We thank the Editor and both reviewers for their constructive comments, which have helped improve the study. We revised the manuscript thoroughly (including the figures) to improve clarity, and corrected grammatical and spelling errors to our best effort. Below we provide point-by-point responses to each comment. Reviewer comments in blue are followed by our responses shown in black. Line numbers refer to those in the revised manuscript (clean version) if not otherwise specified.

We have uploaded a clean version and a track-change version with major changes highlighted/commented.

Editor:

Based on reviews I have received, your manuscript may be suitable for publication after some revisions. Both the reviewers and myself believe that this manuscript is of interest to the community but that the context, in particular compared to other studies, and goals of the paper are not always clear. The paper should also be thoroughly checked for grammatical and spelling errors.

We have added a short literature review and clarified the objective of this paper in the Introduction section. We also expanded the Discussion section. We corrected grammatical and spelling errors to our best ability.

Reviewer A:

Please find below my review for the manuscript “Lower St. Lawrence Seaway Amphibious Network for earthquakes and marine soundscape monitoring” by authors Yajing Liu et al.
The authors introduce a new amphibious seismic network deployed in the Lower St. Lawrence Seaway (LSLS), designed for the dual purpose of geophysical and water-column acoustic (i.e. marine mammals, ship noise) monitoring. The paper thoroughly details field deployments, data availability, data noise, and data quality, and presents preliminary analysis of the data for both earthquakes and marine mammal calls.
The manuscript is exceptionally detailed and useful as a guide for the amphibious network and its data, but it is lacking some context and needs to be thoroughly checked for spelling and grammatical errors. Following revisions, this manuscript should be suitable for publication.
There are two sections below: more general comments followed by line-specific comments.

- In general, this manuscript needs a thorough proofreading for frequent spelling and grammatical errors and continuity of active/passive tense.

We have done a thorough read through and corrected all the spelling/grammatical errors we identified, as well as the consistency of tense.

- From the abstract, it was not immediately clear that this is primarily a paper presenting an experiment, rather than a scientific interpretation. This should be made clearer early in the abstract: this is an experiment/data paper, and although preliminary contents of the data are shown, no interpretations are made.

We have added the following sentence to the abstract:

“This report documents the deployments, examines data quality, and presents preliminary results from the first month of OBS recording.” (line 23-25)

- This is also confusing in the introduction around Lines 75-80, where the authors discuss future plans for the experiment data. The discussion or conclusion section may be more appropriate for this, or the wording should be changed to emphasize that these plans are for future work and not covered in this paper.

We have removed much of that text (Lines 73-88, original submission) from the Introduction and focused on future plans in the Discussion section 5.3 “Future projects using LSLS-ASN data”.

- In general, this manuscript is quite long and contains many figures. The authors should consider combining some of the figures. Figure 3, showing data availability, could be moved to the supplement. Figures 10-12, which all showcase marine mammal calls, could be combined in some way, or the figures that demonstrate the method could be moved to the supplement (10 and 11), with one figure left in the main text that shows OBS versus hydrophone quality and that signals are recorded on multiple OBSs. There are also many noise figures – could Figures 4 and 6 be combined in some way?

We have taken care to condense the whale call figures. We merged the original Fig. 10a-d (fin whale call detection examples) with Fig. 11a-d (blue whale call detection examples) into the new Fig. 10, and moved the R-ratio and periodogram power plots (original Figs. 10e-h, 11e-h) to the Supplementary Information Figs. S8 and S9.

Given that the other reviewer commented asking about a data-summary figure, we have retained Figure 3 in the main paper.

The three noise figures (Figures 4-6) show different characteristics, Fig. 4 on the overall PPSD, Fig. 5 on the along-stream median PPSD variability in different frequency bands, and Fig. 6 on the along-stream frequency shift in the peak power, as appropriate for a data report focusing on data quality and noise issues. Reviewer B also made specific suggestions to improve these figures. We adopted those suggestions and opted to keep Figs. 4-6 in the main text.

- The authors should also check for redundancy between different sections, and edit the writing to be more concise where possible.

We have done a read through to reduce redundancy and make writing concise. For example, we merged all the discussion on possible noise sources into a newly created Discussion section 5.1 and focused Section 3 on describing the observations in Figs. 4-7.

