

Emergent Harmonics in Josephson Tunnel Junctions Due to Series Inductance

Corresponding Author: Dr Max Hays

This file contains all reviewer reports in order by version, followed by all author rebuttals in order by version.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

The authors present a spectroscopic study of the effect of series inductance on the appearance of Josephson harmonics. They construct a modified tunable transmon with large Josephson junctions in a SQUID with significant loop inductance, then tune to near half flux where the primary Josephson effect is suppressed. They show spectroscopic features consistent with a second Josephson harmonic arising entirely due to the presence of the linear inductance, in contrast with some earlier results which attributed higher harmonics to processes in the junction itself.

The data is well fit by theory and the presentation is clear. I have only a few scientific questions:

1. In the supplement the authors point out that a second harmonic (or even fourth harmonic) is insufficient to explain data when the junctions have shunt inductances below ~ 10 fF, which is typical for transmons--in this case a more complete numerical treatment is used. They show no difference between a model with or without a fourth harmonic. Presumably there is a finite number of harmonics required to reach arbitrary accuracy. What do higher-order models imply? Would it be more straightforward to create an effective current-phase relation for the combined inductor and junction as a Taylor series and then truncate after a few terms?
2. The discrepancy between this result and prior results showing intrinsic Josephson harmonics could be explained by these devices having very large junctions. Do the authors have any data with small junctions? I realize that their current method is impractical in that case but perhaps more traditional transmon spectroscopy could pick up the difference? On a similar note, the intrinsic harmonic ratio could depend on junction size due to edge effects--it would help to see data with constant junction size and varying loop inductance. Still, this may be too much to ask for this study, as the data are quite convincing as currently presented.
3. During spectroscopic measurements the authors drive the resonator. This would cause an AC Stark shift on transmon transition frequencies. Can the authors bound this effect and verify that it is not significant?
4. The resonator spectroscopy in Fig. 3 shows significant structure which is well fit by the authors' model. However, there are many avoided crossings, while Eqn 6 is derived in a nondegenerate perturbative approach and thus would fail in these cases. Is numerical diagonalization used for the fits? The authors should explain this and comment on the extensive structure, which is quite striking.

Other minor comments:

-In the caption for figure 2, the reference to "left/middle, right" should be "left, middle/right", shouldn't it?

-In the second paragraph on page 5, the authors refer to a dispersive shift, but this is typically called the Lamb shift (with dispersive shift used to refer to the shift when the transmon is excited, not due to its presence in the first place). Also, kudos for not using the often-invalid rotating wave approximation!

-Supplement Figure S5: is it possible to plot both red and yellow lines so both are visible? At first I thought there was just a

mistake in the graph and the yellow line was omitted. Maybe dotted and dashed lines?

-Eqn S28: I assume Φ is the dimensionful version of φ , and likewise for Φ_L and φ_L --is this correct? It should be stated explicitly.

-During spectroscopic measurements

Reviewer #2

(Remarks to the Author)

The article shows how experimental signatures of higher harmonics of the Josephson effect in tunnel junctions can be explained by considering the series inductance. Their work indicates that previous results attributing the harmonics to Andreev processes are wrong. In their experiment, they measure and fit the spectra of SQUIDs with loops of non-negligible inductance. They show that the scaling of the harmonic ratio is fit by a simple theory of the effect of the series inductance, and that it contradicts the result expected for Andreev processes.

I support publication of their work. The role of inductance in distorting the current-phase relation and effectively introducing harmonics in the Josephson energy has been understood since at least 1972 [11]. I believe those results were overlooked in recent studies claiming that tunnel junctions are not opaque and that channels of relatively high transmission can be present. The authors offer the conventional, inductance based explanation and their scaling argument is very strong. A further "nail in the coffin" would be to measure the DC current-voltage characteristics of the tunnel junctions and show the lack of Multiple-Andreev Reflection.

