Response to the comments of Referee 1

Microbarom radiation and propagation model assessment using infrasound recordings: a vespagram-based approach

By Ekaterina Vorobeva, Marine De Carlo, Alexis Le Pichon, Patrick Joseph Espy, and Sven Peter Näsholm

Dear Igor Chunchuzov,

Thank you very much for your constructive review of the submission. We have made edits to the manuscript according to your comments and suggestions. Below, you can find our point-by-point reply to your report.

Specific comments:

1) *The microbarom model used is based on microbarom generation model that predicts the spatial distribution of the acoustic sources over the ocean surface, and on the atmospheric model that allows one to calculate the microbarom propagation from the microbarom sources to the receivers. Each of these models has its own drawbacks, which introduce errors in the prediction of the parameters of microbaroms at distances of thousands of kilometers from their sources. One of the drawbacks of the propagation model, which the authors themselves pointed out, is the approximation of a horizontally homogeneous atmosphere. The presence in the real atmosphere of the horizontal inhomogeneities in the wind velocity and temperature significantly affects the azimuth of arrival of the signal at the reception point and the prediction of the source back-azimuth.*

Indeed, the approximation of a horizontally homogeneous atmosphere has been made in the model. As you mention, we point this out in the manuscript, especially in the discussion section where we suggest different way to improve the results of simulations.

To make the limitations of this approximation clearer to the reader, changes in Sect. 4 have been made.

2) *Another disadvantage of the propagation model used is that the wind velocity and temperature profiles derived from the European Center for Medium range Weather Forecasting (ECMWF) do not have sufficient vertical resolution to account for the effect of small-scale atmospheric irregularities on microbarom scattering and, as a result, on amplitude attenuation with increasing distance from a source for different directions of propagation.*

This is a very good point. The ECMWF temperature and wind profiles indeed do not resolve small-scale irregularities in the atmosphere. And so far, resolving small-scale structures in atmospheric models, reanalysis and forecasting systems remains a topic for active research. On the contrary, the development and study of methods improving the resolution of atmospheric model's wind and temperature profiles using infrasonic observations are highly pertinent today (e.g. Chunchuzov et al., 2015; Amezcua et al., 2020; Rodriguez et al., 2020).

However, the disadvantage you mention is relevant only if methods requiring atmospheric wind and temperature profiles as an input are used (such as the full waveform propagation modelling or 2D (3D) ray tracing). The semi-empirical attenuation law used in this study accounts for the Veff = V_50km/V_ground ratio, presenting atmospheric conditions above the station which are crucial for detecting the signal. Therefore, wind and temperature values at one specific level are used, and vertical resolution of the ECMWF is not significant.

To make the limitations of the infrasound propagation modelling clearer, we have mentioned them in Sect. 4.

3) *When describing microbarom generation model the authors refer to the state-of-the-art microbarom radiation theory (De Carlo et al., 2020), which "…allows prediction of the location and intensity of the microbarom sources when applied to the Hasselmann integral." It would be important to note briefly in the paper of how are the frequency spectra of counter propagating waves derived in the wave model to calculate the Hasselmann integral, because the latter defines the distribution of the intensity of acoustic sources over ocean surface.*

Thank you for the valuable suggestion. More detailed description of the wave model used has been added into Section 2.2.

4) *The parameters of microbaroms vs time were obtained for the fixed apparent velocity of 350 m/s, which corresponds to the arrivals of the signals from the stratospheric altitudes. Are there in the detected signal the microbarom reflections from the lower thermosphere with another apparent velocity?*

In our study calculations were performed for the fixed apparent velocity of 350 m/s, as you correctly note. To see if there are signals arriving from higher altitudes, for example from the lower thermosphere, the calculations need to be done for higher values of the apparent velocity (Lonzaga, 2015). These calculations are outside of the scope of the current research. However, Näsholm et al. (2020) demonstrated that mesospheric - lower thermospheric (MLT) arrivals originating from Iceland / Greenland hot-spot can be detected at IS37 in summer, but only if signal processing removes stratospheric arrivals from other directions such as Pacific / Barents Sea.

