Response to RC1

Dear Andreas Köhler,

Thank you for your very positive remarks and insightful comments on the paper. We appreciate the time and effort that you have dedicated to our manuscript. We have discussed your suggestions and summarized the outcome below. The small technical corrections will be also incorporated in the revised manuscript, which will be uploaded in a later stage.

- Line 47: “significant contribution of Rayleigh waves” This is most likely true for glaciers with mostly natural sources I guess but note that Love waves have been shown to have a significant contribution in urban areas with anthropogenic sources. Also, body waves may affect HVSRs at lower frequencies. I suggest to briefly mention noise wavefield composition and its effect on HVSR here.

  Good point about the wavefield, I modified the sentence and added the effect of Love and body waves: “The ambient vibration diffuse wavefield has a significant contribution of Rayleigh waves, which are dispersive seismic surface waves with elliptical particle motion that depends on the subsurface structure (Fäh et al ., 2001). Small contributions of Love-and body waves may include the wavefield and contribute to the peaks in the HVSR curve by amplifying horizontal ground motion (Bonnefoy-Claudet et al., 2006)”.

- Line 93-94: Just for clarification: Do you have an idea what controls the hydraulic tremors indicated in Fig 2c, day 237-240 before the drainage?

  Thank you for this sharp point, indeed we should mention the tremor of these days. They are most likely some pre-drainage precursor hydraulic tremors. The pressure is building up in the ice, and fractures and conduits formed to enable the drainage later on (See Lindner et al., 2020 Figure 6). I added a line on this in the manuscript.

- Line 125: It is of course very appropriate to use this well-known formula for a depth estimate. But if I'm not mistaken, this originates from theoretical considerations if SH wave resonance is responsible for the H/V peak. There are studies which showed that the peak frequencies from Rayleigh wave elliptticity peak and SH wave resonance are very close, so this is no issue as such. I mention this because of the wavefield composition mentioned above (Rayleigh waves).

  We agree that the frequency of the H/V peak and the resonance frequency of the site (SH wave resonance) are very close but not exactly equal. Hence, equation (1) is only an approximate relationship. To clarify, we added an explanation to the revised manuscript and a reference to a study with a comparison of these two frequencies (Bonilla et al., 1997).

- Figure 5: It seems the through starts appearing already around day 228. Not very clear though, but could this be due to melt water presence?

  Yes, that could be an explanation. We have observed this earlier appearance of the trough too. Based on the modelled melt discharge or precipitation measurements, there is not a clear relationship. Due to the progress of the melt season, there might be accumulation of melt water causing the appearance of the trough. Though we do not have clear evidence for this.

- 236 ff: The second inversion seems to result indeed in a slightly better fit, but I'm not sure if I would call this a clear improvement visually. Can you quantify the improved misfit?

  Thank you for this helpful comment, the quantification of the misfit strengthened our statements regarding this issue. In the used inversion method, the misfit is quantified by the variance reduction (VR) between synthetic and observed data weighted by reciprocal errors (see error bars in Figure 8). The VR value of 100 % means a perfect fit, the VR value of 0 % means fit on the edge of the data error bars, and the negative values of the VR mean synthetic predictions out of the range of observed data errors. The inversion without the low-velocity zone provides the maximum likelihood and maximum a posteriori models with fits of VR = 41% and VR = 27%, while the inversion with the allowed low-velocity zone provides fits of VR = 75% and VR = 69%. We added these values in the manuscript because it is an objective and clear measure of the improvement in the fit to observed data.

- Line 261ff: I agree that inversion and modelling results support the low-velocity hypothesis. However, the fits in Fig 9 are not very good. Can you speculate a bit what could be the reason why a more complex model is required in your glaciological setting? Additional layers? 2D/3D effects?

  The geopsy software for the modelling of the dispersion curves is not able to exhibit the plateau we observe in the empirical curves. That creates the biggest misfit. A more complex model not with just two layers over a half-space might improve the models, but we do not have a velocity model of the glacier for this period of time of drainage. We expected to have a uniform layer of ice over the bedrock. The presence of pressurized sediments would also act as a low-velocity layer at the base. In our study, we refrained from modeling the presence of a thin sediment layer due to insufficient information on its thickness and properties. The inferred thickness of the low-velocity layer (section 5) exceeds possible sedimentary layer dimensions.

  The glacier has a more or less sheet-like structure and not a deep incised valley. That minimizes the 2D/3D effect. However, formed conduits and the base and the low-velocity layer might act as waveguides. While the modelling is a 1D approach.

  Hence, the forward modeling test is a simplistic approximation, because of the lack of observed data to fully model the complex wavefield influenced by a hypothetical water layer. We added this statement to the revised manuscript.

- Line 296-298: Could then pressurised sediments be an explanation for the misfit? See my comment above.

  In our opinion, the misfit is due to that we are on the low-frequency resolution limit. But I cannot completely rule out pressurized sediments and we add this statement to the manuscript. In the introduction we added this sentence: Here, we define the subglacial environment as the area near the ice-bed interface, which can encompass fractures and voids in the glacier sole as well as the top layer of the bedrock.

This includes the ice-bedrock interface including the bottom part of the ice and the top part of the bedrock, where also maybe some sediments are present. The resolution of the inversion is not sufficient to distinguish in more detail.

Also, Glacier de la Plaine Morte is on a karstic bedrock. Meltwater from the glacier is also disappearing partly via this system and not only via the main outlet in the north We cannot completely exclude that some lake discharge water also enters the karstic system. But based on observation from Lindner et al., 2020 and the development of subglacial conduits due to pressure build up and strong hydraulic tremors, we assume that the majority of the water flows through and below the glacier to the main outlet. This is an important point and will add this to the discussion section.

We hope we cover your comments and are willing to respond to any further questions and suggestions you may have.

Sincerely,

Janneke van Ginkel, Fabian Walter, Fabian Lindner, Miroslav Hallo, Matthias Huss and Donat Fäh