Referee 3

We thank the referee for their consideration of our work and the useful and detailed comments and questions. Here we respond to their requested changes directly and outline how we have improved the manuscript accordingly.

1 Quantum synchronization (QS) is a timely subject and several original approaches and results have been reported in the last years. In this context, this manuscript complements previous work of some of the authors on dynamical symmetries, as in https://doi.org/10.1088/1367-2630/ab60f5, and on the Liouvillian formalism in QS, as in https://doi.org/10.1103/PhysRevA.95.043807. Looking at theorems and examples in comparison with these works, it is not always clear this work novelty.

Our previous work gave certain sufficient criteria for purely imaginary eigenvalues (limit cycles) of quantum Liouvillians and discussed how synchronization cases arise from strong dynamical symmetries in specific cases. Our present work gives both necessary and sufficient conditions for purely imaginary eigenvalues in addition to sufficient conditions for synchronization. Moreover, we show that in cases when the stationary state is invertible, the previous criteria of our Nature Communications 10 (1), 1730 (2019) are both necessary and sufficient. We now explain this in the main text.

2 After reading the whole manuscript it appears that it deals with QS of finite-dimensional systems, with interaction with the environment described by a specific class of master equations, not applicable in a master-slave scenario, nor in presence of significantly different subsystems, nor to phase synchronization. Actually the analysis is restricted to the case of identical subsystems (Sect 3.2) and to weak deviations from this scenario (perturbed Liouvillian, Sect 4).

The claim that we cannot treat subsystems that are not identical is incorrect. We now give an explicit example in Sec. 6.2.1 where the subsystems are not identical since each site is subject to a different on-site potential and interaction strength. We can treat any type of identical synchronization that results from time-independent master equations, including time-independent master-slave scenarios if such exist. We now discuss this specifically in the abstract. Generalizing to time-dependent master equations will be the topic of future work. Moreover, we can treat chaotic synchronization. This can occur if the purely imaginary eigenvalues become dense and incommensurate, as discussed in the manuscript.

3 The main change this manuscript will benefit from is a clear initial statement about the relevance/applicability of the described framework: the authors should identify (ideally avoiding a technical language) the systems and the kind of synchronization that can be formally described by the framework described in Sections 3 and 4. The abstract claim "[...] no comprehensive theory has been found [on quantum synchronization]. We give such a general theory" should be replaced by a fair statement on the specific reported results. See also 5-.

We have now amended the abstract to clarify that our theory applies to spontaneous (or time-independent) synchronization between undriven subsystems. In our introduction to the Lindblad formalism, we have also clarified that there is no restriction as to whether the environment is Markovian or non-Markovian. Importantly, we now note that our theory allows for a full Hamiltonian treatment of an environment, as well as a Markovian one. One example we give is explicitly non-Markovian, i.e. the three spin model for which one of the spins play the role of the environment for the other two.

4 Section 1.1.1 should focus on the relevant synchronization context and properly refer to it. For instance, the authors claim that ref 123 (well-known book on synchronization) considers "synchronization to be a purely periodic phenomenon" when there is even a dedicated chapter on synchronization in chaotic systems. It is also not correct that "In contrast to identical synchronization, phase synchronization does require periodic motion in order to meaningfully define a relative phase difference between the two subsystems." This is not the case, see for instance `https://doi.org/10.1103/PhysRevLett.81.321` **On the other hand, the authors long discussion about getting identical synchronization by rescaling different system observables is not really insightful.**

Section 1.1.1 has now been streamlined to remove less relevant discussions, and we have corrected our misinterpretation of the introduction of Ref 123 as pointed out by the referee. We have also studied the interesting reference they suggest. However, we note that this work does not refer to phase-synchronization as we describe in the introduction. The suggested work describes synchronization between a physical angle within each subsystem (which the authors label as a phase) rather than temporal phase differences between given variables. It is unfortunate that the literature has identical terms for different phenomena (and equally multiple terms for the same phenomenon), but we have tried to be as clear as possible when describing the phenomena we consider. With appropriate modification, our theory could treat a temporal phase difference as in the reference. We now comment on this when we discuss future work.

5 The scenario of metastability of section 4 is a form of known transient synchronization `http://dx.doi.org/https://doi.org/10.1007/978-3-030-31146-9_6` **or Ref 43, but no clear connection is discussed.**

We have now included this reference and pointed out that this phenomenon has been studied under an alternative name.

