nature portfolio

Peer Review File

Intrinsic Phononic Dressed States in a Nanomechanical System

Corresponding Author: Mr Mert Yuksel

This manuscript has been previously reviewed at another journal. This document only contains information relating to versions considered at Nature Physics.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
The authors have addressed my questions in the revised manuscript.
However, I still have some questions about the model of the TSL discussed in Lines 121-131 and shown in Fig. 1B. (1) How do the authors know the atomic structure in the cross section of the resonator? Are they measured experimentally or got from the previous works? (2) Why can they be described by a particle in a double-well potential? The authors should mention the previous works on this subject.

Reviewer #2

(Remarks to the Author)
We thank the authors for considering our comments, addressing our questions, and improving their manuscript. The heating by driving and the increase of level populations are now appropriately discussed. It is clear which parts are understood from a physics standpoint, and which are not. Figure 5 is convincing and our previous misunderstanding regarding the fitting procedure is cleared up.

Reference [13] and its full publication does not compromise the novelty of the current work. They were essentially simultaneous, and the TLS strong coupling was only in a minor role in that work.

Regarding "industry standard" and "time-domain" characterisation, we all know very well that we mean state tomography. These referees are experts in the field. You cannot argue against that this is industry standard.

By "Similar devices have been used before (in a different context)" we meant the authors' own work, especially [5]. There was no DC and it is not clear if the mechanical devices were DC-capable or otherwise essentially dissimilar. While the route on design and fab from those NEMS to the current ones may be nontrivial, we still think the technological progress is minor.

Whether or not we recommend publication in NPhys; we don't have a strong opinion. Our initial opinion of the impact at large or within the field remains unchanged. Thus, we again end up in the confusing situation that we have to compare the prestige of Nature with a sister journal. Sometimes it is a simple job, but the current sibling may be equally selective...? We have simply no idea. It seems the impact threshold of NPhys is decided by the community based on whatever historical mental images.

We have no further major comments, but some minor comments regarding figures, formatting, and mathematical details remain:

Line 237: How do the authors calculate C = 3500? We cannot find a value for k_e in the manuscript (should be added), but even if k_e=0, C<<3500 when using g, k_i, and y_TLS as defined in lines 202 and 235.

Fig 2: red markings are confusing, as they seem to correspond to 3 different things: an unperturbed TLS (2B), coupled NEMS-TLS (2C), and a specific TLS (2D,E).

Fig 2 subtext: 'mirrored […] about Delta_0.' The potential is mirrored horizontally, which is an abstract configuration space axis, not an energy.

Fig 4G: The fit from simulations (dashed lines) cover a parameter space of up to 600mK at -150dBm. Why do Fig 4H and 4I, on which this simulation is based, only cover a parameter space up to 500mK and -148dBm?

Line 922: When estimating the TLS host volume, the authors only consider surfaces and interfaces. This contradicts their previous statements in line 123 that TLS may be located in the bulk material.

Reviewer #3

(Remarks to the Author)

Reviewer #4

(Remarks to the Author)
The authors have improved their discussion on TLS stability, reproducibility, and cleared several points we raised in the first round of review. We are in principle in favor of supporting the publication of the manuscript in Nature Physics. That being said, there are still two major scientific issues that need to be addressed as we believe the responses made by the authors were incorrect.

1. Proper acknowledgement of relevant reference (Ref.13)
We thank the authors for clarifying the timeline of the current work and Ref. 13. We still think it is important to mention this relevant work as an endnote ("During the preparation of this work, we became aware of a related work….") instead of briefly mentioning the reference with other less relevant references. While it is true that Ref. 13's focus is not on thorough characterization of mechanically coupled TLSs, the reference indeed demonstrates clear signature of mechanically coupled TLSs so it is of high relevance to the current work. We do not agree with the authors assessment of the ambiguity of TLS location in Ref. 13 because the TLSs are probed through the mechanical resonance so they must be located inside the mechanical resonator (instead of the much larger superconducting qubit). The large superconducting qubit in Ref. 13 was used to probe the mechanical resonance and has no role in the coupling to the TLS.

