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Interactive comment on "On the Green's function emergence from interferometry of seismic wavefields generated in high-melt glaciers: implications for passive imaging and monitoring" *by* Amandine Sergeant et al.

Anonymous Referee #2

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Sergeant et al. present an overview of different methods used to estimate structural information about glaciers and an ice sheet using seismic waves. The main focus of the study is on estimating the direct Rayleigh wave between seismic station using passive recordings of ambient noise and (near-surface) icequakes. The authors present multiple methods (traditional correlation, MFP, MDD, etc.) and highlight the benefits and limitations of each method. The overall goal is to show the usefulness of high-density seismic arrays deployed on ice to image the structure directly beneath the ice, as well as monitoring changes in this structure through time.

The authors demonstrate each of the proposed methods, albeit using dataset from different glaciers. All of the results are quite convincing, but I do question some of the interpretations about why certain features exist in the recovered wavefields. I have listed those below in the comments. In general the paper is well written and clear. In some aspects the text needs to be tightened up though. Statement are not entirely complete or not entirely accurately described. The figure fonts can all be enlarged as well. In general, the actual results in all methods are very nice and I congratulate the authors on recovering such nice GFs from icequake and glacier noise data. This is not easy.

I have listed major comments/concerns below and provide an annotated PDF with many more minor comments.

General:

Paragraph around line 50: Interferometry recovers an approximation to the Green's function/impulse response. There are many assumptions that influence the accuracy of this approximation. This should be better explained or at least noted. Also, the description of the causal and acausal parts of the GF estimate should not be limited to only the direct wave (as is currently done). The more accurate way to characterize what is happening is to describe virtual sources. The direct wave is commonly observed because not all of the assumptions in SI are valid in most field data studies. This paragraph as written is too simplistic and not an accurate depiction of the theory. Please revise to be more complete.

Paragraph starting at line 55: a diffuse or equipartitioned wavefield is not the same thing. Equipartition means that all modes are excited (P,S,Rayleigh,Love,etc.). Diffuse means waves propagating in all directions. This distinction is commonly neglected by most people that write about SI. The current description in this manuscript again confuses these two distinct properties of the wavefield. Please revise throughout the manuscript.

Line 60: The statement that strong scattering exists in the crust is not true at the frequencies commonly used for ambient noise tomography. This statement is very much untrue. At high frequencies (>0.5 Hz) this statement is true, but ambient noise imaging often works because oceans generate the microseismic wavefield all around earth so it is more accurate that condition (i) is met. See the following reference and the papers that have since referenced this paper. Mulargia, F. (2012). The seismic noise wavefield is not diffuse. The Journal of the Acoustical Society of America, 131(4), 2853. https://doi.org/10.1121/1.3689551

The sentence beginning on line 61 is also not entirely accurate. Most studies on glaciers have been unable to reconstruct GFs not because of the lack of scattering but because of the dominant frequency of the background noise. Seismic arrays on glaciers are tiny compared to regional or continental arrays. In order to recover a usable GF in the microseism band you need stations that are more than 1 wavelength apart (neglecting methods like SPAC). When we correlate signals on glaciers in the microseism band the resulting correlations look like autocorrelations because the sensors are pretty much in the same location at the wavelengths of the microseism band. It is more appropriate to state that the noise field lacks the high frequencies needed to generate GFs that contain useful information at the scale of the glacier. If you wish to use icequakes with frequencies above 0.5 Hz, then yes, your statement is accurate, but you should explicitly state this. Everything depends on the frequencies considered and you are neglecting this point in the way that you are writing these statements.

Line 75: You say on line 73 that they do obtain accurate GFs, but then on line 76 you say they don't obtain accurate GFs. Which is it?

Figure 1: fonts are way too small. I also cannot tell which color is "this event" or the "1000 event average". The colors look identical to me. I am assuming the smoother line is the average.

Line 143: Figure 1b –> Figure 1c

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Discussion paper

Line 166: What is the reason for the partial statement about the 20m resolution DEM? It does not make sense in this sentence. Please read out loud to yourself to see the mistake.

Figure 2: Fonts on axes are again very small.

Appendix A: Line 783: What does "network vs. array" mean?

