Reply to reviews on the manuscript
Modal sensitivity of rock glaciers to elastic changes from spectral seismic noise monitoring and modeling

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Dear reviewer, dear editor,

We would like to thank you for the constructive review following the submission of our manuscript "***Modal sensitivity of rock glaciers to elastic changes from spectral seismic noise monitoring and modeling***" to *The Cryosphere*. We took into account all the comments from the two reviewers and editor.

One of the main problems raised concerns the lack of references to the HSVR method. Indeed, we didn't specified clearly that our seismometers are single (vertical) component, so that the HSVR method was inapplicable in our case. However, we added some mentions to previous publications on this method successfully applied on permafrost, since the methodology is very similar to spectral analysis presented in this article.

Furthermore, you suggested additional figures to demonstrate the feasibility of our method: one showing the temporal evolution of modeled resonance frequencies, and another one showing modeled amplitude of the spectrum for a particular state of freezing. Unfortunately, both of them are difficult to address accurately to our mind, because of the lack of information concerning thermo-mechanical coupling and 3D effects on the rock glaciers. We detail these points further in our response.

As suggested by the reviewers, we also modified some sentences and figures in order to improve both the quality of information that we provide and the readability of the publication. We added also some references to publications about seismic monitoring on permafrost and glaciers, as a completed state of art.

Please find below a point-by-point response to all your major comments (our answers in red), in complement to the new manuscript with highlighted main changes (text in red as well).

Sincerely yours,

On behalf of the authors,

Antoine Guillemot

Comments reviewer 1 (Andreas Kohler)

This is a very good and comprehensive study demonstrating the ability of passive seismic measurements for time-lapse monitoring of near-surface structures in combination with other geophysical methods which provide more high-resolution, but time-restricted information. The presented use of modal analysis in particular is a novel and promising approach for rock glacier monitoring. The results presented in this study support the capability if the method.

(1) I would be interested in a brief discussion on how the HVSR (Horizontal-to-Vertical Spectral Ratio) method is related to the modal analysis of single component seismic data. In case of HVSR, the argument often used is that normalizing the horizontal spectrum by the vertical will reduce the source effects and thus enhance the resonance spectrum of the site. HVSR peaks are interpreted either as SH wave resonance (see equation on Page 6, line 190) or maxima of the Rayleigh wave ellipticity in a layered medium. Would it make sense to present the results of this study as HVSR time-lapse spectra instead of single component spectra (assuming the three-component seismometer have been used)? I would expect to see the same temporal variability by potentially reducing at the same time some spectral peaks related to local sources. Is the 3D nature of the rock glacier the reason for not using spectral ratios?

➔ We understand well this comment, because the HVSR method is now commonly used. However, we used only one-component seismometers that measure vertical movement (we then measure only Rayleigh waves, but they are supposed to dominate surface waves). We precise in the new version: "*The seismometers (Mark Products L4C, one vertical component)*". Indeed, we didn't specified clarly that our seismometers were single (vertical) component, so that the HSVR method was inapplicable in our case. However, we added some mentions to previous publications on this method performed on permafrost, since the methodology is very similar to ours.

(2) The ability of the HVSR method for time-lapse permafrost monitoring has been recently investigated in a few studies:

Abbott, R., Knox, H. A., James, S., Lee, R., and Cole, C.: Permafrost Active Layer Seismic Interferometry Experiment (PALSIE), Tech. rep., Sandia National Laboratories (SNL-NM), Albuquerque, NM (United States), available at: https://prod.sandia.gov/techlib/access-control.cgi/2016/160167.pdf (last access: 7 January 2019), 2016.

Kula, D., Olszewska, D., Dobiński, W., and Glazer, M.: Horizontal-to-vertical spectral ratio variability in the presence of permafrost, Geophys. J. Int., 214, 219–231, https://doi.org/10.1093/gji/ggy118, 2018.

Köhler, A. and Weidle, C.: Potentials and pitfalls of permafrost active layer monitoring using the HVSR method: a case study in Svalbard, Earth Surf. Dynam., 7, 1–16, https://doi.org/10.5194/esurf-7-1-2019 , 2019.

It might be useful to have a look at these studies. Please feel free to not cite them if they are not relevant for the current work (especially since a paper of mine is included).

