nature portfolio

Peer Review File

The detection of microseismic activity with the CUORE tonne-scale cryogenic experiment

Corresponding Author: Dr Carlo Bucci

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)
This article claims the first detection of microphonic noise in low-temperature calorimeters due to micro-seismic vibrations from distant marine sources. They provide evidence in form of correlations of time-dependent measured noise in the CUORE calorimeters with Copernicus Earth Observations of the marine environment closest to the CUORE location at the LNGS in Italy. These correlations consider seasonal modulations of the Mediterranean Sea and modulations of the energy resolution of CUROE detectors. Additionally, selected storm-events from Copernicus data correlate with CUROE energy resolution.
A cited denoising technique is carefully applied and discussed and its implications are clarified for the CUROE project. All relevant details are given.

The claim is substantiated convincingly with detailed discussions of all relevant details including citations of previous work like their [26].
To the best of the referees knowledge the primacy claim is true which immediately implies novelty. The impact for all highly sensitive rare event searches, not merely CUORE, is discussed. The presentation of the article is clear and complete.

Pending minimal, minor suggestions, see below, article acceptance is recommended.
Suggested adjustments:
page 6, line 174: use 'suggests' or 'implies' instead of proves: correlation is not causation.
page 8, line 222: include a reference (, see ...) to section 4.2, Tab. 1 which discusses that the opposite phase value is not a fit result as seemingly implied here.
page 14, line 493: not 'objects', use 'expressions' or 'equations'
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Reviewer #2

(Remarks to the Author)
This paper from the CUORE Collaboration in association with other scientists reports three claims related to the CUORE neutrinoless double beta decay (0vbb) experiment. Firstly, the paper claims the first detection of microseismic vibrations by the CUORE low-temperature calorimeters caused by marine wave activity in the Mediterranean Sea. The paper then shows a seasonal dependence to the detector's sensitivity, both in the low-energy exposure and higher-energy resolution, in (anti-)correlation with the sea wave heights. The impact of not accounting for this effect on the 0vbb sensitivity is then shown. Finally, the paper demonstrates a signal processing strategy to substantially reduce the impact of external noise on the detector, including marine microseisms.

It was very interesting to read the cross-discipline aspects of this paper, combining neutrino detector technology with geophysics and signal processing techniques. The search for 0vbb is an extremely important focus in particle physics, not only because it would demonstrate how neutrinos get their mass (the only fundamental particle we don't know the answer to this question), but also as a means of trying to explain the Universe's matter-antimatter asymmetry. The extremely long lifetimes being searched for require very challenging conditions and techniques to obtain. Therefore, it is exciting to read a paper showing and explaining the marine origin of the seasonal dependence of CUORE's bolometers, and how that can be used to improve their 0vbb sensitivity. The method developed to reduce noise, including from the marine microseisms, has potential positive effects for upcoming low-energy searches. These results and techniques will be valuable in particular for the low-temperature calorimetry community, especially as new detector come online in the coming years.

Detailed comments:

The first major question is about the relationship between this paper, and reference 20, "L. Aragão, et al. The environmental low-frequency background for macro-calorimeters at the millikelvin scale". In the referenced paper, also about the CUORE experiment, the Conclusions section says:
"Here we demonstrate for the first time that low-temperature macro-calorimeters operated at ~10 mK can be sensitive to environmental vibrations induced by marine microseisms, despite the sea being distant ~50 to 100 km from the detectors position. We presented a novel multi-detector approach to correlate environmental data about the Mediterranean Sea (provided by CMEMS), the seismic activity at LNGS (provided by GIGS seismometer from INGV and SEISMO from CUORE), and the response of a couple of CUORE low-temperature calorimeters. We assessed that marine microseisms induce an increase of the noise of CUORE detectors in the sub-Hz frequency range. Such noise is linearly correlated with the sea waves activity reconstructed by CMEMS in two regions of the Adriatic and Tyrrhenian Seas close to the Italian coastline."
There appears to be substantial overlap in the claims made about observing marine microseisms for the first time with low-temperature calorimeters between these two papers. We are unsure to what extent the submitted paper can claim "First-ever detection" in the title if the already-published paper appears to have already achieved this. Also, given this overlap, we feel that the text of this submitted paper should be far more explicit mentioning this, and also explain the key features that are new about the paper.