- Consider annotating in either Figures 4 or 6 the different microseism frequency peaks you discuss in the text to help guide the reader's eye. It could also could help to have the different frequency bands you discuss marked, and point out the shifting frequency peaks in the OBSs.

The vertical grid lines in Fig. 4 help guide the eyes for frequency bands. In addition, we have added to Fig. 4a bracket arrows to denote the three frequency ranges presented in Fig. 5. We also added arrows in Fig. 6 to highlight the frequency shift in peak power.

- I have personally never come across the terms "single-frequency" and "double-frequency" peaks (Line 256+), and am instead used to the terms "primary" and "secondary". I am not sure how widespread this is, but to cater to all members of the community, you might consider saying "single-frequency (primary microseism)" and "double-frequency (secondary microseism)" for clarity.

The "single-frequency peak" and "double-frequency peak" terms are used in highly cited seismic noise papers such as Webb (2002, International Geophysics, Vol 81, Pages 305-318) and McNamara and Buland (2004; BSSA, Vol 94, No. 4, pp. 1517-1527). Thanks for the suggestion, to avoid confusion, we changed the wording to "single-frequency, or primary, microseism peak", and "double-frequency, or secondary peak" when they are first introduced in this paper (line 268; 270).

- In Lines 262-264, the authors claim that the primary microseism peak may not stand out due to already high background amplitudes or data smearing from aggregation. Could this instead have to do with the location in an inland seaway, rather than the open ocean where considerable wave energy is accumulated before waves interact with the coast? Maybe the primary microseism is simply not generated in the LSLS? Are there other papers that discuss inland seaway microseisms that could be referenced?

Thanks for the comment. Several factors may contribute to the seeming lack of the primary microseism peak in our dataset. First, the long-period (10 s and up) energy, most likely from tidal effects, is already very high (higher than the NHNL) for the horizontal components (Fig. 6); primary microseism peak (~10-16 s) would have been masked. Second, the lack of a clear primary microseism peak at the vertical component (Figs. 4 & 6) is likely an effect of "smearing" from aggregation because we do observe this portion of energy from a shorter duration of data, although the peak is more pronounced at some stations than others. For example, Fig. S2 (randomly selected 1-hour waveform) shows that the primary peak is clearly visible at CH3 (vertical component) at the three downstream stations LS106, 107 and 108, less so at other stations. This distinction (from analysis of a limited segment of the dataset) does suggest the inland seaway environment plays a role - the downstream stations are likely more impacted by open ocean conditions. We also note the primary peak is also not visible on the aggregated PPSDs from an OBS experiment in northern Red Sea (Parisi et al., 2024), a similar sea inlet environment as LSLS.

We added the above discussion in Section 3.1 (Microseisms) in the revised version.

- In Lines 274-276, the authors claim that some of the OBS noise may reflect water currents on the riverbed. Bottom current interactions with OBSs are well documented and tend to exhibit an obvious harmonic spectral banding/gliding effect that can be seen in the seismograms. The authors should consider checking for this type of signal on their OBSs to help support their claims of water current effects. See:
  - Essing, D., Schlindwein, V., Schmidt-Aursch, M. C., Hadziioannou, C., & Stähler, S. C. (2021). Characteristics of current-induced harmonic tremor signals in ocean-bottom seismometer records. Seismological Society of America, 92(5), 3100-3112.
  - Corela, C., Loureiro, A., Duarte, J. L., Matias, L., Rebelo, T., & Bartolomeu, T. (2023). The effect of deep ocean currents on ocean-bottom seismometers records. Natural Hazards and Earth System Sciences, 23(4), 1433-1451.

Thanks for the suggestion. We do plan to look for harmonic signals. While this paper was under review we had been in contact with D. Essing for the application of his harmonic tremor detection algorithm to the LSLS dataset. This is ongoing work. We have added these (and other relevant) references in the literature review and in our discussion of the possible effects of water currents (line 454-456).

- A completely different earthquake detection and location workflow is used for the OBSs and the nodal deployment, including very different magnitude definitions and velocity models. Why is this? Why couldn't data be combined or processed in the same way or the same magnitude definition used? The authors need to justify why such different workflows are necessary.

