REVIEW REPORT FOR

Dense Seismic Array Monitoring of the Glacier Tongue of Isunnguata Sermia, West Greenland

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ROUND 1 REVIEWS

REVIEWER 1

Review of Paris et al., submitted to Seismica, reports their extensive seismic observations over the ablation area of Isunnguata Sermia in west Greenland. They analysed seismic data obtained from their dense seismic array and distributed acoustic sensor, and the potential to use seismic signals to study glacier hydrology, ice dynamics, and ice structure. I found that the manuscript is quite well prepared. Here I share very minor comments below. Hope they are helpful to improve your study.

Minor comments
L108 How is this part of the glacier dynamic? Your cloud briefly mentions the ice speed.

L144 and Fig. 1c, I wonder if the bamboo stick might be a cause of noise by vibrating it due to the winds. Do you see any influences?

L262 So the model is only forced by temperature and precipitation? Or does the weather station measure wind speed and radiation as well, and use them to force the model?

L369 It might be worth expanding on why 4–6 Hz was specifically chosen as the primary indicator for subglacial hydrology.

Fig 5c The y-axis tick of surface melt can be placed at the proper value. I don't think that you need to show those small numbers.

L542 It is hard to see if the located event really aligns with crevasses from the plots.

REVIEWER 2

Review of the manuscript "Dense Seismic Array Monitoring of the Glacier Tongue of Isunnguata Sermia, West Greenland" by Paris et al. This manuscript presents a dense seismic array experiment conducted on a glacier in Greenland over different seasons. The study provides a detailed description of the deployment strategy and instrumentation, accompanied by a thorough and well-structured data quality control section.
The paper is of clear interest and adds value from a methodological perspective, particularly regarding the design and implementation of large seismic arrays in challenging environments, as well as the associated data processing and quality control approaches. For example the detailed description on how do deal with the sensor tilting is of interest in

cryospheric deployment areas. The detailed documentation of the deployment strategy will be especially valuable for future field campaigns in similar settings.

The manuscript is well written and easy to follow. While the current focus is primarily on methodology, the dataset appears to offer substantial potential for further interpretation beyond what is presently explored. In my opinion the paper is suitable for publication in Seismica after some revisions. Although the paper is already in a quite good shape and solid on the methodology, I think the paper would be of higher value and interest if the authors can elaborate more on the preliminary analysis results and the glacier structure and dynamics.

As a general comment, the manuscript emphasizes temporal analysis more strongly than spatial variability. The spatial distribution of event locations could provide additional insight, particularly if compared with available ice thickness models, or maybe any layering in the ice as hypothesized by the authors because of the Rayleigh wave discontinuity observed in the dispersion curves. Including such a comparison would further strengthen the overall interpretation and highlight the value of the proposed methodology.

Further comments:

Line 105: The term “structural imaging” is somewhat unclear in this context. It would be helpful to specify more clearly which methods you are referring to. In addition, this would be a nice place to mention the comparison with surface velocities derived from GNSS, as well as with relevant environmental parameters as part of the study.

Section 3.1.1: How was the crevasse situation addressed during the 2023 campaign? For the 2024 deployment, you note that re-deployment in the 2023 area was not possible due to severe crevassing. Could you clarify whether n 2023 this was possible?

Figure 1: Please provide the source of the satellite image (e.g., Copernicus) and indicate the year of acquisition. In 1a the Outlet station is very hard to note on the figure, I'd suggest to make it larger. Also for me it is not clear on the figure where are the permanent stations and what are all the blue sqaures in this figure 1a? A small legend might be explanatory. And can you also add here the weather station or is that outside this area?

Figure 2: Is the GNSS-derived surface velocity also correlated with the rainfall event? More generally, can you clarify whether the observed variations in seismic power are more closely related to precipitation or to changes in surface velocity? This would be of interest to discuss in the text in more detail.

In the data quality control section (4.1), it would be helpful to also discuss aspects of data quality that are not affected by tilt. In particular, could you quantify how much of the dataset remains usable after accounting for tilt-related effects? And briefly disucss the main noise source contributors also for the 2023 pre-melt season when there is no melt yet that is later on dominating.

