*Dear Editor,*

*We thank the Referee for reviewing our paper and furnishing this report.*

*We have considered all comments, and we have applied changes to the original version of the paper to address the issues raised. We have also made a few additional modifications.*

*First, we updated the mass range that was never explored by haloscope experiments before and cited the latest papers from ADMX and CAPP. We also updated Figure 11 and added the results from CAST, UF, RBF, and the 2022 CAPP paper.*

*Second, the limits on the axion-two-photon couplings are updated due to an improved calculation of the cavity form factor in the Ansys HFSS microwave simulation.*

*Third, we add one more citation to a theory paper. More details of these additional changes are presented at the end of this letter.*

*We are at your disposal for any further clarifications.*

*Sincerely,*

*TASEH Collaboration*

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Color code:

Black: the original text of the referee comments

Blue: our response

Green: quoted original text

**Bold green: changes to the paper**

Red: explanation regarding the additional changes

===============================================================Referee Comments for the paper “Taiwan Axion Search Experiment with Haloscope: CD102 Analysis Details”

**Summary:**

This paper describes the analysis details of the Taiwan Axion Search Experiment with Haloscope (TASEH) science run which searched for axion generated microwave signals between 4.70750 – 4.79815 GHz from Oct 13 - Nov 14, 2021.

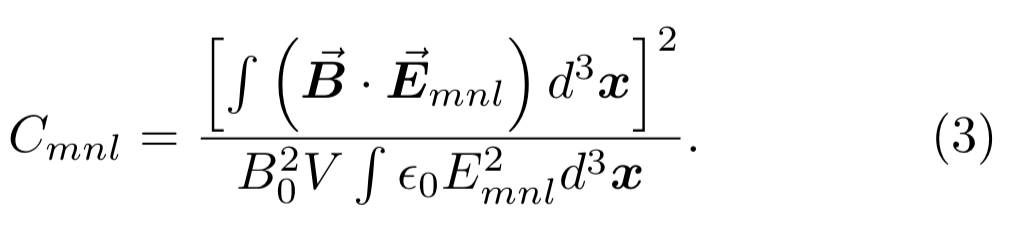
**General Comments:**

1. The authors did a very nice job of laying out the details of their analysis methods and this paper is an excellent companion to the PRL that they wrote outlining the scientific results. I appreciated the attention to detail, and I only had some minor comments and questions (see below). The only minor general criticism I had was that the analysis seemed to follow the generally accepted methods that had been outlined in other experiments and did not appear to add anything significantly novel to the process of searching for axions and setting appropriate limits with a null result. That said it does provide all the important details needed to assess the scientific results which are the more important. I’d recommend publication once the minor comments and questions below are addressed.

**Specific Comments, Questions & Suggested Fixes:**

1. Page 2: Equation 3: You are missing the dielectric constant in the form-factor equation #3.

Thanks for pointing this out to us. We have added the vacuum permittivity in Heaviside-Lorentz units in Eq. 3 and also cited the axion review paper by P. Sikivie. Now the text is as follows:



**Here, the vacuum permittivity in Heaviside-Lorentz units ε0 = 1 is used [48].**

**[48] P. Sikivie, Rev. Mod. Phys. 93, 015004 (2021).**

1. Page 3: When you list the T\_sys of 2.0-2.3 K it would be useful to make clear at this point in the  paper that the noise is dominated by the first stage HFET amplifier. Perhaps add “... T\_sys for  TASEH is about 2.0-2.3 K, **dominated by the first stage amplifier**, which gives a...”

Thanks for your suggestion. The original text

the baseline value of Tsys for TASEH is about 2.0–2.3 K, which gives a noise power of approximately (1.4 − 1.6) × 10−19 W within the 5-kHz axion signal line- width, five orders of magnitude larger than the signal.

is now modified to:

**the baseline value of Tsys for TASEH is about 2.0– 2.3 K, dominated by the first stage amplifier. Therefore, the noise power is approximately (1.4 − 1.6) × 10−19 W within the 5-kHz axion signal line-width, five orders of magnitude larger than the signal.**

1. Page 4: Can you updated Reference 52 in the bibliography with were you are submitting that  paper? Is it also going to PRD or another journal?

This paper was submitted to and accepted by Review of Scientific Instruments. It is not yet published. Therefore, we added a note in Reference 56 (used to be Reference 52):

**[56] H. Chang, J.-Y. Chang, Y.-C. Chang, Y.-H. Chang, Y.-**

**H. Chang, C.-H. Chen, C.-F. Chen, K.-Y. Chen, Y.- F. Chen, W.-Y. Chiang, W.-C. Chien, H. T. Doan, W.-C. Hung, W. Kuo, S.-B. Lai, H.-W. Liu, M.-W. OuYang, P.-I. Wu, and S.-S. Yu (TASEH Collabora- tion), (2022), accepted by Review of Scientific Instru- ments, arXiv:2205.01477 [physics.ins-det].**

1. Page 4: I was a bit surprised by the relatively large variation (1.9 to 2.2 K) over the relatively  small frequency band of 4.72-4.8 GHz. Does this frequency dependent variation match the HFET datasheet or is there something else going on (reflections in the line for example from impedance mismatches)? Not a criticism but I am curious.