5) *The amplitude obtained from the model in Fig.2a (red) in the time interval 200-201 DOY is two orders lower than the amplitude obtained by vespa processing. Could you explain the cause of such discrepancy?*

There could be various reasons explaining the discrepancy in Fig. 2a, e.g. an error in the wave model or in atmospheric winds causing an overestimation of the attenuation using the semi-empirical law. From Fig. 2f we can see that the modelled dominant direction is shifted a little bit towards the north when the discrepancy occurs, while there is no evident shift in Fig.

2j(g) for the vespagram. Therefore, that could indeed be a wind issue, and we are looking at signals originating from different sources.

The corresponding explanation has been added to Sect. 3.1.

6) *Fig. 2f and Fig. 2j: Are the amplitudes (model and vespagram) in these Fig. s normalized by the maximum amplitude?*

Yes, Fig. 2 (e, f, j) and Fig. 3 (e, f, j) present amplitudes normalized by the maximum amplitude at each time step. We have now clarified this in Sect. 3.1 and in the caption of Fig. 2.

7) *The last expression in the right side of (3) defines rather a mean squared error (MSE), than a similarity index (SI), since this expression becomes zero (not 1) in case of full match between model and infrasound vespagram.*

Thank you for the comment, the typo in the right side of (3) has been corrected. Now (3) is as follows: SI = 1 – MSE = 1 – (1/N) Σ (P_model – P_vespa)^2.

8) *Line 185: "Going to higher frequencies, there is a pronounced change in the dominant direction of the source from the Atlantic in winter to the Barents Sea in summer (Fig. 3)." Do the higher frequencies react stronger on the change of wind direction in the stratosphere from eastward to westward than the lower ones? If yes, then why?*

In case of the low frequencies (0.1 – 0.2 Hz), there is a limited number of possible oceanic sources. To generate infrasound at such low frequencies, the source need to have a substantial size. In Fig. 2j(g) one can also see a change in the dominant source direction in summer. Signals coming from NE and SE are interpreted as those from the Pacific and the Indian oceans (see point 12 in the reply to R2 comments). However, this effect is more pronounced for the higher frequencies. The possible explanation could be the distance between IS37 and ocean sources. The North Atlantic microbarom source is located much closer to the station than the Pacific and the Indian oceans (~3000 km vs ~8000 km). Propagating over such a long distance, the attenuation might be crucial and lead to the signal to be below the noise threshold. This can also explain the reason why many data points have been ignored in the infrasound vespagram (Fig. 2j(g)) during summer (see point 11 in the reply to R2 comments).

Thank you for taking the time to review our submission, we believe that your advice have helped to clarify the manuscript.

Your sincerely,

Ekaterina Vorobeva, on behalf of all authors

References

Amezcua, J., Näsholm, S. P., Blixt, E. M., and Charlton-Perez, A. J.: Assimilation of atmospheric infrasound data to constrain tropospheric and stratospheric winds, Quarterly Journal of the Royal Meteorological Society, 2020.

Chunchuzov, I., Kulichkov, S., Perepelkin, V., Popov, O., Firstov, P., Assink, J., and Marchetti, E.: Study of the wind velocity-layered structure in the stratosphere, mesosphere, and lower thermosphere by using infrasound probing of the atmosphere, Journal of Geophysical Research: Atmospheres, 120, 8828–8840, 2015.

De Carlo, M., Ardhuin, F., and Le Pichon, A. (2020). Atmospheric infrasound generation by ocean waves in finite depth: unified theory and application to radiation patterns. Geophysical Journal International, 221, 569–585, https://doi.org/10.1093/gji/ggaa015

Lonzaga, J. B. (2015). A theoretical relation between the celerity and trace velocity of infrasonic phases. The Journal of the Acoustical Society of America, 138(3), EL242-EL247.

Näsholm, S. P., Vorobeva, E., Le Pichon, A., Orsolini, Y. J., Turquet, A. L., Hibbins, R. E., Espy, P. J., De Carlo, M., Assink, J. D., and Rodriguez, I. V. (2020). Semidiurnal tidal signatures in microbarom infrasound array measurements, in: EGU General Assembly Conference Abstracts.

Rodriguez, I. V., Näsholm, S. P., & Le Pichon, A. (2020). Atmospheric wind and temperature profiles inversion using infrasound: an ensemble model context. The Journal of the Acoustical Society of America, 148(5), 2923-2934.