Figure 5: The second peak indicated by the red arrow (around 12 September) in the GNSS and power data still appears to overlap. Could you clarify whether differences in sampling rate or averaging influence this comparison? And what averarging is used? in particular, is

the GNSS data sampled or averaged at an hourly resolution, and could this affect the apparent alignment of the signals?

Figure 5a and Section 5.1: It would be interesting to elaborate further on the features observed in the spectrogram. In particular, could you comment on the apparent banding visible below ~2 Hz and its possible interpretation?

Section 5.2: The first line refers to filtering in the 40–80 Hz band, whereas later sections also discuss lower frequencies. Could you clarify this choice and the rationale for focusing on this frequency band? As noted earlier, the lower-frequency range appears to contain substantial information on subglacial processes, including the diurnal influence of surface melt shown in Figure 5.

Lines 400–404: The statement regarding the increase/decrease of velocities is not entirely clear. Please clarify this point in more detail. Do you have an estimate of the ice thickness at this location, for example from the model of BedMachine? This could help to better constrain the interpretation. Additionally, given that the resolution of the dispersion curves is limited below ~2 Hz, I would be cautious about making strong statements regarding changes in Rayleigh wave velocities. Finally, in Section 5.3.2 you suggest that HBO events in 2023 originate at depths of 200–300 m. Does this imply that the bedrock lies deeper than ~200 m at this site? Clarification on this point would strengthen the interpretation.

Section 5.3.1: It would be helpful to briefly explain the motivation for comparing the node and DAS waveforms. Is this primarily intended as a data quality check, or to demonstrate consistency between the two measurement systems? Clarifying this would strengthen the purpose of the comparison.

Section 5.3.2: You note that fewer events are observed in summer 2024. As you mention later, this reduction reflect that events are masked by elevated noise levels associated with melt processes. It would also be helpful to specify the frequency band used for the MFP-based event localization, perhaps more explicitly in Section 5.3.

Figure 7d: The Rayleigh-wave discontinuity around 10 Hz appears somewhat less pronounced than in Figure 6a. Could this difference be related to differences in processing? If so, it would be useful to clarify whether this feature should be interpreted as a processing artefact rather than a physical property.

Figure 10e: I find it difficult to interpret what this panel implies regarding the noise levels in 2024. Are the different deployment periods re-leveled to a common (updated) noise baseline? Please clarify this more explicitly in the text. It might also improve clarity of the corrections to show the summer 2024 event locations using the adjusted BO levels.

Furthermore, it would be interesting to discuss the depth distribution of the relocated events and how they compare with those from 2023 and autumn 2024 in figure 8d-i. Do you observe systematic differences between the seasons that could be related to the active melt season? This could provide a valuable addition to the results or discussion.

Discussion (lines 530–536): You discuss englacial and subglacial hydrology as inferred from the Bartlett outputs, but the link to specific, observable results remains somewhat unclear. It would strengthen the discussion to more explicitly state what can be concluded from the Bartlett analysis. For example, do the quantitative values or patterns provide direct insight into hydrological processes? While this is touched upon in the following paragraph, a clearer and more concrete connection would improve the interpretation.

Line 558: From Figure 7, differences in the Rayleigh-wave dispersion curves are not immediately apparent. It might help to include a plot of the mean curves for the three periods to better highlight potential variations. In the current figure, the y-axis scale is relatively large, making it difficult to distinguish subtle differences. As presented, the statement of significant temporal variation across seasons in dispersion curves and Rayliegh wave velocities appears somewhat strong. In contrast, the variations in the noise PSD are clearly visible. You may consider revising this paragraph to separate these two observations more clearly and to moderate the interpretation of the dispersion results.

Discussion (lines 530–536): You discuss englacial and subglacial hydrology as inferred from the Bartlett outputs, but the link to specific, observable results remains somewhat unclear. It would strengthen the discussion to more explicitly state what can be concluded from the Bartlett analysis. For instance, do the quantitative values or patterns provide direct constraints on hydrological processes? While this is touched upon in the following paragraph, a more concrete and explicit correlation would improve the interpretation.

Section 6.2: It would be valuable to expand the discussion on the comparison with the nodal data (e.g., Figure 7c). What can be concluded from this comparison? In particular, could you elaborate on why DAS is advantageous in this setup? Beyond wavefield characterization, does the high spatial sampling and imaging capability provide a significant benefit? Or would the nodal array alone be sufficient to image the subglacial environment?