Here are some comments to improve the manuscript:

- Emphasize in the introduction that the role of series inductance in distorting the current-phase relation is well established. Unfortunately it seems to have been forgotten. The authors cite [11], which is the original source, but this point has been discussed in detail in many textbooks since then, including Clark's book on SQUIDs, Likharev's book, Van Duzer, etc.
- In Fig. 2(b) I suggest plotting the classical prediction for the plasma frequency of a SQUID with non-negligible β_L (L/L_J). Except for the anti-crossings, it should describe quite well the overall dispersion in flux. This would connect their interpretation to prior work, and reinforce the authors argument: the appearance of harmonics is nothing new.
- In the supplementary material for [8] there is a discussion invalidating β_L as the source of Josephson harmonics in their junctions. The authors should address this point in their discussion, as they compare their results to that of [8]. Did the authors of [8] make a mistake in analyzing the role of β_L ? Are their junctions in a different regime? Can both interpretations be correct?
- In Eq. (7) it's a bit confusing to use β as the symbol to characterize the Andreev component of the harmonic, as β_L is usually used for the inductive correction.
- The authors should state earlier on in the manuscript that their junctions are very large. They should compare their junction parameters to those of [8-10]. How does β_L compare between all these experiments?

Reviewer #3

(Remarks to the Author)

The authors present an experimental study (supported by detailed theoretical modelling) with the goal to investigate the physical origin of higher harmonics in the Josephson relation of Al-AIOx tunnel junctions. The key idea is to probe the energy spectrum of nearly symmetric dc-SQUID devices close to half flux (where the main harmonic almost cancels), and vary the junction length. Arguing that the two hypothesised main origins for higher harmonics (Andreev physics versus bulk inductance) have a different power law with respect to junction length, the authors conclude, based on their data, that residual inductances are the main culprit. By interpolation of their data, they further estimate an upper bound for the impact of Andreev processes to account for less than 0.1% of higher harmonics. Thus, the authors conclusions oppose the one made by Ref. [8].

Overall, the manuscript is very well written, and highly accessible to a broad audience. The motivation, context, as well as methods/results and conclusions are clearly stated. The device setup, data analysis and theoretical modelling appear scientifically sound. The main conclusion (that Andreev processes are unlikely to account for the dominant source of higher harmonics in Al-based tunnel junctions) seems mostly plausible (see open questions below), and is, if accurate, of quite some significance to the field. This work provides an important antipole to the other recent and widely known work, Ref. [8], which advocated in favour of Andreev processes. I expect this work to further fuel the conversation on this critical aspect of superconducting devices. I am therefore inclined to think the manuscript to be aligned with the criteria of this journal, but there remain some open questions.

1) The conceptual proof of S2.A (the fact that the ratio of E_{J2A}/E_{J1} does not depend on junction geometry) is only of limited validity for the device, as it neglects the Fraunhofer effect. Given the nearly linear E_{J1} as a function of junction length (third column in Tab. 1), it is of course reasonable to assume Fraunhofer to be weak, but there seems to be at least some tendency of a sublinear behaviour of E_{J1} versus junction length (which I would expect when including Fraunhofer, due to the increased junction area). The point with respect to the second harmonic is that (based on the simple argument

that vector potentials coupling to the Josephson effect are effectively doubled for Cooper-pair cotunneling) I believe the second harmonic to have twice the Fraunhofer “frequency”, such that it will have a stronger reduction effect — or in other words, E_{J2A} might in all likelihood have a more pronounced sublinearity than E_{J1} with respect to junction length. If my thinking is right, this might somewhat relax the upper bound estimate of the Andreev contribution to E_{J2} . It could be, that this is a fairly weak effect, but I think it should be investigated to be on the safe side.

2) One of the important points for this work to be impactful, in my eyes, is that the series inductance extracted from fitting with the lumped element model matches with a microscopic explanation as to where the residual inductance is actually coming from. Indeed, the authors provide estimates based on an InductEx simulation of the loop inductance, and they find compelling overlap between fitting and induction simulation (all on the order of 10 pH). But they fail to specify what they mean with the words “loop inductance”, respectively, which part of the overall device geometry they actually included for that simulation. Let us consider the micrographs in Fig. 2 e) and f). Did they simulate the entire superconducting bulk (the entire pink area in Fig. 2e)), or only parts of it? I would presume that the large capacitor pads (e.g., the central pink cross in Fig. 2e)) can be neglected, and the bottleneck residual inductance comes from the Dolan-type bridge (the thin pink part at the bottom of 2e)) which then branches out into the two individual junctions. But which parts of the bridge are important — only the actual loop or the entire bridge part up until the pad? I think this important point should be clarified. After all, for maximum impact, the conclusions by the authors should, in my opinion, not depend on whether or not the SQUID loop is present.