In the sentence starting on line 172, "The striking linear correlation between...". Here the paper claims that the strong linear correlation seen in Figure 2 is proof of sea storms causing microseismic vibrations, causing a worsening of the detectors' energy resolution. It's not clear that the correlation in Figure 2 alone is sufficient to claim causation? There's certainly a strong correlation between the sea heights and detector resolutions, and the model of microseismic vibrations being the cause seems very plausible. However, it would be valuable to show something more at this point in the paper to make the case for this causal connection, which is a stronger claim than mere correlations. For example, the seismometer data?

In figure 3, the sinusoidal fit to M10 appears to be driven by the first half-cycle and data after the threshold change. The period from 12-30 months looks like a poorer fit. It would be good to include some metric on the goodness of fit, perhaps added to table 1. How well can you reject the null hypothesis of no seasonal variation?
The fit procedure for these figures is described in Section 4.2, but could include more details: What procedure was used to perform this maximum-likelihood fit? How were the uncertainties for each parameter extracted?

The explanation of the phase used in the two fits is unclear and we found that we had each interpreted it differently - is the same phi value fit simultaneously to both data sets, just with a Pi offset for figure 3 or was phi fixed to be in phase for figure 4?

It makes sense to us that the average M10 decreases with the operating temperature increase but is perhaps surprising that the oscillation amplitude decreases - the minimum value of M10 (winter values) does not seem to increase in line with the minimum points in the fit.

In figure 4, no change in FWHM performance is anticipated after the change in operating temperature at month 30. The authors should justify why this was not included in the fit. Again goodness of fit metrics could be added.

Given that a single Time period and phase are reported in table 1, we assume that both the M10 and FWHM data are fitted simultaneously - this could be made clearer in the text. It is interesting that the period is a little shorter than a year (though not significantly so) as this would allow processing over time that would be hard to explain - what would the fit result be if T were fixed to 12 months?

Figure 5 shows the summer and winter sensitivities - it would be useful to link to the S_S and S_W nomenclature used in the methods section - but the text proceeding this figure states that S_S^0nu is the sensitivity that could be achieved in the ideal case in which CUORE would be fully decoupled from micro-seismic variations. We do not think the summer represents full decoupling?

The denoising is only applied over the 2-month period where the data from an additional seismometer is available. We realise that there is not enough data from this to use in the time correlation study, but it seems odd that no data is shown from the auxiliary devices and there is no evidence to back up the expectation that the seismometer data will have most impact on the low-frequency noise. (no information is given on the frequency range of measurement for each device). The seismometer data would also be useful to confirm that the waves explicitly cause the CUORE detector performance changes.

The last major question is about the relationship between this paper and reference 26, "Vetter, K.J., et al.: Improving the performance of cryogenic calorimeters with non-linear multivariate noise cancellation algorithms." The submitted paper says that "we recently developed a denoising algorithm which can be implemented during the data post-processing." There are very strong similarities between Figure 6 in the submitted paper, and Figure 13 of the published paper. The authors should clarify what the key novelty in Section 2.3 of the submitted paper is, as compared to the published paper?

Minor comments

The DOI link in reference 6 is broken.
The DOI links in references 22 and 23 are identical. The paper reference number and published date for 23 also appears to be incorrect.
Unless I am mistaken, I don't see references 32, 33, or 34 referenced anywhere in the paper. Were these meant to be cited at some point?

To conclude, the paper is very interesting, and well written, but our main concerns currently are understanding what new it has done beyond the work by CUORE already published in references 20 and 26 (besides the correlated seasonal modulation of the Mediterranean Sea waves and the CUORE detector sensitivity).

Reviewer #3

(Remarks to the Author)
I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Communications Physics initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

Version 1:

Reviewer comments:

Reviewer #2

(Remarks to the Author)
Thank you for taking our comments into consideration. We feel that they have been addressed well and that the additions to the manuscript have improved it. We note a couple of small typos in the added text on p6 ("contributing _to_ the detectors noise" and "brigde") to fix.
We would be happy to see this version published.

Reviewer #3

(Remarks to the Author)
I co-reviewed this manuscript with one of the reviewers who provided the listed reports. This is part of the Communications Physics initiative to facilitate training in peer review and to provide appropriate recognition for Early Career Researchers who co-review manuscripts.

