This manuscript has been previously reviewed at another Nature Research journal. This document only contains reviewer comments and rebuttal letters for versions considered at Communications Physics.

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

The authors have mostly satisfied my concerns and the paper comfortably meets the requirements to be published in Communications Physics. However, I still have some technical comments:

Regarding point 1 from my previous review: Is there any reason not to replace Fig. 4b by Fig. S8?

Regarding my point 2: I still find the way they have calibrated g_+ and temperature obscure. As I understand it, the steps are:
a) Measure a power spectral density at one value of \Phi, to produce a spectrum as in Fig. 3b.
b) Fit this spectrum, from which the product g_+^2 n_m^th is extracted.
c) Estimate g_+ using the geometrical device parameters, from which n_m^th follows.
d) Re-measure and re-fit the spectral density at different values of \Phi, and then extract g_+ by assuming that n_m^th is the value extracted in step c.

The problem with this approach is that the "extracted" values of g_+, plotted in Fig. 3c, in fact rely on the estimate in step c, which is independent of any experimental data!

I propose this alternative approach:
a) Fit each spectrum to extract a power ratio P_m/P_d.
b) Plot this ratio in Fig. 3c, keeping the linear fit to show that, as expected, the power ratio is proportional to \Phi.
c) Use the expected ratio g_+/\Phi, which comes from the device parameters, to convert the slope in (c) to a mode occupation n_m^th.
d) Express this as a mechanical temperature, with an uncertainty incorporating the statistical uncertainty in the fits and the geometrical uncertainty in the device parameters.

This will make it clear how well thermalised the device is, and also I think be easier for the reader to follow.

Reviewer #2 (Remarks to the Author):

The authors have addressed my concerns sufficiently and the manuscript could be published in its current form.

Reviewers' comments:

Reviewer #1 (Remarks to the Author):

The authors have mostly satisfied my concerns and the paper comfortably meets the requirements to be published in Communications Physics. However, I still have some technical comments:

We thank the reviewer for their valuable comments during the review process.

Regarding point 1 from my previous review: Is there any reason not to replace Fig. 4b by Fig. S8?

We have now included the results from Fig. S8 in the main manuscript.

The only reason to exclude the results from the 'semi-classical' model and to show them in the SI instead, was the availability of results from Master equation which resembles with experimental results closely.

Regarding my point 2: I still find the way they have calibrated g_+ and temperature obscure. As I understand it, the steps are:
a) Measure a power spectral density at one value of \Phi, to produce a spectrum as in Fig. 3b.
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This is not the correct interpretation.

The problem with this approach is that the "extracted" values of g_+, plotted in Fig. 3c, in fact rely on the estimate in step c, which is independent of any experimental data!

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This will make it clear how well thermalised the device is, and also I think be easier for the reader to follow.

We have indeed used the "alternative" approach proposed by the reviewer in analysing the results shown in Fig.3c. To arrive at the results shown in Fig. 3c, we,

1) plot $\frac{\kappa}{2}\sqrt{\frac{P_m}{P_d}}$ from the measurements taken at different flux-bias points.
2) fit with respect to flux and obtain the slope given by $\sqrt{n_m^{th}}\frac{g_+}{\Phi}$
3) use the estimated value of g_+/Φ from the device parameters and extract the average phonon occupancy.

We have elaborated this procedure in the revised version of the manuscript and SI.

Reviewer #2 (Remarks to the Author):

The authors have addressed my concerns sufficiently and the manuscript could be published in its current form.

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