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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.

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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.

AS: Thank you for your comment and appreciation. We improved the discussions you were referring to in the comments below (i.e. distributed noise sources in Argentière, anisotropic wavefields from coda waves at Gornergletscher). We shortened some parts, revised our writing and used fewer technical terms to smoothen the reading. We revised the general statements on seismic interferometry with more accurate arguments. We enlarge the fonts on every figure.

Introduction:

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.

AS: We rephrased the paragraph with more accurate statements (Lines 48-55). We use the terms "approximate" rather than "reconstruct" the Green's function. We always say "under specific conditions" or "simplified approximations" that refer to the next paragraph which further details the theoretical requirement for an equipartitioned wavefield (which is often not met in reality) and is replaced in practice by the diffusive condition of the seismic wavefield (see comment below). For the assumptions in seismic interferometry, we also refer later to the study of Fichtner et al (2017) which give an accurate overview. We removed the term "direct wave".

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.

AS: We corrected this. We give a correct definition of equipartion. We rewrote the paragraph to say that equipartion is a theoretical requirement for Green's function estimate from interstation correlation, but is not met in practice in the Earth. We then work in simplified approximations of a diffuse wavefield. We modify the paragraph as (Lines 57-63): "In theory, the GF estimate is obtained in media capable of hosting an equipartitioned wavefield, that is random and uncorrelated modes of seismic propagation (P, S, Rayleigh, Love, etc) with same amount of energy. In practice, the equipartition argument has limited applicability to the Earth because non-homogeneously distributed sources, in the forms of ambient noise sources, earthquakes and/or scatterers, prevent the ambient wavefield from being equipartitioned across the entire seismic scale (Fichtner et al, 2017, and references therein).

The GF estimation from interstation correlation therefore usually relies on simplified approximations of diffusive wavefields which can be reached in (i) the presence of equally-distributed sources around the recording network (Wapenaar, 2004; Gouédard et al., 2008b) and/or (ii) in strong-scattering settings as scatterers act like secondary seismic sources and likely create a diffuse homogenized wavefield in all propagation directions (e.g. Hennino et al., 2001; Malcolm et al., 2004; Larose et al., 2008)."

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

AS: Thank you for bringing this up. We modified the paragraph and include your suggestions as (Lines 65-68): "The latter condition (ii) is sufficiently met only at high-frequency (> 0.5 Hz) in the inhomogeneous Earth's crust. Even if the noise wavefield is not generally diffuse (Mulargia, 2012), the presence of scatterers in the Earth's crust and the generation of oceanic ambient noise all around Earth make ambient noise interferometry applications generally successful."

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.

AS: That is correct. We add two sentences about these details (Lines 69-72): "In glaciers, the oceanic ambient noise field commonly used in crustal studies lacks the high frequencies needed to generate GFs that contain useful information at the scale of the glacier. To target shallower glaciers and their bed, we must work with other sources such as nearby icequakes and flowing water which excite higher-frequency (> 1 Hz) seismic modes (Sect. 2.1). In this context, the lack of seismic scattering (...)"

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?

AS: ndeed, Preiswerk and Walter (2018) were able to compute accurate Green's functions on some glacier settings which presented at that time efficient drainage systems. However, due to changes in the drainage system (localized noise sources which sometimes appeared, or initiation of lake drainage), they could not compute accurate GF at every time scale. We add this late info without going into such details: "However, due to localized noise sources in the drainage system that also change positions over time over the course of the melting season, they could not systematically obtain accurate coherent GF when computed on different time ranges, limiting the applications for glacier monitoring."

Section 2: Material and data

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.

AS: We increased the fonts and modified the color of the red line to blue in Fig 1d.

Line 143: Figure 1b –> Figure 1c

AS: We modified to Fig. 1c.

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.

AS: We rephrased the sentence which had indeed no meaning. The 20 m resolution of the DEM comes from the spatial interpolation of the GPR tracks.

Figure 2: Fonts on axes are again very small.

AS: We increased the fonts.

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

AS: We distinguish an array and a network mainly from the processing involved and the size of the array with respect to the considered seismic wavelength. We explain this in a first paragraph in appendix A3. We removed this statement here as it is explained a few lines later.

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

AS: We modified to Fig.1a.

Section 3: Glacier d'Argentière dense array

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

AS: We refer to the plane wave approximation as we restrict ourselves to far-field events. The Green function is recovered when the incident plane waves arrive with equal strength from all azimuths (and with randomly distributed and statistically independent amplitudes, i.e. with homogeneous source distributions). You need same azimuthal angle for same cross-correlation arrivals at both stations for individual sources and this is induced by the plane wave approximation (Gouedard et al, 2008). In contrast, to get accurate GF, you only need stationary phase sources. We did not modify our statement, although we revised the paragraph for better clarity.