Presumably the frequency-dependent variation of the system noise in Figure 1 could be coming from the frequency response of the HEMT noise, or from the frequency-dependent attenuation between the noise source and the HEMT amplifier due to impedance mismatches. We check the HEMT added noise as a function of frequency in the datasheet provided by the HEMT manufacture. However, both the frequency resolution and the noise magnitude resolution of the HEMT added noise in the datasheet are too coarse to see this level of variation. Therefore, at this point we are not sure about the major cause of this variation.

1. Page 4: You mention data before and after an earthquake. Can you specify the data and magnitude of the earthquake that caused the issue? Does this correspond to a frequency range where data was taken before and after the earthquake?

The data during the frequency range with the largest data-calibration difference were actually taken after a magnet quench at 9:20am on October 16, 2021 and before the first rescan. During this period, an earthquake of the intensity scale 4 stroke the lab at 1:11pm on October 24. We have modified our text to clarify what happened.

The biggest difference is 0.076 K in the frequency range during which the data were recorded after an earthquake. The source of the difference is not understood, therefore, the difference is quoted as a systematic uncertainty together with the RMS of the noise.

Is now modified to:

**The biggest difference is ≈ 0.076 K in the resonant frequency range of 4.77775-4.78731 GHz, during which the CD102 data were recorded after a magnet quench on October 16 due to a cooling water failure and before the first rescan of the CD102 data run (see Sec. IV E for the definition of rescan). In this period, an earthquake of the intensity scale 4 occurred on October 24. Both the magnet quench and the earthquake may have impacts on the readout during the data taking [56]. However, the exact source of the readout change is not understood. Therefore, the difference is quoted as a systematic uncertainty together with the RMS of the noise.**

1. Page 5: Figure 1. Are the 19 measurements for each frequency taken close in time or are they spread out during the run?

The data of the 19 calibration measurements presented in Figure 1 were taken close in time (from 9:04pm on November 18 to 6:43am on November 20) after the data taking. The original text

The average of the added noise Ta over 19 measurements has the lowest value of 1.9 K at the frequency of 4.8 GHz and the highest value of 2.2 K at 4.72 GHz, as presented in Fig. 1.

is now modified to:

**The average of the added noise Ta over 19 measurements, performed after the data taking, has the lowest value of 1.9 K at the frequency of 4.8 GHz and the highest value of 2.2 K at 4.72 GHz, as presented in Fig. 1.**

1. Page 5: You mention that this paper considers uncertainties to be uncorrelated between frequency bins but that Ref [45] does not. Can you explain why you chose that assumption or why the assumption from ref 45 doesn’t apply?
2. Page 5: You mention two independent groups doing the analysis. This is an excellent idea. Can you provide a note on any differences that were discovered between the analysis and how they may have gotten resolved? Are these groups at different institutions?

The two groups are from different institutions. The initial disagreement came from different understandings of the analysis procedure developed by HAYSTAC. For example, one group considered frequency misalignment in the Maximum Likelihood weights for merging and the other did not. Others are more technical issues, such as using different programs to fit the added noise from calibration or quality factors/resonant frequency, etc. However, we consider these details are not of interest to the readers and prefer to keep the text as it is. Thanks for your understanding.

1. Page 6: Just to make it explicit can you add to the sentence “...the difference between the frequency f\_ij in bin j and the **cavity** resonant frequency f\_ci.”

Done.

1. Page 8: Please adjust the following sentence: “... if there were any potential signal with **~~an~~ a** SNR larger than 3.355, a rescan would be **~~proceeded~~ performed** to check if it **~~were~~ was** a real signal or a statistical fluctuation.”

Done.

1. Page 8: Please adjust the following sentence: “... adjusting the tuning rod of the cavity **~~so~~** to match the resonant frequency **~~to the frequency~~** of the candidate.”

Done.

1. Page 8: You mentioned that you had a signal that was persistent after the magnetic field was shut off but that you could not identify with any external signals. Do you look at it on any other modes? Is there any way to rule it out as a potential dark photon?

Due to the limited amount of DR time available to TASEH, we did not look at it on any other modes. Therefore, we cannot rule it out as a potential dark photon. Thanks for pointing this out to us. We will keep this in mind in our next run of data taking.

1. Page 8: At the beginning of Section V: “Analysis of the Synthetic Axion Data” I got a bit confused as to which were the hardware injected synthetic axions and which were the simulated injected axions put into the analysis chain via software. Can you be a bit more specific on how many “hardware injected synthetic axions” and “software injected synthetic axions” were used and when? Is there a reason you did not perform the hardware injections before or during the run and only after it or did I misunderstand the timing?
2. Page 8: Why was the hardware injected axion signal wider at 8 kHz than the expected axion signal?
3. Page 9: Please adjust the following sentence: “Uncertainty due to the **frequency** misalignment...”

Done.

1. Page 10: Perhaps I missed it but did the systematic uncertainties of 4.6% apply uniformly or was there any offset bias?
2. Page 11: Is figure 9 referenced anywhere in the text? I didn’t see that it was.
   1. It was referenced on page 10 in the following text
      1. –  Compare the mean μnoise and the width σnoise of the measured power after applying the SG filter, assuming that no signal is present in the data. See Fig. 9 for an example distribution of the measured power from the averaged spectrum of a single scan;
3. Page 11: Please make clear at the end of section VII that reference [54] is for 90% confidence  limits and not the 95% that is used in this paper.
   1. Due to additional references, now the ADMX paper is Ref[58]. We added the following text at the end of Section VII:

**Note in Ref. [58], the limits were derived at 90% C.L., rather than at 95% C.L. as presented in this paper.**