Response to the comments of Referee 2

Microbarom radiation and propagation model assessment using infrasound recordings: a vespagram-based approach

By Ekaterina Vorobeva, Marine De Carlo, Alexis Le Pichon, Patrick Joseph Espy, and Sven Peter Näsholm

Dear Referee 2,

Thank you very much for your constructive review of the submission. We have made edits to the manuscript according to your comments and suggestions. Below, you can find our point-by-point reply to your report.

Specific comments:

1) *Consider revising the title of your manuscript a little (see general comment).*

Thank you for the suggestion. The manuscript title has been changed to "Benchmarking microbarom radiation and propagation model against infrasound recordings: a vespagram-based approach".

2) *l. 16: Why do microbaroms return to the ground after penetrating the middle atmosphere (hence their potential to probe the middle atmosphere dynamics)? Briefly explain the underlying physical process.*

The explanation of the infrasound waves refraction in the middle atmosphere has been added to Sect. 1.

3) *l. 42: how did you determine the fixed apparent velocity of 350 m/s – from observations (using other processing techniques?) or propagation modeling (average?), or is this based on previous studies (references available? - obviously yes, but these are not cited before line 87/88). For the discussion of the results (e.g., line 205): using this fixed value, what is the corresponding standard deviation of observations at IS37 (e.g., using PMCC)? Based on this, can you roughly quantify the number of other arrivals (especially in the summer) that potentially cause discrepancies?*

This is a good point. An explanation of the choice of the fixed apparent velocity value has been added to Sect. 1. We find the comparison with the PMCC method to be beyond the scope of this article. Hence, all interpretations and explanations are based on discrepancies between the microbarom model outputs and vespagrams.

4) *l. 75 and Fig. 1a: you could add the ARCES array to the map as this is mentioned in the text as the initially planned site for IS37; however, I am wondering if the first part of the sentence (“was initially planned … in Karasjok”) is worth to be mentioned at all. This fact is not relevant to your study but raises the question of why it was less favorable. Therefore I recommend shortening the paragraph accordingly.*

Thank you for the suggestion. The sentence in l.75 has been changed following your comment.

5) *l. 125/126: it is not necessary to repeat all references, the choice of 350 m/s was justified before; I suggest removing the second part of the sentence (beginning with “which is within …”).*

The sentence has been corrected according to the suggestion.

6) *l. 136: Landès et al. (2014) studied the global patterns of microbaroms and only discuss the potential limitations due to the lack of coastal reflections while citing Hillers et al. (2012), among others. Therefore citing that study in the way it is done here is a bit misleading. My suggestion is to modify this and add another sentence, for example: “Studies on microseisms (e.g., Hillers et al., 2012) have demonstrated the limitations of a model that does not account for coastal reflection. These limitations have been accordingly raised in the context of microbaroms (Landès et al., 2014).”*

Thank you for the comment. The paragraph has now been modified following your suggestion.

7) *l. 153/154: which of the ECMWF models in particular? If not the ERA5 reanalysis, did you interpolate the temperature and wind fields in time?*

The ECMWF high Resolution (HRES) model has been used. The temporal resolution of this model is 6 hours which is twice WWIII time step. Therefore, to avoid possible discrepancy caused by interpolation in time, the assumption of the constant wind and temperature fields over 6 hours was made.

Sect. 2.2 has been updated to clarify questions related to the ECMWF model used.

8) *l. 163: remove the parenthesis (private communication with …), M. De Carlo is co-author of this study. Instead, how would the results differ if you accounted for only 3000 km? (Is it essential to account for 5000 km for providing a more realistic spectrum at IS37?)*

The choice of the maximum distance from the station depends on the location of the station and the main sources, as well as on how realistic spectrum is needed for a specific task. The recent study by De Carlo et al (2021) demonstrated a comparison of global microbarom patterns between the PMCC and the microbarom model by De Calo et al. (2020a) used in this study. The calculations have been performed using the maximum distance of 5000 km obtained from averaging over 45 IMS stations and providing more realistic spectra. The analysis reveal a good agreement between the PMCC and the model with the 5000 km cut off distance. Based on results of the aforementioned studies, we use the model configuration that provides the best estimate of microbarom spectra. Sect. 2.2 has been updated to clarify questions related to the choice of the maximum distance.

9) *Section 3.1: Here you present a lot of information (3 figures within more than 24 panels!) within the first paragraph, without much explanation. You could help the reader by focusing on Figs. 2 and 3 first. Also, I suggest that you already define Eq. 3 in Section 2; then all panels can be understood at the first occurrence of a figure in Section 3.*

Thank you for the suggestion. Changes in Sect. 2 and Sect. 3.1 have been made according to your recommendation.