Apologies, the two datasets were originally analyzed by different co-authors with their own preferred workflows. We have now homogenized the workflow (phase picking, association, location, magnitude calculation) using the same velocity model. Both the OBS and nodal array catalogs now use Nuttli magnitudes, which is standard for the region. The homogenized catalogs are presented in the revised Section 4.1 and used in producing Figure 8, with details in Supplementary Table S2.

The nodal data was also run through phase-associators REAL and GaMMA (in addition to PyOcto that we used for OBS+CNSN data), and we haven't gone back to remove the effect of incorporating these tools, as this isn't meant to be an apples-to-apples comparison of the detection-capability of the two datasets.

- The performance of magnitude estimates in comparison to existing catalogs is not covered in any detail. This is important because magnitude over- or under-estimates on given stations could signal strong site effects. How well do the magnitude estimates the authors make line up with existing catalogs? This is presented briefly and Line 349, but the authors do not discuss whether magnitudes are systematically over or underestimated by certain instrument types, etc. This would be especially relevant in the Discussion paragraph starting at Line 416.

Thanks for the comment. We have added Fig. 8c to show the Nuttli magnitudes M_N from the three catalogs (NEDB, nodal array, OBS). The inclusion of OBS stations consistently leads to higher M_N values than the other two catalogs. We added this comparison and discussion in Section 4.1 (Lines 388-392).

- On Lines 436-438, the authors claim that the noise peak from 3-10 Hz on the OBSs may be due to ship noise. Is it possible that this higher peak also be due to bottom currents in the seaway? Is that what the authors meant to say with the word "partly"? If so, it was not very clear.

Indeed, it is possible riverbed currents also contribute to the 3-10 Hz noise, such as in harmonic tremors reported in other studies (Essing et al., 2021; Corela et al., 2023) as the reviewer pointed out above. We included these in the Discussion (Lines 454-456). We do find ship traffic signals (Figures S4-S5). Based on the limited examples of spectrograms, ship noise may contribute more to even higher frequencies (up to the Nyquist frequency 125 Hz). We have revised the discussion in Lines 324-329.

Title: should "seismic network" be capitalized?
We use "Amphibious Seismic Network" as part of the project name so we chose to capitalize it as a proper noun.

Line 27: "at cause" could be reworded to "suggesting tidal variations of riverbed currents as a cause".
Change accepted, thank you.

Line 97: "drop sites are chosen" doesn't match the tense in the rest of this section. Authors should do a thorough check for consistency.
We have made the edit to "were chosen" as well as several other changes in the paragraph to keep it consistently in the past tense.

Figure 1 inset: it would be nice to see large cities marked on the inset to help orientation – maybe Montreal and Quebec City?

We increased the inset area and marked cities "Montréal", "Boston" and "Halifax" as geographic references.

Line 116: should "fireware" be "firmware"?
Yes, thank you, it is corrected.

Line 127: "tolocal" written as one word.
Corrected, thanks.

Line 161: Should be “at” the start of our experiment,… NRCan “operated”… etc. Check for tenses!

We chose to retain “As of the start…”, but we corrected the tense throughout this paragraph.

Figure 3: consider marking where the nodal deployment occurred, and also background shading on the MG plot to show where OBSs overlap with the MG stations.

Thanks for the suggestions. We added background shades for the nodal deployment, OBS Y1 and Y2 operation periods to Fig. 3c.

Figure 3b: I think the years here are off and should read 2024-2025?

Apologies. The years should be 2024-25 for Y2 in Fig. 3b. Corrected.

Line 226: by station performance noise, do you mean instrumental noise? Or instrument-environment interactions (like bottom current interaction)? Station performance is an unclear term.

We changed this to “instrumental noise”. Bottom current interaction is considered a type of “environmental (ambient) noise” in the first part of this sentence (line 233).

Line 280, say systematic frequency “shift” not “glide” – gliding can refer to different spectral characteristics, so use shift to avoid confusion.

The reviewer is right. We changed the wording to frequency “shift”.

Line 318: this sentence needs grammatical reworking.
We have simplified the end of this paragraph to “We expect to continue refining parameters in the phase-detection and event-association workflow while creating a full earthquake catalog using the combined Y1 and Y2 data.” (Lines 344-345)

Line 354: the NEDB events are not in the catalog, but were there some phases detected by your stations that just weren’t successfully associated into events?