Comments on the supplmentary information

S3.2: first sentence: “we relocate” should be “we locate”? Also in the main text you use the ‘relocate’ while I think you locate them for the first instance? Otherwise it feel like you do a second attempt on the location that is already known.

Is the 1800m/s the phase velocity? As a few sentences later you mention 1650 m/s, which is more used as Rayleigh wave velocities in ice. Would be good to specify this.

In Figures S3 and S4, you show periods with tilt followed by intervals without tilt. I would have expected that once a sensor becomes tilted, it would remain so for the remainder of the deployment. Could you clarify how this was handled? For example, were the instruments regularly checked and re-leveled in the field, or is there another explanation for the apparent recovery to non-tilted conditions?

Answers to these reviews are inserted below, following this compilation of text comments.

ROUND 2 REVIEWS

REVIEWER 2

Dear Authors and editor,

The authors adressed my points very well in the revised version of the manuscript. The paper is well written and structured and presents interesting insights on the deployment details and data analysis. So I'd suggest to publish the paper as it is now.

Dear Editor and Reviewers,

Thank you very much for the insightful and constructive comments, which have helped us significantly improve the manuscript. Below, you will find our replies to the reviewers' comments in blue. The line numbers refer to the track-change file.

Best regards,

Nicolas Paris, on behalf of all co-authors

Reviewer C:

Review of Paris et al., submitted to Seismica, reports their extensive seismic observations over the ablation area of Isunnguata Sermia in west Greenland. They analysed seismic data obtained from their dense seismic array and distributed acoustic sensor, and the potential to use seismic signals to study glacier hydrology, ice dynamics, and ice structure. I found that the manuscript is quite well prepared. Here I share very minor comments below. Hope they are helpful to improve your study.

Minor comments

L108 How is this part of the glacier dynamic? Your cloud briefly mentions the ice speed.

We thank the reviewer for the suggestion. We now briefly mention the ice speed in line 125.

L144 and Fig. 1c, I wonder if the bamboo stick might be a cause of noise by vibrating it due to the winds. Do you see any influences?

According to Marusiak et al. (2020) (https://doi.org/10.1029/2021EA002065), a bamboo stick can produce very high-frequency resonances, with the fourth harmonic having the largest energy (in their case, a 1 m free-standing stick produced a fourth harmonic at 320 Hz). They also observed weaker spectral lines corresponding to lower harmonics (90 and 175 Hz). In 2023, the limited sampling rate (250 Hz) made it difficult to assess the influence of stick resonance; however, since the stick protruded only slightly above the ice due to limited surface melt, its influence is likely minimal. In summer 2024, we do not observe the clear spectral lines reported by Marusiak et al. (2020). Given the absence of distinct high-frequency spectral lines and the comparatively weaker amplitude of the lower harmonics, we think that the bamboo stick's contribution to the noise is minimal.

L262 So the model is only forced by temperature and precipitation? Or does the weather station measure wind speed and radiation as well, and use them to force the model?
The model is also forced by radiation and wind speed measured at station KAN_L. The temperature was also measured at this station, but it was further extrapolated away from the station using the surface elevation. We have now explicitly listed all parameters measured at station KAN_L that are used to force the model in lines 267–268.

L369 It might be worth expanding on why 4–6 Hz was specifically chosen as the primary indicator for subglacial hydrology.
We thank the reviewer for pointing out the lack of justification for the chosen frequency band. We selected this frequency band because it is within the range known to be most sensitive to seismic noise induced by subglacial water flow, as reported for instance in Nanni et al. (2021) (https://doi.org/10.1073/pnas.2023757118). We have now briefly justified the choice of frequency band in lines 383–384.

Fig 5c The y-axis tick of surface melt can be placed at the proper value. I don't think that you need to show those small numbers.
We updated the y-axis ticks of Fig. 5c as requested by the reviewer.

L542 It is hard to see if the located event really aligns with crevasses from the plots.
We have added an inset in the top right corner of Figure 8a showing a zoomed-in view of the crevasse field, making the source distribution along surface crevasses easier to observe.