10) *According to Section 2.1 step 5, the vespa output should be power (Pa^2), whereas in Figs. 2 and 3 the colorbar unit is Pascal (amplitude) again, correct? (also, place the units/labels to the right of the colorbars – amplitude in Pa).*

This is correct, the vespa output is power (Pa^2). We have used Pascal unit in Figs. 2 and 3 hoping that this will help the reader to get an intuitive sense of the pressure amplitude. The corresponding explanation has now been added into Sect. 3.1 as well as in Fig. 2 caption. The units/labels in Figs. 2 and 3 has been moved to the right of the colorbars.

11) *Fig. 2b-d: in the summer, infrasound amplitudes at IS37 seem to be not relevant, whereas for the comparison (Fig. 2h) and through normalization (e-j) they certainly are (e.g., lower SI). Would a logarithmic color scale be useful in b)-d)? What is the impact of the detection threshold (noise level) of the station, especially for the summer season comparison – could this explain parts of the discrepancy between model and vespa in Fig. 2a?*

In summer, infrasound amplitudes at IS37 are indeed lower than in winter. However, we believe they are still relevant. The normalization at every time step facilitates interpreting and comparing the directional spectra between data and model (Figs. 2 and 3 e – j (g now)). The main parameter influencing SI is the difference between the model's and vespagram's directional spectra. In winter, when atmospheric conditions are favorable for the stratospheric ducting from the west to the station, the assumption of a horizontally homogeneous atmosphere in the model doesn't affect the results as much as in summer, and the model and vespagrams are in a better agreement. However, in the summer or during SSW events, this assumption is not so valid and the effect of winds along the propagation path needs to be considered to a greater extent. This results in a large difference in directional spectra and, as a result, in lower SI values.

A corresponding explanation has been added to Sect. 3.1. See also point 14) for more details.

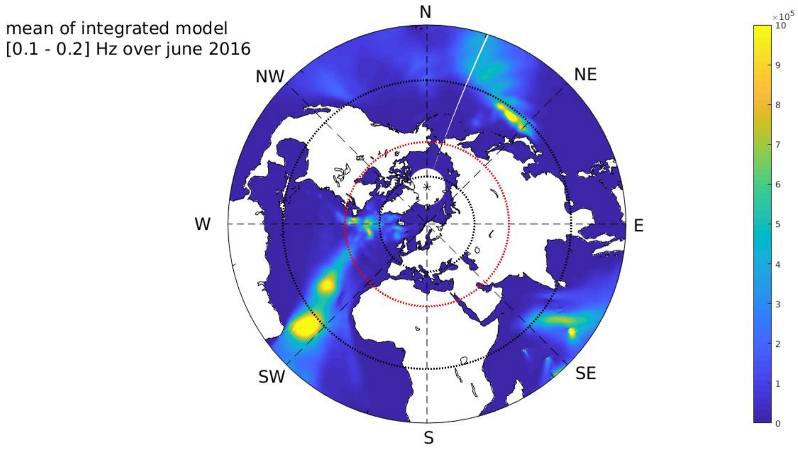
We cannot directly account for the station detection level since we are calculating the power for different directions using a sliding time window – without applying trigger-based event detection approaches.

However, after the vespa processing is done, we apply a quality check threshold based on the vespagram spectrum properties. At the time when vespa processing predicts a directional spectrum with the power (almost) equal in all directions, data are ignored. This is especially pronounced for the 0.1 – 0.2 Hz band during summer (see Fig. 2 g).

Changes in Fig. 2 has now been made in order to highlight the lack of data in the summertime.

12) *Fig. 2j: One can recognize spots of maximum normalized power from south-easterly directions in the summer (not represented by the model). What could be their origin? There are probably not many potential sources in that direction (especially not for low frequencies).*

You are right, there are not so many potential sources in SE direction that would provide so low frequency microbaroms. This could be microbaroms generated in the Indian ocean. The microbarom model's map for June 2016 supports this hypothesis (the dot circles are located at 3000, 5000 (red) and 8000 km from IS37).