Some phases were typically detected, and sometimes weak phases are visible but were not picked by OBSTransformer. Both continuous noise and impulsive noise are common issues masking arrivals. In Figure R1 below, we plot the OBS waveforms for one example NEDB event that was missed in our catalog (the M1.7 event on October 25; P picks in blue, S picks in red; bandpass filter 5-12 Hz).

We have made a note of this in the text: “...for which we could not visibly identify many phases on the OBS.” (line 363-364)

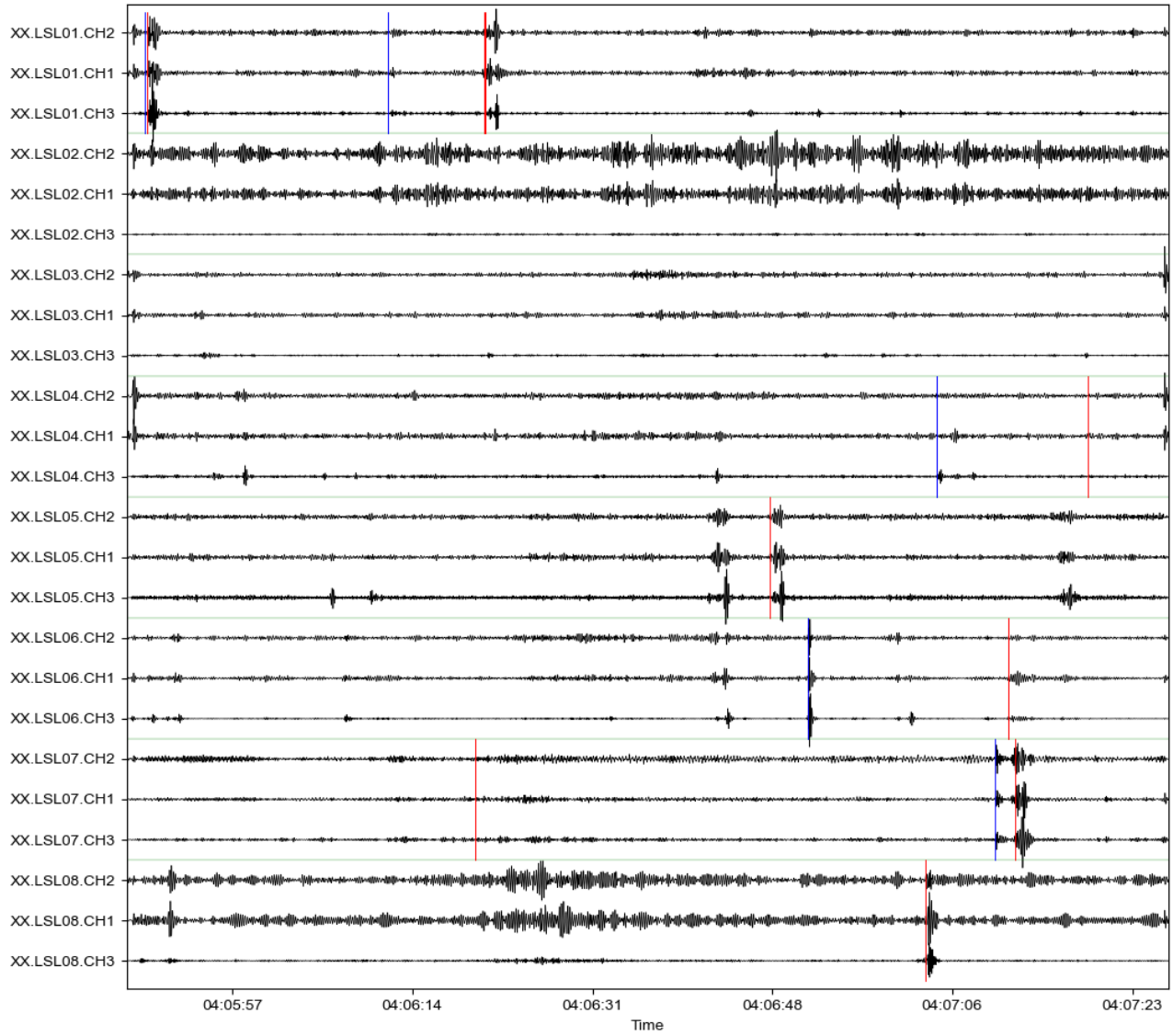

Figure R1. *Waveforms of the M1.7 NEDB event on October 25, 2023, that was missed in our catalog. P picks in blue, S picks in red; bandpass filter 5-12 Hz.*

Paragraph at Line 361: what were your estimated magnitudes for these events compared to the NEDB catalog?