Version 1:

Reviewer comments:

Reviewer #2

(Remarks to the Author)

Thank you for clearly addressing my concerns. I think your explanation for the origin of harmonic content is strengthened by the changes made to the manuscript and the supplementary material. As requested, you have also highlighted that your junctions are larger than in the other referenced experimental work. I concur with the analysis presented in Supp. Section 2B; authors of previous work proposing the Andreev harmonic explanation should revisit the possibility of the series inductance explanation, taking into account the issues you raise. Future experiments have a clear path to reconcile the two explanations or refute one of them. The simpler explanation is often better, if not as exciting. I fully support publication of the manuscript in Nature Physics.

Çağlar Girit

Reviewer #3

(Remarks to the Author)

The authors have in my point of view given satisfactory answers to the remarks and suggestions of all referee reports, including mine. I consider all results and conclusions made in this revised version of the manuscript as valid, reliable, and reproducible. And as already stated in my previous report, the results seem to be, in my point of view, of high accessibility and relevance to the physics community. In the light of that, I support publication of this manuscript.

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Editorial Staff
Nature Physics

Dear Dr. Brierley and Reviewers,

We thank the Editor and the Reviewers for considering our manuscript, "Emergent Harmonics in Josephson Tunnel Junctions Due to Series Inductance." We sincerely appreciate the reviewers' insightful and constructive comments. They highlighted important points that were not fully covered in the original draft, helping us to improve the quality and clarity of the manuscript.

Our point-by-point responses to the reviewers' comments are provided below. We believe that our detailed responses in this letter thoroughly address the questions and concerns of the reviewers.

Answers to questions can be found below in blue text.

Corresponding updates to the manuscript can be found in red text.

Reviewer #1 (Remarks to the Author):

The authors present a spectroscopic study of the effect of series inductance on the appearance of Josephson harmonics. They construct a modified tunable transmon with large Josephson junctions in a SQUID with significant loop inductance, then tune to near half flux where the primary Josephson effect is suppressed. They show spectroscopic features consistent with a second Josephson harmonic arising entirely due to the presence of the linear inductance, in contrast with some earlier results which attributed higher harmonics to processes in the junction itself.

The data is well fit by theory and the presentation is clear. I have only a few scientific questions:

We thank the reviewer for their time and for carefully reading our manuscript. We believe that we have addressed their suggestions and concerns, and that the manuscript has been improved thanks to reviewer's comments.

1. In the supplement the authors point out that a second harmonic (or even fourth harmonic) is insufficient to explain data when the junctions have shunt inductances below ~ 10 fF, which is typical for transmons--in this case a more complete numerical treatment is used. They show no difference between a model with or without a fourth harmonic.

Presumably there is a finite number of harmonics required to reach arbitrary accuracy. What do higher-order models imply? Would it be more straightforward to create an effective current-phase relation for the combined inductor and junction as a Taylor series and then truncate after a few terms?

In our work, we compared the exact full-circuit model and the inductance-induced harmonics model [Eq. (S21)], which is derived under the assumption of current conservation. When $E_J \ll E_L$, the harmonic correction in Eq. (S21) is often described by truncating at the second harmonic term $E_{J2,L}$ and further considering only its leading order in the Taylor expansion, i.e., $(-E_J/E_L)$.

As shown in Fig. S5, we observed a discrepancy between the full-circuit model (black) and this leading-order second harmonic model (red) for small junction capacitance. As the reviewer pointed out, we next checked whether this discrepancy is caused by the above truncation. The orange line shows the transition calculated by including harmonic terms up to $\cos(4\varphi)$ in the Hamiltonian, with each coefficient $E_{Jm,L}$ containing higher powers of (E_J/E_L) .