We thank the referees for their feedback. We think that addressing their comments improved the quality of the paper. In this document, we reply to each comment raised by the referees. Any modification/addition implemented is highlighted in blue in the new manuscript.

Reviewer #1 (Remarks to the Author):

This article claims the first detection of microphonic noise in low-temperature calorimeters due to micro-seismic vibrations from distant marine sources. They provide evidence in form of correlations of time-dependent measured noise in the CUORE calorimeters with Copernicus Earth Observations of the marine environment closest to the CUORE location at the LNGS in Italy. These correlations consider seasonal modulations of the Mediterranean Sea and modulations of the energy resolution of CUROE detectors. Additionally, selected storm-events from Copernicus data correlate with CUROE energy resolution.
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Reply: The cited paper "L. Aragão, et al. The environmental low-frequency background for macro-calorimeters at the millikelvin scale" was a preliminary work, intended to investigate the (at the time) unknown sources of noise in the sub-Hz domain. The scope of that paper was limited to two CUORE crystals, studied over a time scale of the order of a week during a storm outbreak. The main goal of that paper was to describe in detail the analysis procedure developed to correlate the calorimetric data with marine and seismic data, and to provide first hints of the correlation between the marine environment and CUORE sub-Hz noise. No assessment of the impact on parameters like energy resolution and thresholds were performed yet.

Based on what we learnt in that article, the scope of the current article is to:

- extend the analysis on the entire CUORE 988-detector array, to prove that the CUORE array as a whole is sensitive to microseismic noise;
- extend the analysis to a time scale of 4 yr (almost corresponding to the 2 ton*yr exposure accounted in the most recent data release), to prove that the impact of microseismic noise has sizeable effects not only on the time scale of individual storm outbreaks, but also when averaged over longer time scales, highlighting seasonal patterns;
- estimate the impact of such microseismic noise on some crucial parameters relevant for the CUORE performance: low-energy thresholds and energy resolution at the MeV-scale.

All these aspects were not discussed in the previous paper. They required the development of a dedicated analysis framework in order to evaluate the relevant parameters of all CUORE detectors, and to correlate them with marine data from Copernicus. The analysis technique developed for the paper "L. Aragão, et al. The environmental low-frequency background for macro-calorimeters at the millikelvin scale" was the starting point of such analysis framework. We add a comment on it in section 2.1, after the bullet points.
Given these considerations, we think we can refer to a “first detection” when referring to a ton-scale experiment like CUORE over 4 years, rather than a couple of detectors monitored over 1 week. We are open to reconsidering the title if it remains a major issue according to your judgment.

Comment: In the sentence starting on line 172, "The striking linear correlation between...". Here the paper claims that the strong linear correlation seen in Figure 2 is proof of sea storms causing microseismic vibrations, causing a worsening of the detectors' energy resolution. It's not clear that the correlation in Figure 2 alone is sufficient to claim causation? There's certainly a strong correlation between the sea heights and detector resolutions, and the model of microseismic vibrations being the cause seems very plausible. However, it would be valuable to show something more at this point in the paper to make the case for this causal connection, which is a stronger claim than mere correlations. For example, the seismometer data?

Reply: We have modified fig 2, adding a comparison between the time profiles of Copernicus marine data and of seismic data acquired underground at LNGS. We add a discussion on the correlation between marine and seismic data, and on the role of seismic data in allowing a direct correlation of marine and CUORE data (second half of section 2.1).

Comment: In figure 3, the sinusoidal fit to M10 appears to be driven by the first half-cycle and data after the threshold change. The period from 12-30 months looks like a poorer fit. It would be good to include some metric on the goodness of fit, perhaps added to table 1. How well can you reject the null hypothesis of no seasonal variation?
The fit procedure for these figures is described in Section 4.2, but could include more details: What procedure was used to perform this maximum-likelihood fit? How were the uncertainties for each parameter extracted?

Reply: All simultaneous fit in this work is done by means of BAT (Bayesian Analysis Toolkit) software. The fit parameters uncertainties are defined as the 1-sigma interval of the marginalized posteriors (Marginalization algorithm used: Metropolis) (added in sec 4.2, before eq 4 and eq 5). In table 1 we add the chi2 as a meter for the goodness of fit. The choice of chi2 is consistent with the definition of a gaussian likelihood as described in sec 4.2.

