Reviewers' comments:

Reviewer #2 (Remarks to the Author):

The authors study an exactly solvable quantum model of a three-level system, as an example of dissipative adaptation in the quantum regime. The analysis, its methods and conclusions seems to be straightforward and correct.

The authors state in the abstract, “We extend the dissipative adaptation phenomenon to the quantum realm”. In the introduction, they discuss England's perspective article in [1], and then say again, “here, we extend the concept of dissipative adaptation to the quantum realm”. Despite discussing an entirely classical framework, Ref. [1] nevertheless presents a system-independent, generic proposal in where many examples were proposed/discussed. These examples were mostly complex many-body systems with enormous degrees of freedom. This point is extremely important. While specific models with low DOFs will certainly have to fulfil these rules of transition (such as Crooks), self-adaptation/emergent type phenomena are much more meaningful when arising from a system with relatively large amount of complexity, where a system-specific analysis cannot be used to understand the behaviour of the system.

The analysis presented in this paper is interesting and valuable of its own accord, and the specialized nature of its analysis suggests that it is suitable for publication in a more specialized journal. However, I do think that:

1. The three-level system model is a rather simplistic example of dissipative adaptation, and this is especially apparent since a) everything studied in this work is fully analytically tractable, and b) it does not really require conceptual knowledge of thermodynamical behaviour, or Crook's theorem in the quantum regime in order to analyse it.

2. Moreover, the methods and results lack generality and applicability to other systems (importantly, many-body type systems as often pointed out in [1]), to warrant publication in Communications Physics. The main results, which are equalities on transition probabilities and expectation values of certain observables are so specific and hold only for the studied system, that one cannot draw other generic conclusions on how we can expect dissipative adaptation to emerge and be important in quantum many-body type systems.

Based on the above two points, I have trouble making a case that the results provide enough insight to move the field of "quantum dissipative adaptation" forward significantly. Therefore, I would suggest the authors modify the title and abstract to be much more specific, and add to the discussion (at least a little, or 1-2 examples) about where dissipative adaptation can emerge and be relevant in quantum many-body type systems, and submit to a more specialized/technical journal.

In general, I also found that the presentation of the paper can still be improved, with more structure to guide the reader, being clearer about the assumptions (for example why the choosing of initial condition before eq.18 is significant) and notations. I give some examples where there are potential points of confusion while reading:
- H_S in equation 5 is defined only much later (as the Hamiltonian of the system), but should be done here instead. H could have stood for entropy which is another ubiquitous quantity in thermodynamics, so it's important to always define the central quantities when they are introduced.
- There were some parts in the reading where I got confused, for example in Eq. 28, entropy S(t)is denoted with respect to time t, but then later one plots S(p) for transition rates, and S(<Q_diss>). I later found that this was a clash of notation due to Eq. 6 vs Eq. 28, but decide to point this out anyway since it can be rather confusing for a reader.

Reviewer #3 (Remarks to the Author):

The major claim of this paper is the extension of the concept of dissipative adaptation to the quantum realm, by demonstrating it for a genuinely quantum, though simple, scenario. More precisely, a three-level lambda system driven by a single-photon pulse is studied. Firstly, it is shown that, under certain conditions, the system exhibits a self-organizing dynamics and secondly that this dynamics maximizes work absorption and heat dissipation, as expected from the classical concept of dissipative adaptation. Finally, the entropy of the environment is studied, showing that dissipative adaptation is also associated with a maximization of the classical part of the environment's entropy.

To my knowledge, dissipative adaptation has not been investigated in quantum systems prior to this work. The results, though limited to a simple and very specific scenario, are interesting in the community of quantum thermodynamics because they pave the way to further explorations of the concept of dissipative adaptation in various quantum systems. This work encourages us to explore a different aspect of the thermodynamics quantum many-body systems. This could potentially also have wider applications, for instance, at the interface with quantum biology.

The calculations are sufficiently detailed in the Methods section to be reproduced. This article is clearly written and convincingly justifies its claims. However, I think one result regarding the amount of work absorbed by the system could be further discussed: In the monochromatic regime, with Γ_a = Γ_b, W_{abs} = 2hω_a, but this work is provided by a single-photon pulse, initially containing an energy hω_a. This result may therefore seem counter-intuitive at first glance, and, in my opinion, should be commented on.

I recommend this article for publication in Communications Physics.