However, this addition did not change the transition from the red line, indicating that Eq.(S21) has already converged. In other words, the discrepancy is neither due to the omission of higher powers of E_J/E_L in the Taylor expansion of each harmonic coefficient, nor to neglecting higher harmonics $E_{Jm,L} \cos(m\varphi)$ for $m > 2$. Therefore, we concluded that the discrepancy actually arose from a more fundamental assumption of current conservation between the linear inductor and the Josephson junction in each branch, as suggested in Ref. [23] ([27] in the revised version). This method neglects the effect of the capacitance of the node between the junction and the inductor, resulting in the observed deviation between the harmonics model (less complete) and the full model (more complete).

2. The discrepancy between this result and prior results showing intrinsic Josephson harmonics could be explained by these devices having very large junctions. Do the authors have any data with small junctions? I realize that their current method is impractical in that case but perhaps more traditional transmon spectroscopy could pick up the difference? On a similar note, the intrinsic harmonic ratio could depend on junction size due to edge effects--it would help to see data with constant junction size and varying loop inductance. Still, this may be too much to ask for this study, as the data are quite convincing as currently presented.

To first answer the reviewer's question, we don't have data for small junctions.

In this work, our strategy was twofold. First, to maximize the contribution of the second harmonic, we used large junctions to increase both E_{J1} and E_{J2} , and then cancel out E_{J1} so that only E_{J2} remained large.

Perhaps more importantly, these large junctions significantly reduce the computational complexity of the analysis, by ensuring that the junction capacitance is large enough to directly apply Eq. (S21). As discussed in the supplement [Section S2B], small junctions require a more careful treatment using a full-circuit analysis with significantly increased Hilbert space dimension and more fit parameters.

Since we assumed the transparency distribution is the same, and given that the E_{J2}/E_{J1} ratio is well described by a linear function in our large-junction regime, the y-intercept extracted from this regime should be valid in the small junction limit with the same transparency distribution.

Still, as the reviewer points out, there could be edge effects in small junctions that change the transparency distribution. However, we hypothesize that such effects are more likely to lower the average transparency rather than increase it due to oxygen diffusion in from the junction boundary. In this situation, the intrinsic harmonic contribution would actually be further reduced. That being said, this is just speculation, and we now mention the possibility of edge effects in our conclusion:

"We also note that the intrinsic harmonics may be influenced by underlying details of the fabrication process, junction aging, or edge effects, which could be the subject of a future experiment."

The reviewer's suggestion of varying the SQUID loop size would indeed be a nice next step to further examine the contribution of inductance, but we agree that this is beyond the scope of the current study.

3. During spectroscopic measurements the authors drive the resonator. This would cause an AC Stark shift on transmon transition frequencies. Can the authors bound this effect and verify that it is not significant?

We thank the reviewer for pointing out an important factor to the analysis.

We've added a new section to the supplemental material [Section S4B] showing that the effect of the AC Stark shift is negligibly weak.

4. The resonator spectroscopy in Fig. 3 shows significant structure which is well fit by the authors' model. However, there are many avoided crossings, while Eqn 6 is derived in a nondegenerate perturbative approach and thus would fail in these cases. Is numerical diagonalization used for the fits? The authors should explain this and comment on the extensive structure, which is quite striking.

For the theoretical resonator spectroscopy in Fig. 3e and Fig. 3f (dashed magenta), we used Eq. (6), which is derived from the nondegenerate perturbation theory as the reviewer pointed out. We verified that the direct numerical diagonalization of the entire qubit-resonator system shows no difference from this perturbative approach within the measurement window. This implies that the perturbative approach can accurately predict the level crossing between the qubit and the resonator.

We've updated the supplemental material [Section S4A] to provide a detailed discussion on this question.

We've also added a sentence in the caption explaining the avoided crossing features:

"The avoided crossings appear where the device transition frequencies cross the resonator frequency."

Other minor comments:

-In the caption for figure 2, the reference to "left/middle, right" should be "left, middle/right", shouldn't it?