Regarding the time evolution of M10, in the period 12-30 months we observe a poorer match with the modulation pattern. We investigated and we realized that the impact of microseismic activity on the detectors, in terms of low-energy threshold and exposure, is lower in that period. The reason of this behaviour is linked to a different configuration of some elements of the insulation system of the experiment, called minus-k isulators.They are special insulators based on the minus-k technology, highly effective in suppressing above-Hz vibrations, but featuring a resonant behaviour in the sub-Hz domain (peak at about 0.6 Hz). Indeed, 0.6 Hz is the frequency at which CUORE detectors are maximally responsive to microseismic vibrations (as discussed in "L. Aragão, et al. The environmental low-frequency background for macro-calorimeters at the millikelvin scale" for a couple of detectors). Our interpretation is that microseismic vibrations (typical frequency 0.2-1 Hz, most probable frequency at 0.4 Hz) excite an internal 0.6 Hz resonant frequency of the CUORE infrastructure, likely that of the minus K isolators. Therefore, the vibrations at 0.6 Hz are then transmitted to the detectors, enhanced. During the period 12-30 months, one of the three minus K was operated in a different configuration, in which its response to incoming vibrations is reduced. Our current interpretation is that, with only two out of three minus k insulators being fully resonating to microseismic vibrations, the enhancement of sub-Hz vibrations transmitted to the detectors results in a lower sub-Hz noise. This induces the detectors' thresholds to be lower (better) than expected w.r.t the other periods. The role of minus K insulators in enhancing sub-Hz vibrations is yet to be fully investigated and characterized. We are investigating the possibility of performing a detailed study of the role of the minus K insulators on the low-frequency noise of CUORE detectors toward the end of the 0nbb decay data-taking campaign, in order to possibly optimize their working configuration in view of CUORE phase II (dedicated on low-energy signatures) and CUPID.

Comment: The explanation of the phase used in the two fits is unclear and we found that we had each interpreted it differently - is the same phi value fit simultaneously to both data sets, just with a Pi offset for figure 3 or was phi fixed to be in phase for figure 4?

Reply: In performing the simultaneous fit, the phase has been defined as a common fit parameter for the modulation of VHM0, M10 and of FWHM(208Tl), similarly for what is done for the modulation period T. The reason is that both T and phi are expected to be the same in a situation in which the CUORE parameter is modulated by the sea activity.
However, while VHM0 and FWHM are in phase, M10 is in counter-phase, as it should be expected. To account for this, we defined:
phi(M10) = phi(VHM0) + π
where π is fixed. In this way the simultaneous fit is performed with phi(VHM0) being a common parameter for all the sets of data, leaving to the fixed term π the job of reproducing the counterphase pattern of M10. We adopted this choice for two reasons:

- by defining phi(M10) = phi(VHM0)+ π, we could perform the simultaneous fit with a single phase as a common fit parameter. Otherwise, we would have needed to define two independent phases, one for VHM0 and FWHM, and one for M10;
- in reporting the data in table 1, we chose to explicit the π factor for an easier comparison between the phases of the modulations of VHM0, FWHM and M10, and to highlight that the modulation of M10 is in counterphase w.r.t. the other two.

We rephrased sec 4.2 (just after eq 4) for more clarity.

Comment: It makes sense to us that the average M10 decreases with the operating temperature increase but is perhaps surprising that the oscillation amplitude decreases - the minimum value of M10 (winter values) does not seem to increase in line with the minimum points in the fit.