Reviewer G:

Review of the manuscript "Dense Seismic Array Monitoring of the Glacier Tongue of Isunnguata Sermia, West Greenland" by Paris et al. This manuscript presents a dense seismic array experiment conducted on a glacier in Greenland over different seasons. The study provides a detailed description of the deployment strategy and instrumentation, accompanied by a thorough and well-structured data quality control section.

The paper is of clear interest and adds value from a methodological perspective, particularly regarding the design and implementation of large seismic arrays in challenging environments, as well as the associated data processing and quality control approaches. For example the detailed description on how do deal with the sensor tilting is of interest in cryospheric deployment areas. The detailed documentation of the deployment strategy will be especially valuable for future field campaigns in similar settings.

The manuscript is well written and easy to follow. While the current focus is primarily on methodology, the dataset appears to offer substantial potential for further interpretation beyond what is presently explored. In my opinion the paper is suitable for publication in Seismica after some revisions. Although the paper is already in a quite good shape and solid on the methodology, I think the paper would be of higher value and interest if the authors can elaborate more on the preliminary analysis results and the glacier structure and dynamics.

As a general comment, the manuscript emphasizes temporal analysis more strongly than spatial variability. The spatial distribution of event locations could provide additional insight, particularly if compared with available ice thickness models, or maybe any layering in the ice as hypothesized by the authors because of the Rayleigh wave discontinuity observed in the dispersion curves. Including such a comparison would further strengthen the overall interpretation and highlight the value of the proposed methodology.

We thank the reviewer for the constructive and detailed comments, which have helped us improve both the clarity and the interpretations of the manuscript. We have addressed the points raised below, and have revised the manuscript accordingly.

In particular, we followed the reviewer's suggestions to strengthen and, where necessary, moderate our interpretation of the dataset. For example, we now better interpret the relation between rainfall events, surface velocity and seismic power. We have also revised our discussion of the Rayleigh wave dispersion curves to more clearly separate spatial heterogeneity from genuine temporal variations, and to moderate our statements regarding seasonal changes in phase velocity. We also clarified the limits of the Bartlett Output for interpretation of hydrological processes, and wrote a more explicit discussion of the relative advantages of DAS and seismic nodes.

We believe these revisions, together with the other clarifications detailed in our point-by-point responses, have significantly improved the manuscript and we hope it now meets the reviewer's expectations.

<u>Further comments:</u>

Line 105: The term “structural imaging” is somewhat unclear in this context. It would be helpful to specify more clearly which methods you are referring to. In addition, this would be a nice place to mention the comparison with surface velocities derived from

GNSS, as well as with relevant environmental parameters as part of the study.
We agree with the reviewer that the wording was not very clear. "Structural imaging" did not refer to anything specific in this context but was instead intended to mean "seismic imaging" of the structure; we have therefore replaced "structural imaging" with "seismic imaging." As suggested by the reviewer, we have also added a mention of the comparison between seismic power and surface velocities (see lines 105–108).

Section 3.1.1: How was the crevasse situation addressed during the 2023 campaign? For the 2024 deployment, you note that re-deployment in the 2023 area was not possible due to severe crevassing. Could you clarify whether n 2023 this was possible?
In spring 2023, the crevasses were filled with snow, which made fieldwork significantly easier. During summer 2024, however, the crevasses were largely open at the surface, making the area significantly more challenging to navigate. We have now explained this difference in the manuscript (see lines 154–155).

Figure 1: Please provide the source of the satellite image (e.g., Copernicus) and indicate the year of acquisition. In 1a the Outlet station is very hard to note on the figure, I'd suggest to make it larger. Also for me it is not clear on the figure where are the permanent stations and what are all the blue sqaures in this figure 1a? A small legend might be explanatory. And can you also add here the weather station or is that outside this area?
We thank the reviewer for providing us with these suggestions to improve the figure. We have added the source of the satellite image. Because Bing aggregates satellite imagery from multiple time periods, it is difficult to provide a precise date of acquisition. The marker size and font size for the Outlet station label have both been increased for better visibility. A legend has also been added to distinguish the long-term seismic, GNSS, and seismic+GNSS stations shown on the map. Regarding the weather station, it was originally outside the mapped area; we have extended the map extent so that it now appears in the southeast corner.