The double-well potential shown in the middle panel of Fig. 2c is the result of the second harmonic. Since the device is not biased right at half-flux, there occurs an asymmetry between the two wells. Therefore, it should be left (absence) / middle (presence), right (presence)

-In the second paragraph on page 5, the authors refer to a dispersive shift, but this is typically called the Lamb shift (with dispersive shift used to refer to the shift when the transmon is excited, not due to its presence in the first place). Also, kudos for not using the often-invalid rotating wave approximation!

We agree that the term dispersive shift is sometimes used too broadly; thanks for the kudos!

We've revised the manuscript to clarify that this is the shift of the resonator frequency when the device is in its ground state.

-Supplement Figure S5: is it possible to plot both red and yellow lines so both are visible? At first I thought there was just a mistake in the graph and the yellow line was omitted. Maybe dotted and dashed lines?

Thank you, this is a good idea.

We've updated the figure to explicitly show the overlap between red and yellow lines.

-Eqn S28: I assume Φ is the dimensionful version of φ , and likewise for Φ_L and φ_L --is this correct? It should be stated explicitly.

We've revised the manuscript to clarify the relation between Φ and φ .

"Here, $\varphi = 2\pi\Phi/\Phi_0$ and $\varphi_L = 2\pi\Phi_L/\Phi_0$ are the reduced flux variables across the shunt capacitor and the inductor, respectively."

Reviewer #2 (Remarks to the Author):

The article shows how experimental signatures of higher harmonics of the Josephson effect in tunnel junctions can be explained by considering the series inductance. Their work indicates that previous results attributing the harmonics to Andreev processes are wrong. In their experiment, they measure and fit the spectra of SQUIDs with loops of non-negligible inductance. They show that the scaling of the harmonic ratio is fit by a simple theory of the effect of the series inductance, and that it contradicts the result expected for Andreev

processes.

I support publication of their work. The role of inductance in distorting the current-phase relation and effectively introducing harmonics in the Josephson energy has been understood since at least 1972 [11]. I believe those results were overlooked in recent studies claiming that tunnel junctions are not opaque and that channels of relatively high transmission can be present. The authors offer the conventional, inductance based explanation and their scaling argument is very strong. A further "nail in the coffin" would be to measure the DC current-voltage characteristics of the tunnel junctions and show the lack of Multiple-Andreev Reflection.

We thank the reviewer for their positive recommendation and helpful suggestions for improvements to the manuscript. We hope our answers below have addressed their comments.

Here are some comments to improve the manuscript:

- Emphasize in the introduction that the role of series inductance in distorting the current-phase relation is well established. Unfortunately it seems to have been forgotten. The authors cite [11], which is the original source, but this point has been discussed in detail in many textbooks since then, including Clark's book on SQUIDs, Likharev's book, Van Duzer, etc.

We thank the reviewer for suggesting these relevant works; we now cite them in the introduction. We've updated the manuscript to include additional discussions on inductance-induced harmonics and to note that the role of inductance in current-phase relation is well established.

"Although the presence of inductance-induced harmonics in the current-phase relation has been well recognized [3, 7, 11, 20–24], their relative contributions compared to Andreev processes in tunnel junctions remain unexplored."

- In Fig. 2(b) I suggest plotting the classical prediction for the plasma frequency of a SQUID with non-negligible β_L (L/L_J). Except for the anti-crossings, it should describe quite well the overall dispersion in flux. This would connect their interpretation to prior work, and reinforce the authors argument: the appearance of harmonics is nothing new.

As suggested, we calculated the classical plasma frequency of the SQUID and found that it indeed captures the overall trend of the transition frequency obtained from the circuit-QED analysis.

We've added a brief paragraph about it in the supplement [Section S2D], and included a summary line referencing this discussion in the main text:

"We also note that the overall trend of this alternation in the intra-well transition can be predicted by the classical plasma frequency of the SQUID (See Supplemental Material for details)."

- In the supplementary material for [8] there is a discussion invalidating β_L as the source of Josephson harmonics in their junctions. The authors should address this point in their discussion, as they compare their results to that of [8]. Did the authors of [8] make a mistake in analyzing the role of β_L ? Are their junctions in a different regime? Can both interpretations be correct?