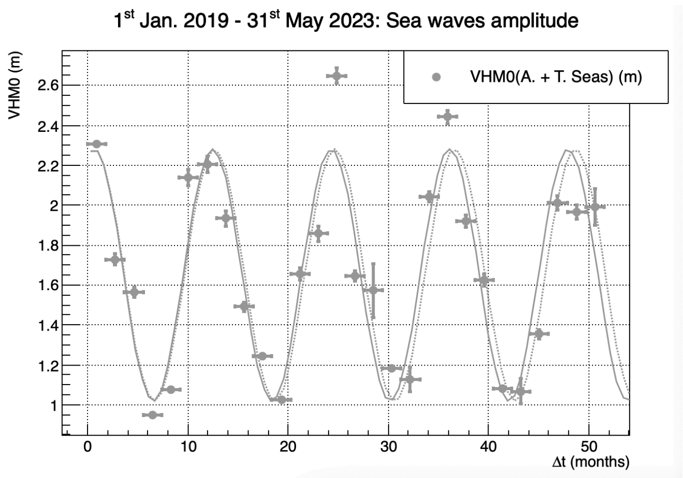
Reply: Increasing the detectors' operating temperatures leads to, on average, higher energy thresholds, and therefore to a lower average exposure mass at the low-energy (in this case defined by detectors with a threshold lower than 10 keV).
Let's consider now a summer period, corresponding to a local maximum of low-energy exposure. The low-energy exposure (below 10 keV) is expected to be higher when CUORE is operated at a lower temperature, and lower when it is operated at a higher temperature. Indeed, at lower temperatures the number of detectors achieving an energy threshold lower than 10 keV will be higher with respect to when the temperature is raised.
Let's consider now a winter period, corresponding to a local minimum of low-energy exposure. The higher microseismic activity tends to increases the thresholds, and many detectors achieve a threshold higher than 10 keV, resulting in a reduced low energy exposure. At higher operating temperatures, the thermal fluctuations play a more sizeable role in defining the thresholds with respect to at lower operating temperatures. Therefore, we could expect that at higher operating temperatures the "relative contribution" of microseismic noise in worsening the detectors threshold is less important than the contribution from thermal fluctuation.
This is how we interpreted the lower modulation amplitude observed when CUORE was operated at a higher temperature. Probably, the lower relative contribution of microseismic noise with respect to thermal fluctuations may also help explaining the higher mismatch between data and sinusoidal patter that you pointed out.

Comment: In Figure 4, no change in FWHM performance is anticipated after the change in operating temperature at month 30. The authors should justify why this was not included in the fit. Again goodness of fit metrics could be added.

Reply: We chose to not split the fit of FWHM(208Tl) between before and after the change of operating temperatures because the overall energy resolution does not appear to be sizeably affected by it. The change of operating temperature affects the thermal fluctuations of the detectors baseline, which impact both the baseline resolution (namely, the energy resolution at zero released energy, representing the contribution to the total energy resolution due only to noise fluctuations) and the low-energy thresholds. The total energy resolution of CUORE detectors, however, accounts not only for the baseline resolution, but also for the detectors response to a particle interaction. This latter contribution (still under investigations), is indeed the dominant one, and is not directly tied to the operating temperature. Comment added in the caption of fig 4.

Comment: Given that a single Time period and phase are reported in table 1, we assume that both the M10 and FWHM data are fitted simultaneously - this could be made clearer in the text.
It is interesting that the period is a little shorter than a year (though not significantly so) as this would allow processing over time that would be hard to explain - what would the fit result be if T were fixed to 12 months?

Reply: hopefully it is clearer after the modifications to address the comment about the phase.
The fit of marine data alone (VHM0 vs time), is almost unaffected in changing the period from 11.8 months (solid line in plot below) and of 12 months (dashed line). The slight offset of 0.2 months = 6 days might be introduced by the time grouping of CUORE data. CUORE data are grouped in dataset. Each dataset lasts roughly 6 weeks, and between a dataset and the next one a period of maintenance and calibration is performed. In the modulation plots we chose to set the time coordinate of each point to the center of each dataset time period, while the error bar represent the full extent of the dataset. Such a choice in the central point of the dataset as time mark could have by chance introduced a systematic offset in the period estimation.
In general, we chose not to fix the period to exactly 1 yr in this work in order to avoid including a bias due to an assumption on the behaviour of marine environments. Given that the estimated period is consistent within 1 sigma with the expected period of 12 months, we chose to keep the period as an unconstrained fit parameter.