2. TLS tuning mechanism (strain vs electric field)
The authors mentioned that the TLSs can not be observed by directly probing using an AC electric field and thus conclude the TLSs are more strain sensitive instead of electric sensitive. We do not agree with this argument because the authors are using a microwave transmission line (as performed in this work) to directly probe microwave strong-coupled TLS. Such an attempt is fundamentally impossible due to the low cooperativity between microwave mode and TLS without a microwave resonance structure. Not observing electrically coupled TLS in the current experimental configuration is therefore expected and can not rule out the possibility of static electric tuning of TLS. We ask the authors to further revise the manuscript to clarify this point.

Reviewer #5

(Remarks to the Author)

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)
The authors have addressed my questions in the revised manuscript. I support the publication of the manuscript in Nature Physics.

Reviewer #2

(Remarks to the Author)
We are basically satisfied, with something small which you may address.

The detail on presenting the value of cooperativity on lines 247-248 is just very confusing. We finally got the point that damping rates for n_eff ~ 1 are used for computing C. However, with the values ki=400kHz, gamma_tls = 200 kHz quoted immediately above on line 245, C << 2800. A simple treatment is just to add a "disclaimer".

There is a separate but related complaint which arises now that we fully understood the situation. You don't need to address this unless the editors want. We don't fully like the practice of not choosing the "no-power" values where C would be much smaller, because in a quantum usage you are not interested typically a case with significant thermal population, if something else is available. If you ignore the thermal population, you could equally well choose values of arbitrarily high n_eff where C is probably even much bigger than the value you quote.

Reviewer #3

(Remarks to the Author)

Reviewer #4

(Remarks to the Author)
The authors have satisfactorily addressed all of my comments. The revised manuscript is significantly improved and meets the standards of Nature Physics. I have no further concerns and support publication of the manuscript.

Reviewer #5

(Remarks to the Author)
I am satisfied with the authors' responses and revisions.

Response to referees' comments for manuscript NPHYS-2025-09-03844-T

Dear Dr. Brierley,

We thank you and the reviewers for your constructive and helpful feedback on our revised manuscript. We appreciate the opportunity to address the remaining points, which we believe have allowed us to further clarify the significance and context of our work.

We have carefully addressed all comments, focusing particularly on improving the description/interpretation of the TLS model and the TLS tuning mechanism. Following your advice, we have revised and integrated a more explicit discussion of Ref. 13 directly into the introduction (lines 37-41).

Below, you will find the original reviewer comments in black and our point-by-point responses in blue. All changes in the revised manuscript have been highlighted in red for this revision while in olive for the previous revision.

Response to Reviewer #1

The authors have addressed my questions in the revised manuscript. However, I still have some questions about the model of the TSL discussed in Lines 121-131 and shown in Fig. 1B. (1) How do the authors know the atomic structure in the cross section of the resonator? Are they measured experimentally or got from the previous works? (2) Why can they be described by a particle in a double-well potential? The authors should mention the previous works on this subject.

We are pleased to learn that the reviewer finds our previous revisions satisfactory. We thank the reviewer for the remaining questions and address them below.

Atomic Structure and Microscopic Model: We clarify that the atomic structure depicted in the cross-section of Figure 1B is a conceptual schematic, not an experimental reconstruction. As the exact microscopic nature of TLSs remains an active area of research, this illustration is intended to visualize candidate locations where such defects are likely to reside, such as at surfaces, interfaces, or within the amorphous electrode material. We have updated the figure caption to explicitly state that this is a schematic representation.

Double-Well Potential Model: The description of a TLS as a particle in a double-well potential is based on the Standard Tunneling Model (STM), which is the widely accepted model describing atomic-scale defects. Since the 1970s, this has been the dominant theoretical framework for explaining low-temperature properties (such as specific heat and acoustic loss) in disordered systems [1]. While the model is a well-established theoretical concept, our work provides direct experimental evidence at the single-TLS level that this model accurately describes the acoustic TLSs we observe; in particular, the measured TLS frequency trajectory, $\omega_{\mathrm{TLS}}(V_{\mathrm{dc}})$, closely follows the hyperbolic dependence (Eq. 2) predicted by the double-well model. Following the reviewer's suggestion and to further clarify, we have added citations to the seminal works establishing the STM in line 132 (Refs. [28 - 31]).

