Solid Earth Discuss.,
https://doi.org/10.5194/se-2020-8-AC2, 2020

Solid Earth
Discussions

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

Alexandr Smirnov et al.

infra.smirnoff@gmail.com

Received and published: 6 July 2020

Dear Jelle,

Thanks a lot for the benevolent appraisal of the paper in its current state, for the time spent for the review preparation, and for the constructive suggestions. We are agreeing with your remarks. Only clause 2 seems to be a point for discussion, the details are below. If you don't mind we are ready to start the paper correction as it is specified below for each item:

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 characterization with seismic and acoustic arrays that are part of a dense network.

We will add the discussion.

In the conclusion, the authors claim that analyzing multiyear 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.

We will exclude the item from the conclusions.

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.

You are right, it is possible to produce these simulations using the same source model. The only difference with the microbarom source is the need to take into account the influence of the bathymetry effect. These simulation has been already done, the results were included in our talk at EGU 2020 https://doi.org/10.5194/egusphere-egu2020-2965. This material was not included in the paper in purpose as authors are going to prepare another paper based on the same observational data but dedicated mostly to seismic data. Microseism simulation would be the focal point in this new paper. We propose to clearly mention this effect and evaluate its impact on the modeling

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

It is a very good point and we will add the comparison.

4. 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.

We will change the figures and add the map and the PDFs. As the first referee also suggested the changes we will try to find some reasonable compromise.

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

This is the most difficult point! We will do our best and thanks again for your editing.

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

We will add the discussion.

Kind regards

Alex

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