Line 793: Do you really mean to reference Fig. 1c here? This is a figure of the spectrogram of a GIS signal.

Line 194-195: plane wave approximation –> stationary phase approximation. I do not understand why plane wave is used here. The proper interpretation of the sinusoidal shape is the stationary-phase. See Snieder, R., Van Wijk, K., Haney, M., & Calvert, R. (2008). Cancellation of spurious arrivals in Green's function extraction and the generalized optical theorem. Physical Review E - Statistical, Nonlinear, and Soft Matter Physics, 78(3), 1–8. https://doi.org/10.1103/PhysRevE.78.036606

Line 194: Why are you referencing Fig. B4 before Fig B1, B2, or B3? Please fix the referencing so that things are referenced in order of appearance. It makes reading easier.

Figure 3 caption: Can you please explain why you think the GF converge better in the along-flow direction based on Fig. 3b? I wonder if you are seeing anisotropy in the ice velocities, rather than some sort of convergence related to the strongest noise sources. That reasoning is somewhat counter to your argument for sign-bitting the data. It can be that the density, not the amplitude, of sources is larger in the along flow direction. That would explain differences in convergence, but what you are stated here is not quite correct. Please revise. Also, note that in line 215 you state that the sources are located homogeneously around the array, which implies the density of sources is even with azimuth. Is this true? Did you do beamforming to look at the azimuthal amplitude of incident waves on the array?

Figure 3: Text is again very hard to read.

Figure 3: The dashed blue line is not the array response. That is the frequency-dependent resolution limit. You actually correctly state this in line 238.

Line 226: Don't you mean 1b, not 1a?

Line 227: See annotated PDF. This first sentence can be stated more accurately because not all phases are dispersive. Instead, you are using the f-k domain to identify phases. We just happen to that particular transform a lot for surface wave dispersion, but as you show in your dispersion image, the P wave is not dispersive.

Line 236: Fig. 3b –> Fig. 3c (You should really pay attention to not mislabeling your figures in the future.)

Line 240: Fig. B2b –> Fig. B2c.

Figure 4: axes fonts could be larger

Figure 5: What are the units on the misfit values? Are the misfits the same in (a) and (b)?

Line 258: Fig. 3b –> 3c

Table 1 states that Vs in the granite can be as low as 1000 m/s, but there are not gray lines in Figure 5b that show you tested this velocity. Can you please explain why? I think it would be easy to change the range in Table 1 and not influence the results of the inversion. It actually appears that the lower layer velocity never goes below the upper layer velocity. Is there something in Geopsy that imposes increasing depth and prevents low velocity layers?

Line 271: I do not follow the statement that the ice thickness is 7 to 15 meters thick on the edges. Figure 5c shows ice on the edges more than 100m thick. Can you please explain this discrepancy between the text and the figure? Am I missing something here?

Appendix B: Line 915: SPAC works for single stations when you have an isotropic incident wavefield, otherwise you need an array and averaging. You even state this on line 919 with "azimuthally averaged". You should be careful with your wording in line 915. You are not telling the whole story.

Section 5.1: Do you really need an "origin of coda waves" section? This is already explained with a references in the introduction of the paper. To me this paper is unnecessarily long because everything is explained rather than simply cited.

Line 533: What is your reason to state that the energy is back-scattered? Rayleigh waves have significant forward scattering. See Snieder, R. (1986). 3D linearized scattering of surface waves and a formalism for surface wave holography. Geophysical Journal of the Royal Astronomical . . ., 581–605. Retrieved from http://onlinelibrary.wiley.com/doi/10.1111/j.1365-246X.1986.tb04372.x/abstract, in particular Figures 6 and 7 for example.

Figure 10: Why are the azimuth ranges in (a) and (b) not the same? Are you using difference sources for each station? Or is the azimuth relative to the interstation path, rather than absolute azimuth? I would think the two matrices should be missing the same azimuths if the icequakes used were the same in the two cases. (It is a very nice result by the way!!)

Line 556: Why not beamform the coda? Take the average beam over all time windows. This would highlight illumination problems.

Line 616: What is an anisotropic diffuse wavefield?

Please also note the supplement to this comment:
https://www.the-cryosphere-discuss.net/tc-2019-225/tc-2019-225-RC2-supplement.pdf

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