In Ref. [8], they discussed the effect of inductance by attempting to fit the data using the formulae in Eq. (S21). However, Eq. (S21) was derived by means of reducing the number of the node variables of the full circuit [Fig. S5a] assuming current conservation between the junction and the inductor, neglecting charging effects on the node between the junction and the inductor. However, as shown in Fig. S5b, Eq. (S21) does not agree with the direct numerical diagonalization of the full circuit when the junction capacitance (CJ) is small, the regime where Ref. [8]'s parameters fall. In this regime, diagonalizing the full circuit is needed to correctly predict the spectrum. Our junctions were much bigger, where the large junction cap ensures that Eq. (S21) agrees with the full-circuit diagonalization. In summary, our conclusion is that the inductance certainly distorts the current-phase relation, but that distortion should not be analyzed by Eq. (S21) in the low CJ regime. So, as we conclude at the end of Section S.II.B of the supplement, there are too many free parameters to make concrete conclusions about the source of the harmonics in the data of Ref. [8].

- In Eq. (7) it's a bit confusing to use β as the symbol to characterize the Andreev component of the harmonic, as β_L is usually used for the inductive correction.

We thank the reviewer for pointing out the notational conventions. We have changed β to γ to reduce confusion. In the revised version, γ denotes the Andreev contribution, β the inductance contribution, and α the sum of the two sources.

- The authors should state earlier on in the manuscript that their junctions are very large. They should compare their junction parameters to those of [8-10]. How does β_L compare between all these experiments?

We've added a note in the third paragraph stating that our junctions are large. Also, a sentence comparing E_{J1} and β_L with previous three works has been added into the discussion:

"We note that our junction E_{J1} is larger (80-250 GHz), compared with these recent works, where E_{J1} was ~ 20 GHz [8,10] and ~ 60 GHz [9], respectively.

The ratio between the fundamental harmonic and the inductive energy $\beta_L = E_{J1}/E_L$ is on the order of 1% in Refs. [9,10] and in the present work, whereas the estimated β_L in Ref. [8] was generally larger, on the order of 5%."

Reviewer #3 (Remarks to the Author):

The authors present an experimental study (supported by detailed theoretical modelling) with the goal to investigate the physical origin of higher harmonics in the Josephson relation of Al-AIOx tunnel junctions. The key idea is to probe the energy spectrum of nearly symmetric dc-SQUID devices close to half flux (where the main harmonic almost cancels), and vary the junction length. Arguing that the two hypothesised main origins for higher harmonics (Andreev physics versus bulk inductance) have a different power law with respect to junction length, the authors conclude, based on their data, that residual inductances are the main culprit. By interpolation of their data, they further estimate an upper bound for the impact of Andreev processes to account for less than 0.1% of higher harmonics. Thus, the authors conclusions oppose the one made by Ref. [8].

Overall, the manuscript is very well written, and highly accessible to a broad audience. The motivation, context, as well as methods/results and conclusions are clearly stated. The device setup, data analysis and theoretical modelling appear scientifically sound. The main conclusion (that Andreev processes are unlikely to account for the dominant source of higher harmonics in Al-based tunnel junctions) seems mostly plausible (see open questions below), and is, if accurate, of quite some significance to the field. This work provides an important antipole to the other recent and widely known work, Ref. [8], which advocated in favour of Andreev processes. I expect this work to further fuel the conversation on this critical aspect of superconducting devices. I am therefore inclined to think the manuscript to be aligned with the criteria of this journal, but there remain some open questions.

We thank the reviewer for the time they spent reading our manuscript, and are glad to hear that they find our work potentially suitable for publication. We hope the answers below have addressed their concerns and questions.