Referee #1

The authors study a three-level system driven by a single-photon pulse at zero temperature and investigate the concept of dissipative adaptation in the quantum regime. The three-level system consists of an excited state e and two states of lower energy a and b, Figure 1. The authors calculate the transition probabilities $p_{a\to b}(\infty)$ and $p^*_{b\to a}(t)$, spontaneous emission rates Γ_a and Γ_b, and from the energy balance equations derive sound definitions for the work absorbed $\langle W_{abs}\rangle$ and heat dissipated $\langle Q_{diss}\rangle$. As a result, they find that $p_{a\to b}(\infty) \gg p^*_{b\to a}(\infty)$ and that this leads to a maximal increase in the classical contribution to the environment entropy. The authors conclude that these results are the starting point of a quantum thermodynamics of driven self-organization.

The results described in the manuscript are interesting and constitute a significant contribution to the subject of nanoscale thermodynamics and self-organization. I want to recommend the manuscript for publication after the following minor revisions are considered.

We thank the Referee for the interest in our work and for giving us opportunity to improve it on relevant points.

1. The thermodynamics analysis lacks an explicit discussion on the contribution of the interaction energy term H_I defined in Eq. (10) to the energy balance and the definition of heat and work rates. How is that possible? Why $\langle H_I(t)\rangle$ does not contribute to the heat dissipated?

   Curiously, <H_I> vanishes at resonance, indeed. In the previous version, we made a brief comment on that, just below Eq.22. Now, we have further discussed that point. The dissipated heat comes from a second-order contribution in the matter-field interaction (note that <Qabs> is proportional to Γ_a and Γ_b, both $\propto g^2$, so heat dissipation comes from the variance of H_I, not the average. Such a fact reveals the stochastic character of this energy exchange due to the spontaneous emission process).

2. Equation (14) suggests that the system follows a Markovian dynamics. Is this a condition imposed in the model or, it follows directly from the equations of motion? In general, the dynamics of quantum mechanical systems is non-Markovian due to convolution integrals arising from quantum correlations. The authors should expand on the assumptions that lead to Eq. (14). Is this result a consequence of the Weisskopf-Wigner approximation?

   We have now better clarified the assumptions that lead to Eq. 14 of the first version (Eqs. 13 and 14 of the revised version). It follows directly from the initial mixed state we chose in Eq. 7 of the first version (Eq. 6 of the revised version). Our motivation was to devise the closest possible analogy with the classical framework.

Indeed, the reduced dynamics of $\rho_S(t)$ is non-Markovian, even within the Weisskopf-Wigner approximation due to a non-negligible system-environment quantum correlation. The convolution integral expected by the Referee does appear in Eq. 36 of the first version (Eq. 35 of the revised version). We have now added Ref. [23], which characterizes non-Markovianity in a similar context.

3. The equality in Eq. (19) is misleading. I strongly suggest that the authors simply indicate in the line below Eq. (19) that $p_{a\longrightarrow b}(\infty) = 1$ when $\Gamma_a = \Gamma_b$.

   We have solved that issue, as suggested by the Referee.

4. How is the zero temperature assumption compatible with the dissipation and entropy change in the environment? Should one expect an increase in the local temperature in the environment during the dissipation process? Can one also expect that some of the results discussed here will also be valid for non-zero temperatures?

   Entropy production at extremely low temperatures is indeed a subtle theoretical topic. Our key idea here is that dissipation of heat is related with production of statistical mixture in the field modes (characterized by the informational entropy). The dynamics of our model does not lead to the thermalization of the environment, which always remains out of equilibrium. It is reasonable to think that an experimental realization of our predictions, by contrast, would bring about extra coupling mechanisms eventually able to thermalize the field modes at a temperature slightly above its initial value. We have added a comment on this point.

   We expect that, at non-zero (low) temperatures, the forward (a -> b) transition should still be favoured with respect to the backward (b -> a) transition probability, as long as work from an external drive promotes a -> e transitions. Such forward/backward asymmetry should be either transient or stationary, depending on the work source type (pulsed or continuous, respectively). We have added a comment on this point either.

Also, in line 220, right paragraph Schrödinger is misspelled.

We have corrected that typo.

Referee #2

The authors study an exactly solvable quantum model of a three-level system, as an example of dissipative adaptation in the quantum regime. The analysis, its methods and conclusions seem to be straightforward and correct.

