Review report on « Near surface structure of Sodankylä area in Finland, obtained by passive seismic interferometry " by Nikita Afonin et. al.

The deduced results are interesting and can be considered for publication. provided some areas can be improved. The authors applied seismic interferometry to passive seismic data to retrieving surface-wave and to image the near subsurface structures related to mineral exploration. They used SNRS as part of the processing procedure to retrieve the green function. However, it is not clear to me how and why they used this algorithm. They also performed synthetic tests which the goal is unclear to me. Although the resulted 2D sections of shear-wave velocity models from inverting of the dispersion curves and the geological interpretations are very interesting, there are some points that need consideration.

My major concerns are the following:

1- In general the paper has a good structure and is well written but there are some (unnecessary) statements in the abstract, introduction, and also other sections that need to be rephrased on removed to improve the manuscript (ms). Also, there are some typos that need to be checked.

2- Lines 116-117: I can't see how they concluded about the distribution of the sources from PSD of the signals in Fig. 2. Usually, for different frequency band that will be used in a study, we apply a beamforming or FK analysis to locate the main source but I don't see such an analysis and there are just some statement in the text that is not enough in my opinion.

3- Line 170: I can't see any spectral peaks at those frequencies in Fig. 2. I think the figures cross-referencing in the whole ms need some improvement as there are some sentences without (correct) cross-referencing to the corresponding figure.

4- Line 178-187: I do not understand how the numerical simulations results address any of the problems and how it helped the authors as the results demonstrations in Figs 6 and 8 is different. They didn't show the cross-correlation results for the single source. And to me, it's not clear how this helped the authors as they didn't apply their processing technique to the simulated data. And there is no analysis of how applying SNRS improves the results. I understand it has been explained in another earlier paper but it would be useful, if possible, to do the comparison for this simulated data as well.

5- Line 200: In the processing, you mentioned 1-100Hz bandpass filtering but in all the figures for the dispersion analysis you only used frequencies <50, is there any specific reason to use 1-100 and not 50Hz for filtering? And in lines 245, what did you use to eliminate the surface-wave? Is it a notch filter?

6- Line 263: Where are the drilling locations? I couldn't find any map about their location.

7- Lines 268-270: I don't understand the meaning of this paragraph, usually the non-stationary phases increase the apparent velocity so there is no need for this statement.

8- Line 287: Again, I can't see how you concluded if the noise sources are isotropic or not?

9- Could you please explain why you used different packages/codes for dispersion curve calculation for different parts of the data? Somewhere you used "Geopsy" and then changed to "MASW"? I know they both do the job but maybe it would be better to be consistent if you want to compare the results.

10- General suggestion on figures: Maybe you can combine Figures 6 and 8 for a better comparison. There is no need to plot the particle motions. Use a scalebar for some of the figures is possible.

Hope these questions and comments help in the improvement of ms.
Sincerely
M. Rezaeifar