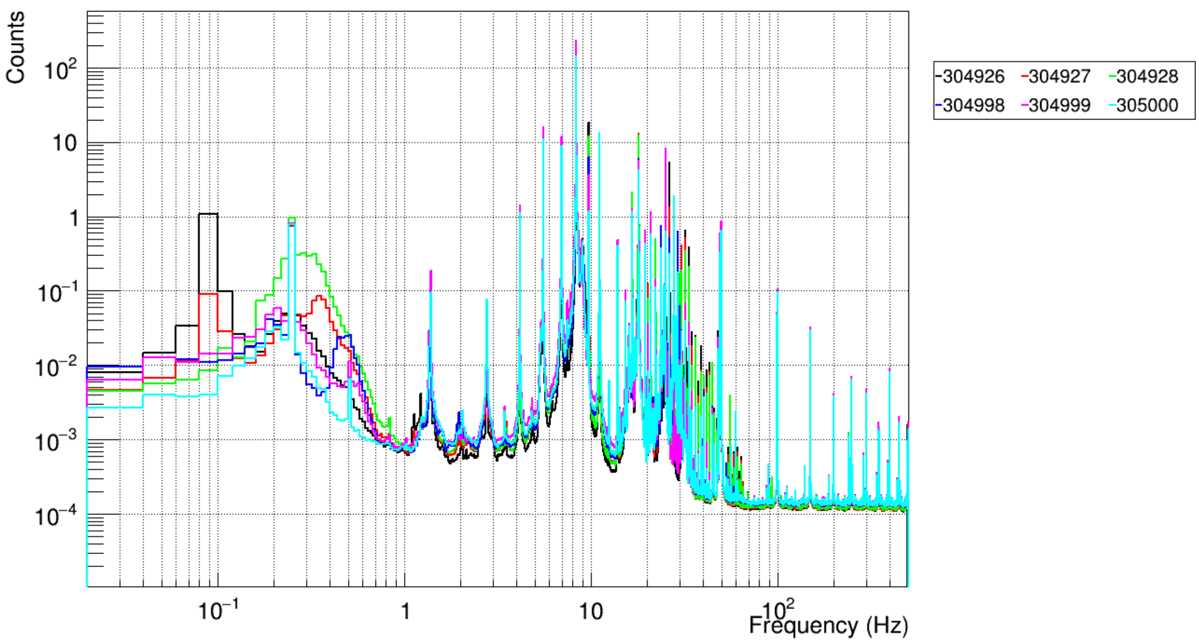


Comment: Figure 5 shows the summer and winter sensitivities - it would be useful to link to the S_S and S_W nomenclature used in the methods section - but the text proceeding this figure states that S_S^0nu is the sensitivity that could be achieved in the ideal case in which CUORE would be fully decoupled from micro-seismic variations. We do not think the summer represents full decoupling?

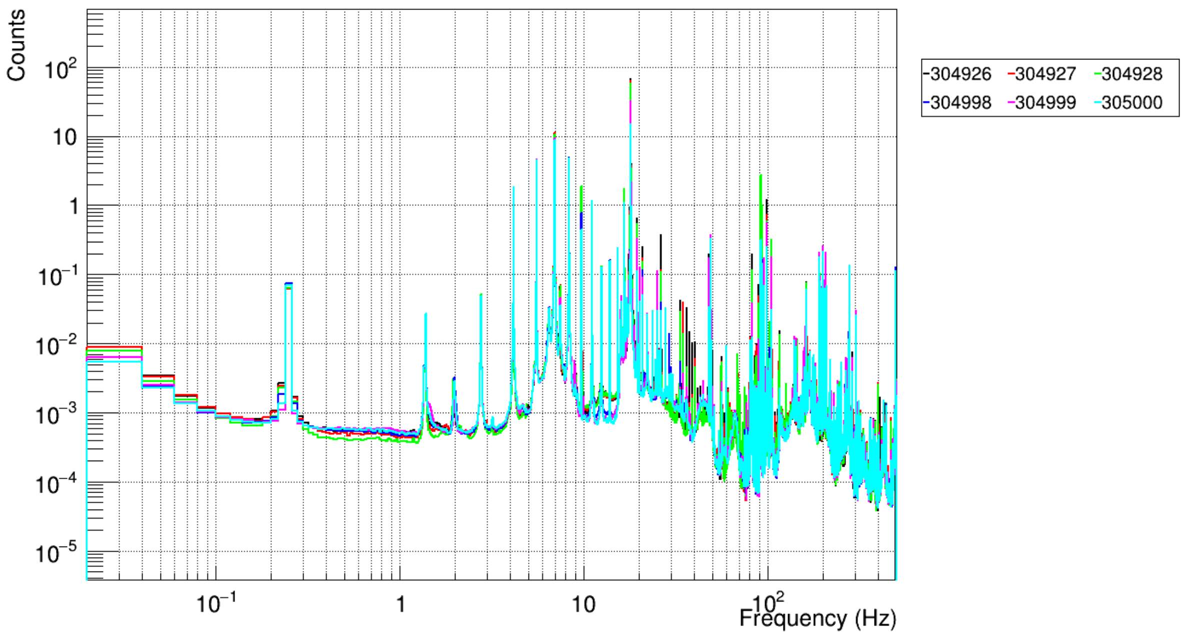
Reply: we define S_S is the sensitivity of CUORE in the limit case in which its energy resolution is constant over time, and equal to the resolutions achieved in summer. This is a period of minimal microseismic activity from the Mediterranean Sea. However, the microseismic activity is not zero. If CUORE were fully decoupled from microseismic activity, we could expect that it would achieve an equal or better energy resolution with respect to the summer resolution. For this reason, we assume S_S as the best data-driven estimation of the sensitivity to 0nbb that CUORE could be able to achieve if it were unaffected by microseisms, and therefore we use it to estimate the minimum sensitivity loss due to such environmental noise. We rephrased the text before Fig 5 for better clarity.