Figure 2: Is the GNSS-derived surface velocity also correlated with the rainfall event? More generally, can you clarify whether the observed variations in seismic power are more closely related to precipitation or to changes in surface velocity? This would be of interest to discuss in the text in more detail.
We do observe a correlation between surface velocity and rainfall events, as multiple large surface velocity speed-up events occur close in time to rainfall events. However, this correlation is complex, since surface melt also occurs simultaneously, making it difficult to separate their individual contributions to glacier dynamics. The observed variations in seismic power are more closely related to changes in surface velocity than to precipitation. Rainfall likely affects seismic power through multiple mechanisms: a

direct effect from raindrops hitting the surface, and an indirect effect from enhanced surface water flow, which increases glacier dynamics as more water reaches the ice–bed interface. Since rainfall noise is concentrated at frequencies above 80 Hz (Dean, 2019, "The seismic signature of rain," 10.1071/ASEG2018abP068), it is unlikely to significantly affect the power in the 4–6 Hz band that we compare to surface velocity. Thus, the seismic power appears more closely related to the indirect contribution of precipitation to surface velocity, possibly through rain-induced sliding (Horgan et al., 2015, "Glacier velocity variability due to rain-induced sliding and cavity formation," 10.1016/j.epsl.2015.10.016). We mention this briefly in the revised version (see lines 377–379).

In the data quality control section (4.1), it would be helpful to also discuss aspects of data quality that are not affected by tilt. In particular, could you quantify how much of the dataset remains usable after accounting for tilt-related effects? And briefly disucss the main noise source contributors also for the 2023 pre-melt season when there is no melt yet that is later on dominating.
The proportion of usable data was previously indicated in the Discussion section (lines 619–623) of the submitted manuscript. We agree with the reviewer that this information is more relevant here, and we have therefore moved the quantification of the remaining dataset after discarding tilt-affected periods to the data quality control section (see lines 307–312 in the track-change file). We have not identified any obvious source of instrumental noise in 2023 prior to the melt season.

Figure 5: The second peak indicated by the red arrow (around 12 September) in the GNSS and power data still appears to overlap. Could you clarify whether differences in sampling rate or averaging influence this comparison? And what averaging is used? in particular, is the GNSS data sampled or averaged at an hourly resolution, and could this affect the apparent alignment of the signals?
We acknowledge that the previously chosen peak was not the most representative example, as there is, although weak, a peak in surface velocity that overlaps with the seismic power peak. Instead, we now point to September 4, when there is no peak in velocity despite a large peak in seismic power (see updated figure and line 387). The PSDs are computed over 1-hour-long windows (without overlap), whereas the GNSS records position measurements every 10 seconds, which are then low-pass filtered with a 6-hour window (Butterworth filter) before being resampled to 10 minutes by taking the median position. The velocity is then calculated by differentiating this signal, after which a 6-hour moving average is applied. Further details are provided in Section 3.5.1, "GNSS measurements of ice surface motion." All processing was carried out to avoid introducing any phase shift. Thus, although the temporal sampling (1 hour vs. 10 minutes) and temporal resolution (1-hour vs. 6-hour window) differ between the two datasets, there should be no phase shift between the signals. The longer averaging

window used to smooth the GNSS signal could cause us to miss very short-duration speed-up events that may appear in the PSD time series; however, this remains to be investigated, as our focus here is on the overall first order correlation between the two parameters.

Figure 5a and Section 5.1: It would be interesting to elaborate further on the features observed in the spectrogram. In particular, could you comment on the apparent banding visible below ~2 Hz and its possible interpretation?
We do not have a clear interpretation for the banding below 2 Hz. However, since it can be observed outside the melt season, it may not be related to a specific source but rather to the structure. An in-depth analysis should be conducted to investigate this signal further; however, this is outside the scope of this data paper, so to keep the message clear, we have chosen not to comment on it further.