Response to Reviewer #2

We thank the authors for considering our comments, addressing our questions, and improving their manuscript. The heating by driving and the increase of level populations are now appropriately discussed. It is clear which parts are understood from a physics standpoint, and which are not. Figure 5 is convincing and our previous misunderstanding regarding the fitting procedure is cleared up.

Reference [13] and its full publication does not compromise the novelty of the current work. They were essentially simultaneous, and the TLS strong coupling was only in a minor role in that work.

We are pleased to learn that our revision on drive-induced heating and the statistical analysis of random telegraph signals were satisfactory, and we again thank the reviewer for helping us improve the clarity and impact of the manuscript. We also appreciate the reviewer's positive assessment regarding the novelty of our work in the context of Reference [13].

Regarding "industry standard" and "time-domain" characterisation, we all know very well that we mean state tomography. These referees are experts in the field. You cannot argue against that this is industry standard.

We fully acknowledge and agree that state tomography and time-domain control are the gold standard for full state characterization in established quantum platforms. Our intention was not to argue this standard, but to respectfully clarify that our spectroscopic results *do* characterize the nonclassical behavior by observing the nonlinear energy spectrum of the Jaynes Cummings ladder. We have updated the text (lines 472 - 478) to reflect this more explicitly: "The spectroscopic signatures of this strong interaction serve as an unambiguous hallmark of the system's non-classical, quantum nature. This observation paves the way to future time-domain control, state tomography and the preparation of specific non-classical states; steps necessary to establish a fully quantum-capable device platform."

By "Similar devices have been used before (in a different context)" we meant the authors' own work, especially [5]. There was no DC and it is not clear if the mechanical devices were DC-capable or otherwise essentially dissimilar. While the route on design and fab from those NEMS to the current ones may be nontrivial, we still think the technological progress is minor.

We appreciate this clarification. While we continuously try to improve our procedures in each fabrication run, we confirm that the device fabrication is similar to our previous works (e.g., Ref. [5]). The observation of strong coupling was enabled by specific experimental conditions rather than fabrication changes alone. Since a similar question was raised by Reviewer 1, and recognizing the importance of this distinction, we have moved the explanation from the Supplementary Information to the main text in lines (205-212): "Compared to past studies of phononic crystal resonators [5,38,41], the observation of strong coupling was primarily enabled by two critical experimental conditions: 1) We probe the system at very low phonon numbers to avoid TLS saturation and decoherence while maintaining high signal-to-noise ratio and, 2) Crucially, we employ a tunable DC bias to frequency match individual TLS with the mechanical mode."

Regarding the reviewer's remark on technological progress: while the fabrication steps are similar to previous works, we emphasize that the *capability* to resolve single-phonon strong coupling and the Jaynes-Cummings ladder represents a distinct and major advancement in the performance and utility of these nanomechanical systems.

Whether or not we recommend publication in NPhys; we don't have a strong opinion. Our initial opinion of the impact at large or within the field remains unchanged. Thus, we again end up in the confusing situation that we have to compare the prestige of Nature with a sister journal. Sometimes it is a simple job, but the current sibling may be equally selective...? We have simply no idea. It seems the impact threshold of NPhys is decided by the community based on whatever historical mental images.

We have no further major comments, but some minor comments regarding figures, formatting, and mathematical details remain:

Line 237: How do the authors calculate C = 3500? We cannot find a value for κ_e in the manuscript (should be added), but even if $\kappa_e = 0$, $C << 3500$ when using $g, \kappa_i, and \gamma_{TLS}$ as defined in lines 202 and 235.

We estimate the single-phonon Cooperativity using the decay rate parameters extracted from the fit for the single-phonon regime where $n_{\text{eff}}, n_{\text{cav}} \approx 1$. These values are provided in Supplementary Table II, and we have revised the explanation of this choice for clarity in lines 852-854.

While checking these values again prompted by the reviewer's comment, we identified a calculation error in our previous draft. Using the standard definition $C = 4\text{g}^2/\kappa\gamma_{TLS}$(where $\kappa = \kappa_i + \kappa_e$) and the parameters from this regime, the corrected value is $C \approx 2800$ for TLS,C1 and 1600 for TLS,C2. The previous error resulted from using only κ_i rather than the total rate κ. We have updated the value in the manuscript accordingly, and we thank the reviewer for catching this.