1) The conceptual proof of S2.A (the fact that the ratio of E_{J2A}/E_{J1} does not depend on junction geometry) is only of limited validity for the device, as it neglects the Fraunhofer effect. Given the nearly linear E_{J1} as a function of junction length (third column in Tab. 1), it is of course reasonable to assume Fraunhofer to be weak, but there seems to be at least some tendency of a sublinear behaviour of E_{J1} versus junction length (which I would expect when including Fraunhofer, due to the increased junction area). The point with respect to the second harmonic is that (based on the simple argument that vector potentials coupling to the Josephson effect are effectively doubled for Cooper-pair cotunneling) I believe the second harmonic to have twice the Fraunhofer “frequency”, such that it will have a stronger reduction effect — or in other words, E_{J2A} might in all likelihood have a more pronounced sublinearity than E_{J1} with respect to junction length. If my thinking is right, this might somewhat relax the upper bound estimate of the Andreev contribution to E_{J2} . It could be, that this is a fairly weak effect, but I think it should be investigated to be on the safe side.

We thank the reviewer for pointing out an important property of the junction. In the following, we show that the Fraunhofer effect is negligibly small in our experiment.

As shown in Fig.2f, the area of our SQUID loop is approximately $80 \mu\text{m}^2$. We apply an out-of-plane magnetic field (perpendicular to the SQUID plane) to bias the SQUID using an external coil. At the operating bias near half flux ($0.5 \Phi_0$), the corresponding out-of-plane magnetic field is approximately $0.5/80 = 6.25 \times 10^{-3} \Phi_0 / \mu\text{m}^2$.

We next consider the extreme (worst-case) scenario in which an in-plane magnetic field exists with the same magnitude as the applied out-of-plane field. Assuming a

rectangular junction, the ratio between the junction side area perpendicular to the in-plane field (JJ thickness $\times$ length) and the SQUID loop area gives an estimated in-field magnetic flux of $\Phi_i = 3.75 \times 10^{-3} \Phi_0$. Therefore, the Fraunhofer effect is expected to be $\sin\left(\frac{\pi\Phi_i}{\Phi_0}\right) / \left(\frac{\pi\Phi_i}{\Phi_0}\right) = 0.99998$, indicating that it is negligible under the worst-case assumption.

We've added a brief sentence in the main text stating that the Fraunhofer effect is small:

"We note that the change of E_{J1} due to the Fraunhofer effect in our experiment is estimated to be negligibly small, less than 0.01\%."

2) One of the important points for this work to be impactful, in my eyes, is that the series inductance extracted from fitting with the lumped element model matches with a microscopic explanation as to where the residual inductance is actually coming from. Indeed, the authors provide estimates based on an InductEx simulation of the loop inductance, and they find compelling overlap between fitting and induction simulation (all on the order of 10 pH). But they fail to specify what they mean with the words "loop inductance", respectively, which part of the overall device geometry they actually included for that simulation. Let us consider the micrographs in Fig. 2 e) and f). Did they simulate the entire superconducting bulk (the entire pink area in Fig. 2e)), or only parts of it? I would presume that the large capacitor pads (e.g., the central pink cross in Fig. 2e)) can be neglected, and the bottleneck residual inductance comes from the Dolan-type bridge (the thin pink part at the bottom of 2e)) which then branches out into the two individual junctions. But which parts of the bridge are important — only the actual loop or the entire bridge part up until the pad? I think this important point should be clarified. After all, for maximum impact, the conclusions by the authors should, in my opinion, not depend on whether or not the SQUID loop is present.

As the reviewer mentioned, we simulated the Dolan bridge structure until the pad (SQUID loop + two extending interconnects at the top and bottom, as shown in Fig.2f). The effect of this interconnect outside the SQUID loop can be modeled as an additional inductance in series with the loop. Using Inductex simulations, this additional inductance is estimated to be approximately 20 pH.

In the supplement (paragraph below Eq. S27), we briefly discussed the effect of this additional interconnect inductance. There, we numerically diagonalized a more general circuit model, where an additional inductor was introduced to the circuit shown in Fig. S3,

between C_{sh} and the SQUID. We confirmed that the resulting transition spectrum does not significantly deviate from the energy of the Fig. S3 circuit, where the difference fell within the measured linewidth.

This is because the inductance inside the SQUID loop primarily modifies the fluxon transitions, which are the main features in our measured bias range, whereas the interconnect inductance mainly affects the plasmon transition.

We've added a section in the supplement [Section S6] discussing the Inductex simulations.