The stratospheric summertime westward wind could guide the infrasound waves towards the IS37 station. The distance between the station under consideration and the Indian ocean is much larger than 5000 km (around 7000 – 8000 km) which is the model's cut off limit. Therefore, these arrivals are not presented by the model.

The corresponding explanation has been added to the discussion of Fig. 2.

13) *Fig. 3d and particularly 3j: The vespagrams exhibit some horizontal lines (e.g., E and NW). Could these be artifacts of the vespa/beamforming processing?*

Indeed, sidelobes in the steered response can appear as an inherent effect of array geometry. Still, it is maybe not so straightforward to find the source of those lines. Additional power peaks that arise in the vespa processing represent side lobes appearing when extracting power values along the fixed apparent velocity circle (Fig. 1b). As can be seen from Fig. 1b, for lower frequencies we have less side lobes. Since Fig. 3 considers the 0.5 – 0.6 Hz band, the number of side lobes is higher, but their amplitude is several dB lower than the main lobe. More importantly, for any side lobes related effect, the position of the "lines" would change over time, staying approximately at the same angular distance from the dominant signal direction.

Therefore, we lean towards not believing this is a result of the vespa processing and assume that those lines could present some stable local background sources of infrasound with frequencies within the microbarom range.

14) *l. 180 and Fig. 4: the median differences in direction of max. power are about 0-2 degree lower (by eye inspection) when using the smoothed model; the trend favoring the smoothed model is clearer recognized in the uncertainty ranges. However, if these uncertainties also correspond to the difference at the maximum power only, these are relatively large (not only at low frequency but also at the highest frequency band). How would you explain this?*

Thank you for this question. Both medians and uncertainty ranges in Fig. 4 are estimated based on the back-azimuth difference at the maximum power only. Thanks to your request, we have checked the calculation procedure and found an error in the calculation for the lowest frequency band. The calculation results for the remaining frequency bands remain unchanged.

An updated version of Fig. 4 can be found in the manuscript.

Uncertainty values falling between 25 and 75 percentiles are an objective assessment of the discrepancy between the model and vespagrams. These values originate from the wintertime when atmospheric conditions are favorable for stratospheric ducting from the West. In summer, atmospheric conditions are not so stable and there are several factors that can cause discrepancies as we mention in l. 200 – 210. The vespagram-based approach, in turn, is very sensitive to atmospheric changes opposite to the model which uses only atmospheric conditions at the station to access the possibility of a wave front arrival. Therefore, summer arrivals predicted by the model look more stable than those predicted by vespagrams (see Fig. 3). The difference between the direction of max in the summertime can reach up to tens of degrees, for example when the model predicts arrivals from the Barents Sea and the vespagram predicts arrivals from the North Atlantic (Fig. 3 around day 210 in 2016). This also causes a fall of the similarity index.

A corresponding explanation has been added to Sect. 3.1.

15) *Eq. 3: Please check if the equation is correctly noted. According to my understanding, the right-hand side is the definition of MSE(t). In this case, the equation should be modified to SI=1-MSE=1-(1/N)… or SI=1-MSE with MSE=(1/N)…*

Thank you for the comment, the typo in the right side of (3) has been corrected. Now (3) is as follows: SI = 1 – MSE = 1 – (1/N) Σ (P_model – P_vespa)^2.

16) *Eq. 3 / Figs. 2&3 / model output: The vespa analysis is done at a time step of 30min (1h time window), but the time step of the p2l data is 3h; do you interpolate the microbarom model output to 30min while smoothing or integrate the vespa over 3h? Do the time series in Figs. 2a and 3a (and b-g) differ in temporal resolution? What is the temporal resolution of the similarity index? Please briefly clarify in the manuscript.*

Thank you for this question. In this study, in order to avoid the model output's interpolation in time, the vespa processing output has been sub-sampled to match the three hourly microbarom model grid. Further, all results are presented with the time resolution of 3 h. The corresponding description has been added to Sect. 2.3.

17) *l. 193-194: consider rephrasing this sentence towards SI instead of MSE; also, once SI has been defined in Section 2 (see comment 9), use SI for the axis labels in Figs. 2, 3, and 5, rather than 1-MSE.*

Corresponding corrections have been made in Figs. 2, 3, 5 as well as in Sect. 2.3.