Fig 2: red markings are confusing, as they seem to correspond to 3 different things: an unperturbed TLS (2B), coupled NEMS-TLS (2C), and a specific TLS (2D,E).

We agree that the red markings were used inconsistently in the original figure. We have revised Figure 2 to use color cues more coherently: the dashed red line is now uniquely associated with the ω_{TLS} trajectory, while blue represents the mechanical mode and purple (blue+red) denotes the coupled NEMS-TLS system.

Fig 2 subtext: 'mirrored [. . .] about Δ_0.' The potential is mirrored horizontally, which is an abstract configuration space axis, not an energy.

We thank the reviewer for this careful observation. We have revised the subtext to clarify that the mirroring refers to the configuration space of the double-well potential, removing the confusing reference to Δ_0.

Fig 4G: The fit from simulations (dashed lines) cover a parameter space of up to 600mK at -150dBm. Why do Fig 4H and 4I, on which this simulation is based, only cover a parameter space up to 500mK and -148dBm?

We thank the reviewer for pointing out this discrepancy. We have updated Figure 4G to align the simulation range with the experimental data shown in Figs. 4H and 4I (up to 500 mK). We have removed the extrapolation to 600 mK for consistency. At this higher temperature regime (T>500 mK), reliable estimation becomes difficult as it requires a larger Fock space to simulate and the observable splitting significantly reduces. We mention these details on this in Supplementary Note 3.

Line 922: When estimating the TLS host volume, the authors only consider surfaces and interfaces. This contradicts their previous statements in line 123 that TLS may be located in the bulk material.

We thank the reviewer for pointing out this contradiction. Our calculation in the Supplementary Note.6 focuses on the surface volume to capture what we anticipate to be the *dominant* contribution, as we expect a significantly higher density of disorder at the surfaces and interfaces, particularly given the fabrication processes involved. This approach is complementary to recent studies from our group [2] and others suggesting that in materials like ion-milled thin-film lithium niobate, the damaged surface layer is the primary location for TLSs and loss mechanisms. We do not imply the total absence of bulk TLSs, but rather that surface defects are the most likely source of the strongly coupled TLSs observed here. We have revised the text in the Supplementary Section line 933-935 to clarify this distinction.

Response to Reviewer #4

The authors have improved their discussion on TLS stability, reproducibility, and cleared several points we raised in the first round of review. We are in principle in favor of supporting the publication of the manuscript in Nature Physics. That being said, there are still two major scientific issues that need to be addressed as we believe the responses made by the authors were incorrect.

1. Proper acknowledgement of relevant reference (Ref.13) We thank the authors for clarifying the timeline of the current work and Ref. 13. We still think it is important to mention this relevant work as an endnote ("During the preparation of this work, we became aware of a related work. . . .") instead of briefly mentioning the reference with other less relevant references. While it is true that Ref. 13's focus is not on thorough characterization of mechanically coupled TLSs, the reference indeed demonstrates clear signature of mechanically coupled TLSs so it is of high relevance to the current work. We do not agree with the authors assessment of the ambiguity of TLS location in Ref. 13 because the TLSs are probed through the mechanical resonance so they must be located inside the mechanical resonator (instead of the much larger superconducting qubit). The large superconducting qubit in Ref. 13 was used to probe the mechanical resonance and has no role in the coupling to the TLS.

2. TLS tuning mechanism (strain vs electric field) The authors mentioned that the TLSs can not be observed by directly probing using an AC electric field and thus conclude the TLSs are more strain sensitive instead of electric sensitive. We do not agree with this argument because the authors are using a microwave transmission line (as performed in this work) to directly probe microwave strong-coupled TLS. Such an attempt is fundamentally impossible due to the low cooperativity between microwave mode and TLS without a microwave resonance structure. Not observing electrically coupled TLS in the current experimental configuration is therefore expected and can not rule out the possibility of static electric tuning of TLS. We ask the authors to further revise the manuscript to clarify this point.