6 Complete synchronization has been already proposed in `https://doi.org/10.1038/s41598-019-56468-x` **This previous work should be acknowledged when presenting this concept.**

We have now included this reference.

7 Referring to 36 and 37, the authors claim that synchronization between spin $\frac{1}{2}$ systems is possible because of non-Markovianity. On the other hand, many works also cited in this manuscript deal with synchronization between spins $\frac{1}{2}$ systems in the Markovian case. Do the authors framework predicts that no stable synchronization can be achieved there? What would "fail" if the authors considered a system of only 2 instead of 3 identical units and a common bath under Markovian dissipation? Mentioning/discussing cases where this formalism cannot be applied while synchronization has been reported would be as useful as the already included examples, adding strength to this work.

Our theory can be applied to show that there is no stable synchronization in several spin-1/2 systems in the Markovian case. This may be accomplished using Th. 4, under the mild assumption of having a full rank stationary state. As we now discuss in Sec. 5, we may employ the construction of Prosen in Ref. [169]. This is applicable to the system in Ref. [42], which is also consistent with their claim that there is no stable synchronization in the system.

There is nothing in the formalism that would 'fail' in the case of two spin-1/2 units with a common Markovian bath. For instance if we take the Hamiltonian

$$H = \sigma_A^x \sigma_B^x,$$

with a single Lindblad operator

$$L = \sigma_A^- \sigma_B^-,$$

we find a pair of purely imaginary eigenmodes

$$\rho^{\pm} = (|1,0\rangle \mp |0,1\rangle)(\langle 1,0| \pm \langle 0,1|),$$

which have eigenvalues $\lambda = \pm 2i$. Notice that under the exchange of subsystems, $A \leftrightarrow B$, the expected value of σ_j^z in either state is anti-symmetric. In the long time , for any choice of initial state, this leads to

$$\langle \sigma_A^z(t) \rangle \to \alpha + \beta \cos(2t - \theta),$$
$$\langle \sigma_B^z(t) \rangle \to \alpha - \beta \cos(2t - \theta),$$

for $\alpha, \beta, \theta \in \mathbb{R}$ determined by the initial state. This is almost anti-synchronization, but unfortunately the expected value of σ_j^z in the NESS manifold leads to $\alpha \neq 0$ which spoils the anti-synchronization (this is similar to the example in the manuscript). Clearly under a weaker definition of synchronization we could consider this system to be synchronized. The purpose of the example included in the manuscript was to demonstrate that our theory indicates how to easily construct a non-Markovian environment that does synchronize the two spin-1/2's, and was motivated by the claim of [37] that two spin-1/2's cannot be synchronised.

8 The classification presented in Section 1.1.1 is a bit misleading, mixing the form of synchronization (e.g. in phase or amplitude) with the systems configuration (autonomous vs driven). Also, the very first definition of synchronization in the abstract is not the generally accepted one. Same criticism for the definition in sect.3.2 "Recall, that the crucial feature of quantum synchronization is that the various parts of the subsystem lock into the same phase, frequency and amplitude." Synchronization is a broader phenomenon.

We have now made it more evident that we consider only spontaneous synchronization in undriven systems. The introduction and abstract have been amended accordingly. This should now avoid confusion around terminology. However, as we stated in the conclusion, going beyond spontaneous synchronization requires extending our theory to time-dependent Liouvillians, and we plan to study this in future work.

9 The physical ground of the strong coupling case described in Eq 21 should be commented and related to possible microscopic derivation. Also the assumption after eq 25 of $\mathcal{L}_1$ anticommuting with the permutation operator should be clarified.

The physical microscopic derivations for the strong coupling 'Zeno' limit are discussed in the cited references, and references therein. We now explicitly direct the reader to these works for further details, as such discussions are beyond the scope of our work. Here we take the limit as an assumption. We have explained in the text that this limit corresponds to a situation where the noise is the dominant contribution to the dynamics and point the reader to the experimental example in Sec. 6.3 as a situation where this limit is relevant.

We have now clarified that the anti-commutation between $\mathcal{L}_1$ and the permutation operator corresponds to anti-symmetric symmetry breaking (as per Referee 1's comments too).