-I and -II errors reversed, and a footnote to this effect has been added.

- *p. 17, Eq. (86): $D(\sigma,\rho)$ should be $D(\sigma\|\rho)$*

  Corrected [now it is Eq. (90)].

- *p.17: the arguments of the proof between the last paragraph of the left column, finishing the proof for $\lambda\in \mathcal R_R$ are quite unclear and confusing, in particular see the three points below.*

  - *Are the functions in Eq. (87) invertible? It is better to explain, prove or give a reference for this.*

    Not strictly inverses, but a slightly weaker notion of quasi-inverses, which just requires that they act as inverses on each other's domain/co-domain. We've added a note about this.

  - *p. 17, line under Eq. (89): the swapping of ρ and σ should also affect the bound in $\mathcal R_L$*

    Fixed.

  - *p.17, looking at Eq. (91), I would say that the inequalities are opposite…*

    No, we believe they are correct as written. Eq. (95a) [(91a) in the original manuscript] follows just from the inequality fg>id, and (95b) [(91b) in the original manuscript] requires gf>id and that f is monotone non-increasing.

- *p. 19, Lemma 19: As far as I can see, the limit values $\check{D}_{\pm\infty}$ were not specified. (In case I missed it: it is better to point to the place in the text where it is done)*

  This has now been included.

- *p.19, proof of Lemma 19: this is a bit confusing. Q_t were defined to be projections, but the test given in Eq. (106) is not a projection in general (unless Π is rank one).*

  We now explicitly deal with the issue of degeneracy in an added paragraph.

- *p. 33, paragraph below Eq. (A8): $A\le \Phi$ or $A\ge \Phi$?*

  Yes, it was backwards, fixed.

- *p.34, line below Eq. (A16): $y\in (0,\epsilon)$ or $y\in (\epsilon,1)$?*

  Yes, it was the wrong way, fixed.

- *p. 35, line below Eq. (A21): what is "our expression for ϵ"?*

  We are now more explicit, and flesh out that part of the proof more.

Some additional remarks and suggestions

p.3, last line above Sec. II.B: There are some conditions that characterize transformations between general quantum dichotomies (and more generally families of states), given in terms of expressions related to state discrimination, in the spirit of classical Blackwell and Le Cam theorems, e.g. [74], Prop. 5 (exact case) and (A. Jenčová, Comparison of quantum channels and quantum statistical experiments, 2016 IEEE International Symposium on Information Theory (ISIT), 2249 - 2253, IEEE Conference Publications, 2016), Thm. 4 (approximate case). Usefulness is not clear, however.

Since, indeed, various necessary and sufficient conditions have been found, we have replaced the sentence “As a consequence, necessary and sufficient conditions for transforming general quantum dichotomies are still missing.” with “For attempts to overcome this limitation, see, e.g., Refs. [74-76]”, where we cite the two suggested papers together with “Quantum majorization and a complete set of entropic conditions for quantum thermodynamics".

In the proof of Lemma 25, to show that the limit in eq. (B16) exists, one can use the following argument: by Lemma 23, there is some $C_0>$ such that the sequence $y_n:=nC_0+D_\alpha(\mathcal P_{\sigma^{\otimes n}}(\rho^{\otimes n})\|\sigma^{\otimes n})$ is nonnegative and eq. (B19) shows that it is subadditive: $y_{n+m}\le y_n+y_m$. By the Fekete Lemma, this implies that $\lim_n n^{-1}y_n=\inf_n n^{-1}y_n$ exists.

Thank you for this suggestion, we streamlined the proof using Fekete’s Lemma.

Reply to the Referee C

However, before I recommend the manuscript for publication in PRX Quantum, I would like to raise the following comments:

1. *In the introduction, the authors motivate the importance of studying quantum/classical dichotomies by considering distributions q1 and q2 as prior knowledge. However, it is not very clear from the context in which sense one pair of distributions (p1, q1) is more informative than another (p2, q2) with the additional requirement of the mapping between two distributions q1 and q2 of the prior knowledge. If q1 and q2 are already known, why do we need to consider the transformation between the two? It would be helpful for readers to if the authors*

could provide more explanation behind this logic with some intuitive examples (e.g., energy distribution as discussed in the current version of the manuscript).

We clarified this passage in the introduction. We hope that now the motivation behind studying dichotomies becomes more apparent.