18) *l. 200: An SI of 0.5 corresponds to an MSE of 0.5, but the absolute difference between vespa and model must be even larger (and thus quite large!), due to the squared nature. In other words: For normalized distributions (within [0,1]), the MSE heavily weights small discrepancies instead of significant outliers, as opposed to when the absolute values exceed 1. Have you already contemplated using the mean absolute error instead?*

The calculation of SI based on normalized distributions is justified by the significant effect of smoothing procedure on modelled amplitudes. Comparison of the unsmoothed model with the vespa calculation results is not used, because the model does not account for the frequency-dependent resolution of the infrasonic array. The mean square error calculation is a widely used approach that allows a comparison between two statistical models. Therefore, MSE can represent the difference between the actual observations and the observation values predicted by the model.

Following your advice, we have now explored using the mean absolute error (MAE) instead. The main conclusion from that experiment is that using the MAE doesn’t significantly change the results or conclusions based on them. For this reason, the SI calculation procedure has not been changed in our manuscript.

19) *Fig. 5: how are the data within a 3-day interval handled (mean/median, discrete)?*

The data in Fig. 5 are presented as a discrete set with 3-day step, namely, day 0 00 hours, day 3 00 hours etc. The median is presented in the last panel only. The corresponding explanation has been added to the manuscript.

20) *l. 239/240: “usually appears earlier” (3-24 hours) – this applies only to 2017 (and 2016), doesn’t it?*

This applies to all years under consideration depending on the frequency band used. Fig. 6 presents the results for 0.3 – 0.4 Hz band where, as you correctly mention, this applies to 2016 and 2017.

The corresponding explanation has been added to Sect. 3.2.

21) *l. 250: "resulting *in* model-vespagram discrepancies" – Can you quantify these discrepancies caused by ECMWF wind along the infrasound path?*

These discrepancies have been already quantified in l. 238. L. 250 has been changed to "resulting in the above-mentioned model-vespagram discrepancies".

22) *l. 252/253: Do vespagrams perform better than other methods such as PMCC in the context of SSW events? I am aware that this is not your point here. Nevertheless, in other sections, you correctly highlight the advantage of the vespa approach (all directions simultaneously), whereas in Fig. 6 you compare the back-azimuths of the dominant signals only – which are likely similar to the output of PMCC.*

As mentioned in the initial part of Sect. 3.2, studying the behavior of SSW events is not the main objective of the study. The main point of this section is rather to examine the ability of the vespagrams to detect extreme atmospheric events and see if there are significant discrepancies with the model. We considered changes in the back-azimuths of the dominant signals only in Fig. 6 because this is one of the infrasound signatures of SSW events. Moreover, such approach is one of the few ways to present vespagram and model output in the same plot.

It is impossible to present two colorbar plots in one. However, trying to follow your advice, updates have been made in Fig. 6. The figure now presents the microbarom azimuthal distribution at IS37 estimated by vespa and normalized per time step, as well as the back-azimuths of the dominant signals predicted by the model (red dots). Comparison of the vespagrams and the PMCC is not within the scope of the current study but could serve as an idea for future studies. The model by De Carlo et al. (2020a) has already been compared with the output of PMCC for multiple stations including IS37 (De Carlo et al., 2021).

Technical corrections:

- De Carlo et al. (2020) reference: this is not unique, there are two entries in the list matching this citation! Add a/b letters.

Thank you for spotting this. We have made the associated corrections.

- l. 8: revealed --> reveals

Corrected.

- l. 9 - add "events": sudden stratospheric warming [events].

Corrected.

- l. 16 - remove "back" (return or turn back are both appropriate, but return back looks like a tautology)

Corrected.

- l. 69 - Blanc et al. (2018) was referenced in the sentence before, could be saved here

The reference has been removed from l. 69.

- Fig. 1b - Sx/Sy = slowness components (I suggest you add this information to the caption, it is not defined in the text)

A definition of *Sx/Sy* has now been added to the caption of Fig. 1.

- l. 108: of the incoming signal

Corrected.

- l. 111 - remove "a" (or add a noun such as "approach" after "applied")

The article "a" has been removed.

- l. 124: to the square root

Changed according to the suggestion.

- l. 134 - the WW3 reference is missing in the bibliography

The reference has been added to the bibliography.

- l. 136: […] as described by Ardhuin et al. (2011).

Corrected.

- l. 153: assess --> determine ("assess" is also used in the next sentence)

Changed.