The authors state in the abstract, "We extend the dissipative adaptation phenomenon to the quantum realm". In the introduction, they discuss England's perspective article in [1], and then say again, "here, we extend the concept of dissipative adaptation to the quantum realm". Despite discussing an entirely classical framework, Ref. [1] nevertheless presents a system-independent, generic proposal in where many examples were proposed/discussed. These examples were mostly complex many-body systems with enormous degrees of freedom. This point is extremely important. While specific models with low DOFs will certainly have to fulfil these rules of transition (such as Crooks), self-adaptation/emergent type phenomena are much more meaningful when arising from a system with relatively large amount of complexity, where a system-specific analysis cannot be used to understand the behaviour of the system.

We agree that the search for a generic, system-independent (encompassing complex many-body systems) extension of dissipative adaptation to the quantum realm is still an (extremely important) open problem. We did already acknowledge that viewpoint in the manuscript: "The quantum dissipative adaptation we have found can be regarded as a *proof of principle, in need of generalizations* towards many directions. (…) It may be the case that the quantum fluctuation theorems developed in the papers above (and in the references therein) present fruitful methods in *the search for a general quantum theory of dissipative adaptation*. In summary, we provide the *starting point* towards a quantum thermodynamics of driven self-organization". To that end, our work gives a concrete hint that this is indeed a very promising research line, because now we know that dissipative adaptation not only has broad applicability in classical regimes at finite temperatures, but also can *remain valid* in quantum mechanics at zero temperature, where the original framework was insufficient. Our underlying rationale is that, if dissipative adaptation is well established for a broad class of driven classical systems at high temperatures [1] *and can be found in a fully-quantum regime at zero temperature*, it should then be valid even more broadly, for generic large (composite) systems at intermediate temperatures, where classical and quantum effects can compete.

We emphasize, however, that our model has at least two aspects of generic value. We have now added a brief comment on each aspect in the new version of the manuscript. **(i)** The model shows that dissipative adaptation is robust to an incoherent drive, in the sense of randomness in the phase of the average oscillating force (the average electric field in a single-photon state is precisely zero at all times, in contrast with a coherent Glauber state (laser), which gives rise to a classical force on the quantum system). This may motivate further generalizations of the (system-independent) quantum fluctuation theorems that we have found in the literature (none of them has been devised to encompass single-photon pulses as work sources, for instance). **(ii)** Part of the complexity of huge classical systems (that makes them more meaningful for discussions on self-organization) is, counterintuitively, reminiscent of our model. Namely, among the huge amount of available "non-organized" states that the system could in principle occupy, only a single (rare) state is populated when the work absorption is maximal. Whereas for classical systems one needs a huge number of local energy minima for having those many possible long-lasting states (configurations) in the non-driven regime (justifying the interest in a huge number of DoF), in our quantum scenario there is an infinite number of (pure or mixed) combinations of the lowest energy states that are equally stationary (long lasting, well adapted) to the non-driven zero-temperature environment. In other words, among the infinitely many possible stable final states available for our three-level system to be found after being driven, it always ends up, nevertheless, in a single (rare) state once it has absorbed the maximum possible amount of work (regardless of the infinitely many possible initial states).

Finally, the value of developing general abstract ideas (as in [1]) within specific examples becomes clear, for instance, in [4], from which we quote: "It must be emphasized at the outset that the character of this signature may differ depending on the detailed physical rules of a system under study".

The analysis presented in this paper is interesting and valuable of its own accord, and the specialized nature of its analysis suggests that it is suitable for publication in a more specialized journal. However, I do think that:

1. The three-level system model is a rather simplistic example of dissipative adaptation, and this is especially apparent since a) everything studied in this work is fully analytically tractable, and b) it does not really require conceptual knowledge of thermodynamical behaviour, or Crook's theorem in the quantum regime in order to analyse it.

Indeed, our results do not strictly require Crooks microscopically reversible condition (though (i) the concept of first law of thermodynamics in the quantum regime is required, (ii) the asymmetry between forward and backward processes bares remarkable resemblance with the Crooks condition, and (iii) Crooks theorem in the general quantum regime is still an open debate in quantum thermodynamics). This means to us that the dissipative adaptation phenomenon cannot be simply regarded as a mere special case of Crooks theorem. Precisely because of this, it has been possible to find signatures of dissipative adaptation within the specific models in refs. [4] and [5], which go beyond Crooks theorem (both results published in high-impact broad audience journals). Simplicity was also a valuable resource in [4], where linearly coupled driven-dissipative classical harmonic oscillators have been considered and Langevin forces have been neglected.