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.

AS: Figure B4 is here to illustrate two things: (1) the source-azimuthal dependency of the reconstructed arrival times in the cross-correlation functions when computed on plane waves (as explained in the main text, Lines 200-210), and (2) the method introduced to measure Rayleigh wave phase velocity from the sinusoidal fit to the azimuthal-dependent arrival times (as described in appendix B3). Figure B4 is then an integral part of the appendices and is more cited here as a supplementary figure

for readers who are not familiar with this. Figure B4 is difficult to be renamed without completely rearranging the appendix which would not completely make sense as the appendix is ordered in the same way as the referencing to the different processing techniques that are used in the main text. We did not modify the referencing to figure B4.

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?

AS: You are right about this. We used misleading terms when speaking about "stronger sources" rather than "more sources". We modified the text and caption accordingly. We modified our statements about distributed noise sources which are, according to our analysis, not distributed homogeneously. We rewrite the paragraph (Lines 219-229) to better explain this. Now in our discussion, we separate spurious arrivals at time 0 at station pairs perpendicular to the flow line, with spurious arrivals at other pairs constituted by faster waves which maybe arise from non-aligned sources and/or anisotropy as you point out. We modified the text as: "More sources downstream are likely generated by faster water flow running into subglacial conduits toward the glacier ice fall (Gimbert et al, 2016, Nanni et al., 2019b). Indeed, looking closer at NCC for individual receiver pairs, we sometimes observe spurious arrivals around time 0 (marked as green dots in Fig. 3b), mostly at stations pairs which are oriented perpendicular to the glacier flow (i.e. azimuth $0^o \leq \phi \leq 50^o$), indicating that dominant noise sources are located along the flow line. At other station pairs (i.e. azimuth $\psi \sim 90^o$), the reconstructed arrival times are slightly faster than expected. This could be an effect of non-distributed noise sources, or anisotropy introduced by englacial features (Sect. 3.3). This analysis shows that even if the noise sources are not equally distributed in space, averaging the NCC in regular distance intervals on a dense array deployment helps the GF convergence."

Furthermore, we located the noise sources by taking advantage of the causal/acausal amplitude asymmetry and the spurious arrivals (approach similar to Stehly et al, 2009 and Retailleau et al 2017). We find that dominating sources are near the glacier ice fall (downstream) and upstream of the array as stated in the main text, but also along the glacier flow line at the center of the array. This observation also satisfies our expectation for an englacial water conduit along the flow line given the along-flow anisotropic patterns we measure at the array center (Fig.6b and modified text in section 3.3). Nanni et al (AGU Fall Meeting abstract 2019) also looked at the locations of seismic hydraulic tremors with beamforming and their observations are well correlated with our noise source locations. Noise source locations are beyond the topic of the paper as our take-home message is on the advantages of using dense arrays which allow to stack and average the GF estimates (see modified text above). We do not further discuss the noise source locations as it will be part of a future study.

Stehly et al, 2009Âă: A study of the seismic noise from its long-rangecorrelation properties, JGR.

Retailleau et al, 2017Âă: Locating microseism sources using spurious arrivals in intercontinental noise correlations, JGR

Nanni et al, 2019Âă: Mapping the Subglacial Drainage System from Dense Array Seismology: a Multi-method Approach, AGU Fall Meeting abstract.

Figure 3: Text is again very hard to read.

AS: We increased the fonts.

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.

AS: We modified the caption accordingly to the main text.

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

AS: We modified to Fig.1b

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.

AS: Thank you for reporting this. We modified our statement accordinglyÂă(Line 235): "Seismic phases and their velocities can be identified on the frequency-velocity diagram (Fig. 3c, black dots) that is obtained from frequency-wavenumber (f-k) analysis (...)".

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

AS: We modified to Fig.3c.

Line 240: Fig. B2b –> Fig. B2c.

AS: We modified to Fig B2c.

Figure 4: axes fonts could be larger AS: We increased the fonts.

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

AS: The misfit values are the same in (a) and (b) and are calculated as the normalized RMS of the residuals between dispersion curves. More precisely, the misfit can be defined in Geopsy as the RMS error normalized by the standard deviation provided by the error estimate on the dispersion curve to be fitted. When the maximum misfit value is reached (i.e. 0.1), the dispersion curve is reproduced with an approximate error of 10%. We explain this more accurately in Lines 266-270: "Misfit values correspond here to the root-mean square error on the dispersion curve residuals, normalized by the uncertainty average we obtained from the seismic data extraction (error bars in Fig. 5a). The inversion well resolves the S-wave velocity in the ice layer as all best matching models yield to Vs = 1707 m/s for misfit values below 0.05 meaning that the data dispersion curve is adjusted with an approximate error below 5%."