- l. 154: Forecasting --> Forecasts

Corrected.

- l. 167: resolution of array --> array resolution

Corrected.

- *l. 174/175 - rephrasing suggestion: Figures 2a and 3a show the maximum amplitude per time step over one year, i.e. the dominant signals in the azimuthal spectra*.

This sentence has been rephrasing according to your suggestion.

- *l. 178: accompanied with --> accompanied by [the] (or: combined with the)*

The phrase "accompanied with" has been replaced with "accompanied by the" following the suggestion.

- *l. 179 - "applying" is redundant*

The word "applying" has been removed from the sentence.

- *Fig. 2/3 - j) should be g) in order to avoid confusion when reading the caption (e.g., e-j)*

Thank you. The index j) in Figs. 2 - 3 have been changed to g).

- *Fig. 2 caption - I assume that panels 2-4, 6, and 7 are b-d, f, and j(g), correct?*

This is correct. The numbers in the caption have now been replaced with the letters to avoid confusion.

- *Fig. 2 caption: similarity score --> similarity index (Eq. 3)*

This caption has now been corrected.

- *Fig. 2 caption: the colormap reference is also given in the acknowledgments of the manuscript; consider removing it from the caption to focus on the essentials*.

This reference was initially only mentioned in the acknowledgements. However, during the preprocessing of the manuscript by the journal, the editorial support team kindly asked us to add the image credit to the corresponding figure caption(s). Hence, although we would prefer to follow your advice, we opt to stay with what was requested by the editorial. Still, in order to avoid repetition, we have now removed this reference from the acknowledgments.

- *l. 188/189 - why do you use negative back-azimuths instead of 266° (265°), 239° (245°), and 26° (34°), respectively? Please also add the degree symbol (unit).*

The negative values of the back-azimuth have been used in order to keep 0° (or the North) in the middle of (-180°, 180°) interval. However, following your advice, we have changed negative values to positive and have added the degree symbol.

- Fig. 4 - please add a unit to the y label (°); the figure size could be smaller in the final version (width of one column)

Thank you for the recommendation. The unit (°) has been added to the *y* label in Fig. 4. The figure size will be changed by the editorial office later.

- l. 202: in the Arctic

Corrected.

- l. 212: promising

Corrected.

- l. 213: the analysis

Corrected.

- Fig. 5 caption: Multi-year comparison between vespagrams and smoothed modelled microbarom soundscapes at the IS37 station.

The caption has been changed according to the suggestion.

*- Fig. 5: could you include the legend of the last panel *inside* this panel? Consider using different colors for this panel.*

Fig. 5 has been updated following your suggestion.

- l. 235: […] until late March or early April, which corresponds […]

Corrected according to the suggestion.

- Fig. 6 caption: days --> onset days

Changed.

- l. 243: […] addressed by Diamantakis (2014) and Smets et al. (2016).

Corrected.

- l. 249: […] demonstrated by Evers and Siegmund (2009) and Smets and Evers (2014) that […]

Corrected.

- *l. 270/271 - rephrase this sentence*

The sentence has been rephrased.

- *General technical remark: no space between number and % as well as °N, °E, …*

We consulted the journal guidelines at https://www.annales-geophysicae.net/submission.html#manuscriptcomposition), and found that manuscripts shall include a space between number and % as well as between ° and N.

Looking at the final typeset version of other ANGEO papers, it looks like these have a reduced-width blank between number and symbol – so this will hopefully come out visually pleasing also in our final product.

- *General grammatical remark: I think you should add articles to a number of nouns*.

Thanks for this advice. The grammar has been double-checked.

Thank you for taking the time to review our submission, we believe that your advice have helped to clarify the manuscript.

Your sincerely,

Ekaterina Vorobeva, on behalf of all authors

References

De Carlo, M., Ardhuin, F., and Le Pichon, A.: Atmospheric infrasound generation by ocean waves in finite depth: unified theory and application to radiation patterns, Geophysical Journal International, 221, 569–585, https://doi.org/10.1093/gji/ggaa015, 2020a

De Carlo, M., Hupe, P., Le Pichon, A., Ceranna, L., and Ardhuin, F.: Global Microbarom Patterns: a First Confirmation of the Theory for Source and Propagation, Geophysical Research Letters, 48, e2020GL090 163, https://doi.org/https://doi.org/10.1029/2020GL090163, 2021