Section 5.2: The first line refers to filtering in the 40–80 Hz band, whereas later sections also discuss lower frequencies. Could you clarify this choice and the rationale for focusing on this frequency band? As noted earlier, the lower-frequency range appears to contain substantial information on subglacial processes, including the diurnal influence of surface melt shown in Figure 5.
Indeed, here the idea of looking at ambient noise correlation functions is to determine which phases are present in the wavefield, rather than to focus on a specific source, which is of interest for seismic imaging. This frequency band was chosen to highlight the fact that high-energy, high-frequency body waves are also present in the ambient noise wavefield (whose reflections can provide valuable information on the ice-bed interface, as discussed later in Section 6.1.2, "Glacier structure"). High-quality ambient noise correlation functions at lower frequencies (which are more commonly retrieved in glacier settings) would not allow us to observe these body waves and would not highlight the quality of the noise correlations as effectively. The first part of the paragraph has been revised to better clarify the choice of frequency band (see lines 391–395).

Lines 400–404: The statement regarding the increase/decrease of velocities is not entirely clear. Please clarify this point in more detail. Do you have an estimate of the ice thickness at this location, for example from the model of BedMachine? This could help to better constrain the interpretation. Additionally, given that the resolution of the dispersion curves is limited below ~2 Hz, I would be cautious about making strong statements regarding changes in Rayleigh wave velocities. Finally, in Section 5.3.2 you suggest that HBO events in 2023 originate at depths of 200–300 m. Does this imply that the bedrock lies deeper than ~200 m at this site? Clarification on this point would strengthen the interpretation.

We have an estimate of the ice thickness from BedMachine v5, which ranges between 350–500 m across the array (line 124). This part of Section 5.3.2 has been revised to include the ice thickness, as suggested by the reviewer, and to soften the statement regarding the change in phase velocity (see lines 418–422).

Section 5.3.1: It would be helpful to briefly explain the motivation for comparing the node and DAS waveforms. Is this primarily intended as a data quality check, or to demonstrate consistency between the two measurement systems? Clarifying this would strengthen the purpose of the comparison.
We thank the reviewer for raising this lack of positioning of our analysis. The motivation here is to evaluate DAS data quality. Surface melt poses the question of the cable-ice coupling, which questions the capability to record the seismic ground motion. The purpose of the comparison is now stated explicitly at the beginning of the paragraph in lines 437–439.

Section 5.3.2: You note that fewer events are observed in summer 2024. As you mention later, this reduction reflect that events are masked by elevated noise levels associated with melt processes. It would also be helpful to specify the frequency band used for the MFP-based event localization, perhaps more explicitly in Section 5.3.
We thank the reviewer for this suggestion. We now specify the 4–6 Hz frequency band used for the MFP at the beginning of section 5.3 in line 427.

Figure 7d: The Rayleigh-wave discontinuity around 10 Hz appears somewhat less pronounced than in Figure 6a. Could this difference be related to differences in processing? If so, it would be useful to clarify whether this feature should be interpreted as a processing artefact rather than a physical property.
We think the main reason for the difference is the difference in spatial coverage. Ambient noise correlations cover the whole extent of the array with large path density in the center of the array. On the other hand the sources used to make the frequency-velocity diagram can be outside of the array, with event-station paths sensitive to regions not covered by the ambient noise correlations. This interpretation is provided in lines 465–467.

Figure 10e: I find it difficult to interpret what this panel implies regarding the noise levels in 2024. Are the different deployment periods re-leveled to a common (updated) noise baseline? Please clarify this more explicitly in the text. It might also improve clarity of the corrections to show the summer 2024 event locations using the adjusted BO levels.
We use different noise levels to generate events with different BO, and then update the noise level to match the ratio between the summer/fall 2024 noise level and the 2023 noise level, such that we can quantify the decrease in BO due to elevated noise level for

a given reference 2023 noise level. We now better explain it in the manuscript in lines 533–544.

Furthermore, it would be interesting to discuss the depth distribution of the relocated events and how they compare with those from 2023 and autumn 2024 in figure 8d-i. Do you observe systematic differences between the seasons that could be related to the active melt season? This could provide a valuable addition to the results or discussion.
We briefly discussed the depth distribution of the sources in the original manuscript (lines 548–550). In this data paper, we do not aim to analyze extensively the spatio-temporal distribution of the seismic sources, as this would require a more in-depth analysis that we leave for future work.