Line 258: Fig. 3b –> 3c

AS: We modified to Fig 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?

AS: This is right, thank you for bringing this up. Indeed, we force Geopsy to explore only increasing velocities with depth, so the lower layer velocities are always higher than the ice velocities. We then have modified the velocity range for the low layer in Table 1, and add this condition to the text, Line 264.

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?

AS: Indeed, the previous text was maybe unclear. Seismic inversions for the lines on the array edges yield to the determination of three layers: (1) a thin layer of 7-15 meters where we find decreased P-velocities with respect to the ice velocities, (2) the ice layer and (3) the bedrock. We discuss the origin of the low P-velocity zones which could be attributed to snow and more likely to the presence of transversal crevasses which introduce anisotropy that can then be modelled by a slow top layer of a few dozens of meters as shown by Lindner et al, 2008a. We also modified Figure 5c to represent this low velocity layer on the top of the ice. We now better explain this in the main text: "For the receiver lines near the array edges (Lines 1-3 and 8), the inversion yields to a low P-velocity surface layer of thickness 15 m and 7 m respectively, above thicker ice (dashed blue zone in Fig. 5c). (. . .) This low-velocity surface layer could also at least partially be attributed to the presence of pronounced transversal crevasses (i.e. perpendicular to the receive lines) near the array edges, which do not extend deeper than a few dozens of meters (Van der Veen, 1998) and can be modelled as a slow layer above faster underlying ice (Lindner et al., 2018a)."

Lindner, F., Laske, G., Walter, F., and Doran, A. K.: Crevasse-induced Rayleigh-wave azimuthal anisotropy on Glacier de la Plaine Morte, Switzerland, Annals of Glaciology,

pp. 1–16, 2018a.

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.

AS: Thank you for reporting this. We added the condition of the isotropic wavefieldÂăin Line 915: "this technique does not require specific array geometries to compute phase velocities and can be used on single pairs of stations as long as you are in the presence of an isotropic incident wavefield."

Section 4: CWI at Gornergletscher

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.

AS: We shortened this section and moved some general descriptions with appropriate referencing on the diffusive character of coda waves to the introduction of section 5. We renamed section 5.1 as "Icequake coda waves at Gornergletscher". This section now focuses on the description of icequake coda seismogram.

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, in particular Figures 6 and 7 for example.

AS: We did not want to imply anything on the back or forward scattering of the Rayleigh wave propagation mode. We were referring to single versus multiply scattering and

removed the term "back-scattered" accordingly.

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!!)

AS: We are essentially using the same sources for the two station paths, except that we exclude events that lie in the vicinity of the stations, i.e. at distances shorter than approximately half of the interstation distance given the considered seismic wavelength (as described in the general section 3). As the first station pair in Fig 10a is closer than the one in Fig 10b, we excluded more events for the computation in Fig 10a. Furthermore, the azimuth is here defined as the event azimuth relative to the station path (and not absolute azimuth). As the two station paths are oriented differently with respect to the source distribution, it results in a different azimuth range. The definition of the azimuth was well stated in the main text. We define it correctly in the figure caption.

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

AS: We did not try beamforming for the following reasons: (i) The short coda duration and the fast drop of coda coherency across the array prevents for time-lapse beamforming. This is also why CWI fails at some station paths as in here. (ii) Low beamformer resolution given the seismic array and coda coherency. (iii) Given sensitivity kernels of seismic coda, seismic coda are strongly sensitive right beneath the stations, and we do not expect to see any convergence to the illuminated zones given the two points above.

Line 616: What is an anisotropic diffuse wavefield?

AS: With "anisotropic diffuse wavefield" we were referring to the anisotropy of the energy flux which is still observed in the late coda as it is still contains information on the source incidence. To overcome this effect in weak scattering medium, numerical simulations of Paul et al (2005) suggest to work with equally-distributed sources. We modified "anisotropic diffused wavefield" to "the long-lasting anisotropy of the diffuse energy flux". We add these explanationsÂă(Lines 608-610): "Indeed, in weak (or homogeneous) media, the incident energy flux from earthquakes can still dominate the late coda resulting in GF time-asymmetry, provided the sources are located in the same distant region. The CWCC asymmetry is expected to disappear with an isotropic distribution of sources or scatterers around the seismic network.". In general we rearranged and rephrased the whole discussion for more accurate and clearer statements.

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

AS: Thank you for such a careful reading. We took into account all of your suggestions.

Please also note the supplement to this comment:
https://www.the-cryosphere-discuss.net/tc-2019-225/tc-2019-225-AC2-supplement.pdf

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