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Interactive comment on “Microbarom radiation and propagation model assessment using infrasound recordings: a vespagram-based approach” *by* Ekaterina Vorobeva et al.

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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) SUM (P_model – P_vespa)ˆ2.

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 ($\sim$3000 km vs $\sim$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 advices have helped to clarify the manuscript.

Your sincerely, Ekaterina Vorobeva, on behalf of all authors

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Please also note the supplement to this comment:
https://angeo.copernicus.org/preprints/angeo-2020-78/angeo-2020-78-AC1-

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