We added Fig. 8c to show the event magnitudes from each of the three catalogs and associated description in Lines 388-392. The M_N variations are within 0.5-unit compared to the NEDB catalog.

Figure 8b: is this histogram shown as cumulative, where the number of earthquakes from each catalog are added up and plotted in different colors, or are the different colors plotted on top of each other? For instance, on what looks to be 09-30, do the OBS+CNSN detect 6 earthquakes

or 1 earthquake? This is unclear. Consider replotting this using transparent bars or simply histogram outlines so this is easier to distinguish.

The number of events from each catalog are shown in different colored bars stacked on each other, hence cumulative. We have used semi-transparent bars for each catalog and noted in the caption that they are stacked. In response to the catalog magnitude comment, we also added Fig. 8c which also helps to illustrate the number of events per day from each catalog.

Line 381: does the detection algorithm you mention here detect only blue whale A/B notes and the fin whale 20 Hz calls? Are there other calls it checks for that you do not record? Or are there other calls you might be recording that it does not check for?

The algorithm only targets the blue whale A/B notes and the fin whale 20 Hz calls; we did not check for other calls in this study. We have updated the text (Lines 395-398) to clarify - "... we applied the double-spectrogram detection algorithm of Plourde and Nedimovic (2022) for whale calls with a regular internote interval (INI), to one month of data from LSL01 targeting both fin whale ``20 Hz" calls (Roy et al., 2018) and blue whale A/B notes (Mellinger and Clark, 2003). Note that other calls without regular INI, such as blue whale ``arch" calls, were likely recorded but are not examined in this report."

Figure 9: It would be nice to see the magnitudes of these earthquakes, both NEDB and your calculated magnitude, listed in the title/on the plot.

See new Fig. 8c.

Line 397: "we assume this is true for higher frequencies as well." Why?

Fair question! We have just removed this statement for simplicity. We do assume it should be the case for frequencies up to 100 Hz (the seismometer instrument response should be flat until 100 Hz), but we are less confident about lower frequencies.

Line 440: I'm a bit confused by the reference to glacial tremor here. Are there glaciers nearby?

We removed this reference to glacial tremors, and added Essing et al. (2021) and Corela et al. (2023) for ocean current-induced harmonic tremors that are more relevant for our study.

Review by Zoe Krauss (feel free to contact zkrauss@uw.edu with questions).

Recommendation: Revisions Required

Reviewer B:

I have read the Data Report submitted by Liu et al. about the new amphibious seismic network in the north east of Canada and I believe that their experience on deploying OBS, nodes and land broadband stations is worth to be published as it helps the community to learn strengths and limitations of this type of deployments. However, before being published, I recommend to revise the manuscript to include a more systematic comparison with previous similar deployments, to homogenize the workflow for the earthquake locations and to make the discussion points more consistent with the rest of the manuscript.

More specifically:

Abstract

Line 28: ensure that this statement is consistent with what stated in the manuscript

Apologies, we failed to update that sentence after a revision of our original catalog. After making some other changes to homogenize our catalog-building procedure across OBS/Nodes and remove suspect blast events, we revised the statement in the Abstract to "The seismicity catalog from combined OBS and land stations shows two times more earthquakes than the Canada National Earthquake Database in the first month of deployment, as well as many suspected blasts" (Lines 28-30).

Introduction

The motivations are clearly stated but there is a clear lack of references to previous similar experiments, even more being a Data Report. E.g., Parisi et al., 2024 (Seismica) is one of those and this paper cites several other experiments. Moreover, there should also be more recent ones. Therefore, I suggest to deepen the literature review and include it in the introduction and refer to them through the text when needed (see other comment on the Noise Analysis section).