Comment: The denoising is only applied over the 2-month period where the data from an additional seismometer is available. We realise that there is not enough data from this to use in the time correlation study, but it seems odd that no data is shown from the auxiliary devices and there is no evidence to back up the expectation that the seismometer data will have most impact on the low-frequency noise. (no information is given on the frequency range of measurement for each device). The seismometer data would also be useful to confirm that the waves explicitly cause the CUORE detector performance changes.

Reply: We have added some details on the frequency range of each type of auxiliary device used in the denoising algorithm, as well as the sensitivities of the seismometers and accelerometers, which are the relevant low-frequency devices. We also found that in practice, the average noise power spectrum (ANPS) from the seismometer exhibits variation in the low-frequency power in a way that matches the behavior of typical microseismic activity, with a broad peak between 0.1 and 1 Hz. We do not see such a feature in the accelerometer data. Below, we include images of the seismometer and accelerometer ANPS from several CUORE runs. Note that these data were taken much later than the data discussed in this manuscript, but their configurations are the same as during the period discussed. We chose not to add these plots to the manuscript given the number of figures and tables already present.



Seismometer ANPS with a broad peak varying in intensity and maximum frequency. The numbers in the legend denote the CUORE run number, and each run lasts ~ 1 day. Here, counts correspond to V^2/Hz.



Accelerometer ANPS from the same CUORE runs as the above seismometer ANPS. Notice that at sub-Hz frequencies, the only feature that stands out above the noise is a ~0.25 Hz peak, which we believe is actually due to electrical pickup instead of vibrations, as we also see it on newly added antennas in our system (which were added after the initial drafting of this manuscript). No features that resemble microseismic noise are present in the accelerometer data. Here, counts correspond to V^2/Hz as well.

Comment: The last major question is about the relationship between this paper and reference 26, "Vetter, K.J., et al.: Improving the performance of cryogenic calorimeters with non-linear multivariate noise cancellation algorithms." The submitted paper says that "we recently developed a denoising algorithm which can be implemented during the data post-processing." There are very strong similarities between Figure 6 in the submitted paper, and Figure 13 of the published paper. The authors should clarify what the key novelty in Section 2.3 of the submitted paper is, as compared to the published paper?

Reply: Figure 13 of reference 26 shows the results of denoising a single channel (i.e. one bolometer & TeO_2 crystal) of CUORE data for a single CUORE run (~ 24 hours). Figure 6 of the submitted manuscript is the average over the entire CUORE detector after analysis cuts, with approximately 940 channels, over an entire dataset (~ 6 weeks). To make this more clear, we have changed the text when introducing the denoising algorithm and referencing our previous work. We now explicitly mention that this approach was shown to remove noise from a single channel of CUORE data from a roughly 24-hour period.

Minor comments
The DOI link in reference 6 is broken.
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Reply: fixed..

Unless I am mistaken, I don't see references 32, 33, or 34 referenced anywhere in the paper. Were these meant to be cited at some point?
Reply: By mistake, they remained from a preliminary version of the paper. We removed them.

Comment: To conclude, the paper is very interesting, and well written, but our main concerns currently are understanding what new it has done beyond the work by CUORE already published in references 20 and 26 (besides the correlated seasonal modulation of the Mediterranean Sea waves and the CUORE detector sensitivity).

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