2. Moreover, the methods and results lack generality and applicability to other systems

(importantly, many-body type systems as often pointed out in [1]), to warrant publication in Communications Physics. The main results, which are equalities on transition probabilities and expectation values of certain observables are so specific and hold only for the studied system, that one cannot draw other generic conclusions on how we can expect dissipative adaptation to emerge and be important in quantum many-body type systems.

We have the same vigorous curiosity as that of the Referee for a general quantum theory of dissipative adaptation. Our understanding is that, once formulated, it would be considered a breakthrough with implications in several research fields, similarly to those works published in Nature Nanotechnology [1] and in Nature Physics [12]. Our proof of principle encourages the searching for this general quantum theory of dissipative adaptation. We have added a discussion on how quantum dissipative adaptation can emerge and be important in quantum many-body type systems.

Based on the above two points, I have trouble making a case that the results provide enough insight to move the field of "quantum dissipative adaptation" forward significantly. Therefore, I would suggest the authors modify the title and abstract to be much more specific, and add to the discussion (at least a little, or 1-2 examples) about where dissipative adaptation can emerge and be relevant in quantum many-body type systems, and submit to a more specialized/technical journal.

We have added several examples of quantum many-body type systems where dissipative adaptation can emerge and be relevant.

In general, I also found that the presentation of the paper can still be improved, with more structure to guide the reader, being clearer about the assumptions (for example why the choosing of initial condition before eq.18 is significant) and notations. I give some examples where there are potential points of confusion while reading:

We have better justified our choice for the initial condition before Eq.18.

- H_S in equation 5 is defined only much later (as the Hamiltonian of the system), but should be done here instead. H could have stood for entropy which is another ubiquitous quantity in thermodynamics, so it's important to always define the central quantities when they are introduced.

We have properly defined H_S in Eq.5.

- There were some parts in the reading where I got confused, for example in Eq. 28, entropy S(t)is denoted with respect to time t, but then later one plots S(p) for transition rates, and

S(<Q_diss>). I later found that this was a clash of notation due to Eq. 6 vs Eq. 28, but decide to point this out anyway since it can be rather confusing for a reader.

We have clarified notation concerning entropy.

Referee #3

The major claim of this paper is the extension of the concept of dissipative adaptation to the quantum realm, by demonstrating it for a genuinely quantum, though simple, scenario. More precisely, a three-level lambda system driven by a single-photon pulse is studied. Firstly, it is shown that, under certain conditions, the system exhibits a self-organizing dynamics and secondly that this dynamics maximizes work absorption and heat dissipation, as expected from the classical concept of dissipative adaptation. Finally, the entropy of the environment is studied, showing that dissipative adaptation is also associated with a maximization of the classical part of the environment's entropy.

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We have added a comment on that point.

I recommend this article for publication in Communications Physics.

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

The authors provided satisfactory answers to my criticisms and have accordingly made modifications to the manuscript. Consequently, I want to recommend the revised version of the paper for publication in Communications Physics.

Reviewer #2 (Remarks to the Author):

I find the author's response and the extended discussion in the manuscript to have provided more clarity, with regard to the concerns I pointed out earlier. I have no additional objections for the paper to be published in Communication Physics.

Reviewer #3 (Remarks to the Author):

I feel that the points raised in the previous round of review have been satisfactorily addressed.

Additionally, I noticed the following minor issues in the modified parts of the article:
- The way the verb ”find” is used sometimes sounds strange to me, especially lines 408 and 417 where I would rather use ”express”. And lines 421-422, I would reformulate the sentence ”we have found the monotonic function ...”: it is not clear whether what has been found is the expression of the monotonic function or its monotonicity.
- line 420 ”at the Introduction”should be ”in the Introduction”
- line 463 the sentence is not clear, I think there is a ”so” missing: ”so as to give …”
- line 466 ”at the vicinities of” should be ”in the vicinity of”

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Lines 408 and 417: we have replaced "find" by "express";
Lines 421-422: we have reformulated the sentence. Two sentences now replace that one sentence in the previous version: the first sentence states that we have found the expression of the function, whereas the second sentence emphasizes the monotonicity of the function.

- line 420 "at the Introduction"should be "in the Introduction"
We have replaced "as mentioned at the Introduction" by "as mentioned earlier", since the Introduction has been altered as requested by the Editor.

- line 463 the sentence is not clear, I think there is a "so" missing: "so as to give …"
We have inserted the previously missing "so".

- line 466 "at the vicinities of" should be "in the vicinity of"
We have replaced "at the vicinities of" by "in the vicinity of".