Discussion (lines 530–536): You discuss englacial and subglacial hydrology as inferred from the Bartlett outputs, but the link to specific, observable results remains somewhat unclear. It would strengthen the discussion to more explicitly state what can be concluded from the Bartlett analysis. For example, do the quantitative values or patterns provide direct insight into hydrological processes? While this is touched upon in the following paragraph, a clearer and more concrete connection would improve the interpretation.
While Bartlett Output (BO) is affected by the source mechanism (the phase replica assumes an isotropic mechanism), and variations in BO can therefore reflect different processes involving different mechanisms, the quantitative value of BO cannot provide direct insight into hydrological processes, as it is also influenced by the structure, array geometry, sensor tilt, etc. We have now clarified this point in lines 563–565.

Line 558: From Figure 7, differences in the Rayleigh-wave dispersion curves are not immediately apparent. It might help to include a plot of the mean curves for the three periods to better highlight potential variations. In the current figure, the y-axis scale is relatively large, making it difficult to distinguish subtle differences. As presented, the statement of significant temporal variation across seasons in dispersion curves and Rayliegh wave velocities appears somewhat strong. In contrast, the variations in the noise PSD are clearly visible. You may consider revising this paragraph to separate these two observations more clearly and to moderate the interpretation of the dispersion results.
We thank the reviewer for the suggestions. We chose to keep the figure as is and not to include a direct comparison between the dispersion curves of the different seasons. Such comparison may be confusing and misleading because of the change in the spatial coverage over seasons, such that variations over seasons are not only temporal variations but also spatial variations. We agree that the separation between spatial heterogeneity and temporal variations was poorly drawn in the initial manuscript. We

revised this paragraph to better separate the observed changes in Rayleigh wave phase velocity from the changes in noise PSD and to moderate the interpretation of the dispersion results in lines 591–599.

Section 6.2: It would be valuable to expand the discussion on the comparison with the nodal data (e.g., Figure 7c). What can be concluded from this comparison? In particular, could you elaborate on why DAS is advantageous in this setup? Beyond wavefield characterization, does the high spatial sampling and imaging capability provide a significant benefit? Or would the nodal array alone be sufficient to image the subglacial environment?
The aim of this comparison was to demonstrate that despite uncertain coupling we could retrieve waveforms similar to those obtained with traditional seismic nodes. In this case, the higher spatial sampling of the DAS does not provide a significant benefit over the nodes, mainly because the sledgehammer blows, which generate high-frequency signals, did not yield the expected signal-to-noise ratio. However, DAS can still offer advantages over nodes, notably its ability to measure strain, which cannot be captured by the nodes (though this is outside the scope of this paper), and its ability to record horizontal ground motion, which can be challenging for nodes due to sensor tilt. We revised the first paragraph of Section 6.2 to better highlight the strengths of DAS, although we still believe that using both nodes and DAS together is preferable.

Comments on the supplmentary information

S3.2: first sentence: “we relocate” should be “we locate”? Also in the main text you use the ‘relocate’ while I think you locate them for the first instance? Otherwise it feel like you do a second attempt on the location that is already known.
We agree that “locate” is more suited than “relocate”. We changed “relocate” to “locate” in the revised manuscript (see first paragraph of S3 and S3.2).

Is the 1800m/s the phase velocity? As a few sentences later you mention 1650 m/s, which is more used as Rayleigh wave velocities in ice. Would be good to specify this.
Indeed, the choice of 1800 m/s is for the S-wave phase velocity. This value was chosen to avoid biasing the convergence of the Nelder-Mead algorithm, as we wanted to verify that it converged toward the expected Rayleigh wave phase velocity. We have now stated this explicitly in Section S3.2.

In Figures S3 and S4, you show periods with tilt followed by intervals without tilt. I would have expected that once a sensor becomes tilted, it would remain so for the remainder of the deployment. Could you clarify how this was handled? For example, were the instruments regularly checked and re-leveled in the field, or is there another explanation for the apparent recovery to non-tilted conditions?

The only time a geophone was re-leveled was on July 4, 2024 when we noticed that geophone 1092 was tilted. Other than that, the geophones were left untouched until their recovery. Therefore, the geophones can tilt back and become re-leveled on their own, which we now explicitly state in lines 291–292 of the main manuscript. We do not have a clear explanation for the mechanism that causes the recovery. It is possible that the spikes at the bottom act as a counterweight which facilitates the recovery, but this was not investigated.