Thanks for the comment. We have added a short paragraph (Lines 84-92) in the Introduction as a brief (far from complete) literature review of previous OBS experiments with focuses on seismicity, fault structure, and ocean soundscape, including Parisi et al. (2024).

Line 36: when mentioning the number of events, specify the magnitude of completeness, or at least a the minimum one.

These M>6 earthquakes were from pre-modern instrumentation times (1663, 1791, 1860, 1870, 1925) so there is no magnitude of completeness available. We added "with the most recent (magnitude 6.2) in 1925" to clarify.

Line 81-84: this sentence is confusing and inaccurate. E.g., interference from OBS element is a seismic noise. Perhaps, did the authors mean non-tectonic instead of non-seismic?

In response to Reviewer 1's comment on the discussion on future projects (Lines 73-88, original submission) being inappropriate for the Introduction section, we have moved and merged to Discussion Section 5.1. The reviewer is right, interference from part(s) of an OBS is not a seismic noise. We removed this phrase.

Figure 1. Include a large area in the inset for people non familiar with Canada geography. I strongly advice against using triangles up/down of the same color, as it is non easy to see them.

We revised Fig. 1 with a larger inset area, marked large cities for geographic references, changed symbols for OBS Y1/2 sites, and used larger symbols for the nodal sites.

Offshore deployment

I suggest to shorten the hardware description to what is really important for the data.

We have removed descriptions of the digitizer, storage, and acoustic communication devices to keep this section shorter.

I would slightly expand on the implications of the low sampling rate of the hydrophone for the whale calls.

We have added a phrase indicating that "…sei, humpback, and North Atlantic Right Whale calls" (Line 130) are among the other calls we could target with a higher sampling rate (with an example reference for each species).

The community will be grateful for the instrument failure reports. It would be great to have a visual summary of the final data acquisition

Thanks for the suggestion. We have a visual summary of the data acquisition in Figure 3, but did not refer to it until later. We have now added references to Figure 3a and 3b in this section.

Line 124: I would not assume that every reader knows the RV Coriolis II. Be more specific.

Rather than being more specific, we just removed this part of the sentence for concision.

Data coverage

Check date of nodal deployment (line 183 mentions 2023, line 202 mentions 2024)

Figure 3. Check the time line of plot b

Apologies. Fig. 3b time axis was incorrectly labeled. It should be 2024-2025; this is corrected in the new Fig. 3b.

Noise Analyses

While it is exciting to highlight noise contributions (currents, tides, traffic), there are not specific evidences for these, neither comparison to similar signals in other papers. If these are not

possible, it should be clear that these observations are hypotheses.

The focus of this report is on data quality and noise documentation, and possible causes of various types of noise. Such causes remain hypotheses in this report, and will be cross-checked with other independent observations/models, such as ocean currents in the St. Lawrence Estuary, in our future studies. We moved the speculations of noise sources to Discussion Section 5.1, which is titled “Possible sources of noise peaks” to clarify the hypothetical nature of these causes.

When discussing the microseismic peaks, I suggest to review Parisi et., 2024 (seismica) where similar signals are discussed.

Thanks for the suggestion. We added Parisi et al. (2024) to the reference list.

Figure 5. As highlighted in Figure 1, please refrains from using triangles up/down of the same color as it doesn’t allow to see the trends you discuss in the text. Why is MG16 not presented? There should be enough data to be included.

We now use different shapes/colors of symbols to differentiate north and shore nodal sensors, Y1 and Y2 OBSs. MG16 does have ~8 months of continuous data but with problematic data quality (see Fig. R1 below) that may result from sensor/ground coupling or compatibility settings between the CMG-3ESP sensor and the Nanometrics Centaur digitizer. We will need to check and correct this during our next maintenance trip, scheduled for Summer 2026.

To provide context from other land broadband stations, we added to Figs. 5 and 6 relevant information from MG14, which has ~3.5 months of continuous data, May-August 2024. MG14 also complements MG13 (north shore, upstream) as a south shore, downstream station. MG14 unfortunately was victim to instrument theft, therefore no data is available from August 2024 onwards.