We thank the reviewer for their positive assessment and for supporting the publication of our work. We appreciate the opportunity to clarify the two remaining points, which we address below.

1. **Proper Acknowledgement of Relevant Reference (Ref. 13):** Following the specific advice of the editor, we integrated the discussion of Ref. 13 directly into the introduction. We have revised the text (lines 37–41) to explicitly mention that a signature of coupling between a mechanical mode and TLSs was reported contemporaneously in Ref. 13. We have also changed our statement that "achieving strong coupling of a mechanical mode to an *intrinsic* TLS has not yet been achieved" to "achieving strong coupling of a mechanical mode to an *intrinsic* TLS has remained a significant challenge" in line 33.

2. **TLS Tuning Mechanism (Strain vs Electric Field):** We thank the reviewer for this insightful comment and agree with their assessment that we cannot definitively distinguish between strain and electrical tuning of the TLS in the current setup. We have revised the manuscript to correct this logic. Specifically, we have removed the claim that the tuning is dominated by the strain field based on the lack of electrical visibility. The revised text now simply states that the applied DC bias creates both electric and strain fields which modify the well asymmetry (lines 176-179), acknowledging that either or both mechanisms may contribute to the static tuning of the TLS frequency. Crucially, regardless of the specific mechanism, the effective tuning of the TLS frequency via the applied bias is clearly validated in the experimental data.

[1] W. A. Phillips, Two-level states in glasses, Reports on Progress in Physics **50**, 1657 (1987).

[2] R. G. Gruenke-Freudenstein, E. Szakiel, G. P. Multani, T. Makihara, A. G. Hayden, A. Khalatpour, E. A. Wollack, A. Akoto-Yeboah, S. Salmani-Rezaie, and A. H. Safavi-Naeini, Surface and bulk two-level-system losses in lithium niobate acoustic resonators, Physical Review Applied **23**, 064055 (2025).

Response to referees' comments for manuscript NPHYS-2025-09-03844A

Dear Dr. Brierley,

We thank you and the reviewers for your feedback on our revised manuscript and are pleased to receive overall satisfactory comments. As requested, we have addressed the final remark from Reviewer 2 regarding the presentation of cooperativity values.

Below, you will find the original reviewer' comments in black and our point-by-point responses in blue. All changes in the revised manuscript have been highlighted in red for this revision while the text from the previous revision has been reverted to black. Additionally, we have updated the manuscript formatting to comply with the submission requirements provided in the author checklist.

Response to Reviewer #2

We are basically satisfied, with something small which you may address.

The detail on presenting the value of cooperativity on lines 247-248 is just very confusing. We finally got the point that damping rates for neff 1 are used for computing C. However, with the values ki=400kHz, gammatls = 200 kHz quoted immediately above on line 245, C << 2800. A simple treatment is just to add a "disclaimer".

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We thank the reviewer for their careful and detailed analysis of our manuscript and constructive feedback throughout the review process, which has significantly improved our manuscript. We agree that the presentation in lines 246-249 was confusing. To resolve this, we have removed the sentence regarding the cooperativity calculation from the main text entirely, and include this discussion in "Simulation and Analysis" section which is now moved to Methods. We agree that the choice of different n_{eff} values leads to different cooperativity values. In agreement with the reviewer, we added the following clarification: "The cooperativity of the coupled NEMS-TLS system, defined as $C = \frac{4g^2}{\kappa\gamma_{\mathrm{TLS}}}$ (where $\kappa = \kappa_i + \kappa_e$), varies with drive power due to the power-dependence of the decay rates. To estimate the single-phonon cooperativity, we use the decay rates of the TLS and the mode for $n_{\mathrm{eff}}, n_{\mathrm{cav}} \approx 1$ from Extended Data Table 2. This corresponds to the parameters at a drive power of -142 dBm, where $\kappa_i/2\pi = 100$ kHz, $\kappa_e/2\pi = 24$ kHz, and $\gamma_{\mathrm{TLS}}/2\pi = 65$ kHz. Using these values, we calculate single-phonon cooperativities of $\sim$ 2800 for TLS,C1 and $\sim$ 1600 for TLS,C2."
