

Interactive comment on “Characterizing the global ocean ambient noise as recorded by the dense seismo-acoustic Kazakh network” by Alexandr Smirnov et al.

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In this paper, the authors present multiple years of microbarom and microseism observations on a dense array network in Kazakhstan. The microbarom observations (time series of back azimuth and amplitude as a function of time) are compared with numerical simulations. The authors claim that additional knowledge can be obtained by considering a network of arrays, instead of single array studies, such as done in earlier work.

The authors suggest that this network can be used to develop synergetic approaches to better constrain microbarom source and evaluate propagation effects.

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Discussion paper



I much like the approach that the authors take in this study and I believe that this paper would be a great addition to the scientific literature. However, before acceptance there are several issues that should be resolved by the authors. Therefore, I would like to recommend a major revision. In particular, I would like the authors to consider the following critiques:

1. I missed in the abstract and introduction some discussion on the novelty of this study: e.g. what the added value is of a characterisation with seismic and acoustic arrays that are part of a dense network. In the conclusion, the authors claim that analyzing multi-year archives of continuous recordings yields additional information about the spatial and temporal variability of the ambient noise originating from two hemispheres. This is an interesting aspect, but in my opinion the manuscript does not provide sufficient evidence for that.

2. A shortcoming is the lack of microseism predictions. Certainly since these simulations can be produced by the same model. Please add these to a revised manuscript.

3. I also missed a more direct comparison of microseism and microbarom observations, e.g. MKIAR/MKAR and KURIS/Kurchatov

3. Some figures are missing where others are superfluous. In particular, I consider that Figures 1, 2, 4 and 5 could be combined in one single figure. I missed figures that (1) show a map of the distribution and characteristics (amplitude/dominant frequency) of microbarom and microseism sources that are considered in this study (also from the southern hemisphere?) and (2) spectral characteristics of the observations, i.e. Probability Density Functions of the Power Spectral Densities for winter and summer months, for all arrays considered.

4. I would like the authors to address spelling and grammatical errors. I have included a few suggestions and have included a rephrasing occasionally.

5. I would like the authors to discuss the shortcomings in the current method (data

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processing, range-independence) in a revised version of the manuscript. In particular, the used array processing method is known to produce biased results when the signal consists of multiple, concurrent sources (the case when studying microbarons).

These, and other minor points can be found in the annotated PDF.

Best regards, Jelle Assink

Please also note the supplement to this comment:

<https://se.copernicus.org/preprints/se-2020-8/se-2020-8-RC2-supplement.pdf>

Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2020-8>, 2020.

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