2. *The manuscript involves an extensive number of equations and technical details. While they seem necessary to comprehend the main results, the authors may consider providing a brief sketch of their proof techniques for better readability before going through the full details.*

   First, we have now added a brief Sec. IV.A that describes the physical intuition behind the derived results and we hope that should clarify them. Secondly, please note that the Derivations section is built up step by step, starting from Stein's lemma and building up through various deviation regimes, first for the hypothesis testing scenario, and then for the transformation rates, with each section starting with a clearly described goal. To improve the readability even further, we have now made a serious effort to correct all notational errors and typos that could have obscured our proof techniques.

3. *The resonance condition ξ=1 related to the gap between the zero-error case (Z) and first-order rate (R) seems to be an interesting finding. However, I wonder if the cases studied in the manuscript can go beyond a summary of previously known results. Is there any new intuition that can be learned from the authors' formalism? Also, such resonance in state transformation strongly depends on the initial and target state pairs and does not seem to have a particular structure. Is this always the case, or is there any general scenario for achieving the condition ξ=1?*

   Please let us note that the previously known results concerning the resonance phenomenon were limited to the small and moderate deviation regimes for incoherent states. As we explicitly state in the paper ("The results we presented in this paper, allow us to extend the resource resonance phenomenon in three novel ways…"), novel results consist of:

   a) Coherent resonance, where changing the level of coherence while keeping the distribution in the distinguished basis unchanged, can tune the state to the resonance.

   b) Work-assisted resonance, where one can avoid irreversibility by investing or extracting work.

   c) Strong resonance, where, based on the large deviation analysis, we show that the error is not only exponentially suppressed, but completely eliminated.

   Also, please note that the resonance condition has a very particular structure. Namely, it occurs for pairs of states with equal relative fluctuations of their generalised free energy content (where D(rho||sigma) measures the generalised free energy of rho with respect to sigma, and V(rho||sigma) its fluctuations). This should now be even clearer with the new section IV.A describing an intuitive phenomenological model for the resource transformation.

4. *The authors conjectured (Conjecture 10) that their bounds for a generic non-commuting pair of quantum states as a target might be saturated. However, considering that a non-commuting pair of initial states already results in additional contributions of coherence to the second-order asymptotics compared to the classical cases, it seems more natural to expect that a non-commuting pair of target states might lead to a similar effect. If possible, the authors may provide some reasoning behind why they believe their conjecture could be true.*

   Indeed, we do expect additional contributions arising from the coherence of the final state. That is why we do not claim that the rates will be expressed using D(p_2||q_2) and V(p_2||q_2), where p_2 and q_2 are probability distributions describing the classical information (i.e., the diagonal entries) about rho_2 and sigma_2. Instead, we conjecture that these additional contributions will be exactly captured by quantum relative entropy and its variance, D(rho_2||sigma_2) and V(rho_2||sigma_2). In other words, the expressions appearing in our bounds already accommodate for additional coherence terms, so that when we calculate the rates for a final state with coherence, they will have different values than for the corresponding decohered final state.

More minor comments are:

1. *In Eq. (14), defining a statistical distance between two cumulative distributions sounds a bit odd, and I recommend defining it between two probability distributions. This is because Eq. (15) already utilizes a proper distance measure (total variational distance) between two probability distributions. Also, it should be clearly indicated that A'(t) in Eq. (15) is the derivate of A(t).*

   Corrected.

2. *There are some typos and minor suggestions in the presentation style:*

   1) In Eq. (53), double bars should be used instead of a single bar for D and V.

   Corrected.

   2) When defining log odds in Eq. (74), it seems that λ and x are used inconsistently.

   Fixed.

   3) In the conclusion, "Interestingly, Refs. [109, 110] suggests … " seems to be "Interestingly, Refs. [109, 110] suggest … ."

   Corrected.

Additional remark

Besides the corrections suggested by the three reviewers, there was one more technical mistake that we independently identified and have now corrected. Specifically, the zero-error analysis contained an invalid step, and thus the claim that the achievability and optimality were tight in this regime was not correct. This error occurred in Equation 223 of the original manuscript (specifically going from a to b). As a result, the achievability and optimality bounds differ, which changes both Theorem 7 and Corollary 21. The statements and proofs of these two results, as well as all other parts of the paper that refer to them, have been altered to accommodate this.