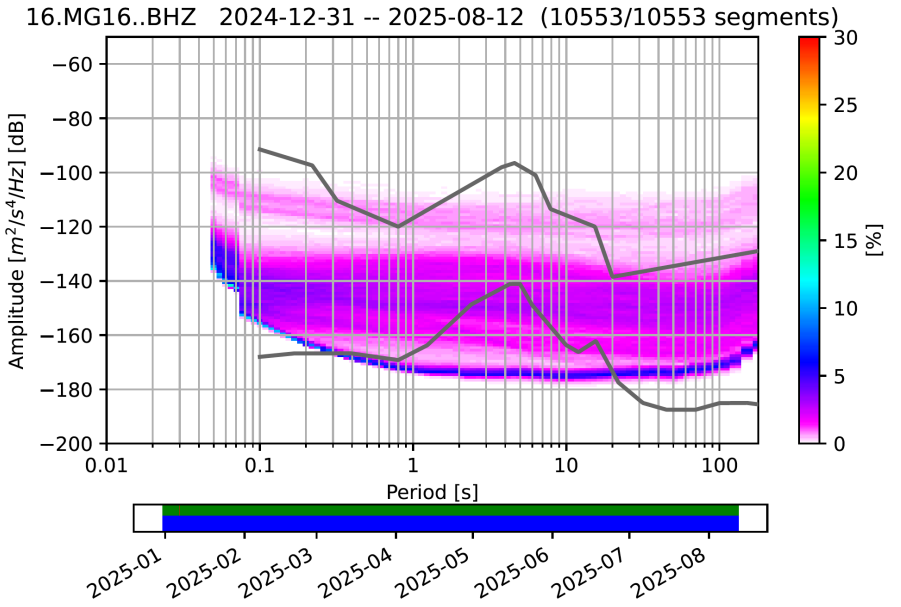

***Figure R2**. PPSD plot for MG16, possibly due to incorrect instrument response files from sensor and digitizer configuration mismatch.*

Figure 6. Please annotate the figure with what you discuss in the text, otherwise it is difficult to follow your reasoning. I would also include MG16 in this figure.

We added arrows in each panel of Fig. 6 to highlight the frequency shift. MG14 median PPSD is also added.

Earthquakes and possible blast

I found very interesting to include this preliminary earthquake analysis in this article and even more comparing the results from two difference subnetworks. However, the use of different methods and velocity models limit the usefulness of these analyses. Please, consider homogenize the two workflows.

Reviewer 1 had the same comment. We have homogenized the workflow for creating catalogs using the nodal array and OBS+land broadband stations. Please refer to the reply above, and to revised Section 4.1 and Fig. 8.

Figure 8. Make the yellow triangles larger.

Done.

Whales calls

The statement at line 396-400 is very interesting. If it is the case, the exclusive use of the seismometers should be clearly recommended.

Thanks. We have re-emphasized this point in the discussion, although we have not added a statement explicitly recommending the use of seismometers over hydrophones — we do not have a full sense of the use-cases of hydrophones in passive-acoustic monitoring so such a statement might be a bit outside our lane.

Discussion

The discussion could be strengthened and connected better to the rest of the article. An actual discussion on the implications of the findings of the previous sections is missing or limited. Clear take-home messages are recommended.

Thanks for the comment. We revamped the Discussion section by using subsections, focusing on the hypothetical sources for various noise peaks, uncertainties in earthquake and whale call detections, and suspected blast events, which are direct implications of findings in the previous sections. We also shortened and merged the discussion on future projects into a new subsection in Discussion. We added a Conclusion section as a summary of main findings and take-home messages from this report.

It is also interesting to know the potential plans for this dataset, however, the rest of the manuscript doesn't directly prove how these studies can be done with this dataset. Therefore, I suggest to keep these points but in a reduced form.

As these research ideas are major motivations for our experiment, they deserve to have a fair amount of discussion in a data report as data usage prospects. We followed the reviewer's comment to keep the description short but also sufficient to help readers who are not familiar with the study area to understand the various local processes, hence why the LSLS-ASN could be of use. These ideas will need to be proven in future projects, some of which are ongoing efforts and beyond the scope of this data report.

Review by Laura Parisi

Recommendation: Resubmit for Review