➔ We were very grateful for sharing these other references, and added some on polar permafrost into our new version. Indeed the HVSR method is broadly used with strong similarities to our spectral analysis, making the parallel relevant. We added this sentence : "*Furthermore, time-lapse monitoring using Horizontal-to-Vertical Spectral Ratio (HSVR) method has already been applied to polar permafrost areas, showing a detectable influence of seasonal variability in the active layer on spectral content of recordings (Köhler and Weidle, 2019; Kula et al., 2018).*"

(3) I miss a figure directly comparing the temporal evolution of the measured resonance frequencies with the modeled once for different states of thawing. Furthermore, a figure comparing the measured and modeled amplitudes spectrum for a particular time would be very useful (for example overlaying figure 2d). In my opinion such figures are more important for demonstrating the reliability of the method than showing the GPR and active seismic results in Fig 4,5 and 6. Those can be moved to the Appendix.

➔ We agree with this comment. This temporal modelling may clearly strengthen the interpretation of our results and improve our publication, but actually we cannot address correctly the transition between seasons. In fact we focus more on the general difference between summer (assuming a complete thawing of the active layer marked by lower values of resonance frequency), and winter (complete freezing of the medium between the surface and the ZAA depth, marked by higher values of resonance frequency). We show the comparison between these extreme values on Figure 13 (winter and summer), but no modelling of the temporal evolution between seasons is proposed. Indeed, a complex thermo-mechanical coupling is required in order to simulate the propagation of the heat wave, but diffusivity properties are poorly known (lack of thermal data from boreholes). In addition, the advection of heat from water adds complexity, and too many assumptions are then required in order to correctly model the time series of resonance frequency using a poroelastic model. Therefore we wouldn't propose this modelling in this publication, even though it should be suggested for future works. The goal of Figure 14 is simply to interpret the relation between ground surface temperature and measured frequencies over the whole year, without any modelling.
The modeled amplitudes spectrum for a particular time would be difficult to compare with the measured one, since 2D and 3D effects on frequency spectrum may appear (see Preiswerk et al. (2019) *(doi: 10.1017/aog.2018.27*). These effects may be very difficult to assess and to predict for rock glaciers (not studied for now). In this article, we focus on the resonance frequency of the fundamental vibrating mode of the rock glacier (at around 15-20 Hz), rather than on the whole amplitude spectrum. But we specified in the first manuscript the complete methodology of the mechanical modelling performed thanks to Comsol software (shown in Figure 12): the computed Frequency Response Function (FRF) is able to model the fundamental mode of the whole structure, but local 2D and 3D effects may affect this curve, making difficult the comparison between modeled and measured curves.

➔ Concerning the GPR and active seismic figures, they should be moved to the appendix session if the editor will consider that there is too many figures in the main part. Indeed this article focus on passive seismic methods and modal analysis, rather than general geophysics results on rock glaciers.

Minor comments:

General: Please check if all commas are needed.

The first sentence of the abstract and the Introduction are identical. Please consider rephrasing.

- ➔ We changed the first sentence of the introduction, as follows: "*Among periglacial landforms, rock glaciers are tongue-shaped permafrost bodies*."

Line 42: highly challenging?

- ➔ Yes, replaced.

Line 49: Do you mean "coast-intensive"? Or "…remain cost-effective only when limited to one single …"?

- ➔ We specified "*cost-intensive and limited to one single point*", as we mean.

Line 61: active permafrost layer

- ➔ By "surface layers", we want to refer to the active layer, but also the deeper one (until around 10 m depth), which is called permafrost layer since it is composed of permanently frozen materials.

Line 73: Please explain "as a bending beam"

- ➔ We specified : "*as buildings*", in order to suggest a vertical structure fixed at the bottom.

Line 76: "Our goal … of the rock glacier and the time variability of their resonance frequencies which gives hints …"

- ➔ Ok.

Line 78-79: "… are numerically modeled …"

- ➔ Ok.

Line 81: "In the second part, …"

- ➔ Ok.

Line 97: Something is wrong with formatting of exponents

- ➔ Ok.

Line 123: one bracket too much

- ➔ Ok.

Line 130: Are these three-component sensors? Was maintenance required during the measurements (releveling etc.)?

- ➔ We specify in the new version: "*The seismometers (Mark Products L4C, one vertical component)*". For the maintenance required, we already mentioned that the sensors are subject to tilting, forcing to frequent site visits in order to releveling them.

Line 140: I think the time-lapse resonance spectra of the other sensors should be included as supporting materials or in the appendix (do not need to be discussed)

- ➔ We agree. However, since the numbers of figures and appendixes are already high, we share the figures of spectrograms from other sensors (Laurichard sensors C01, C03 and

C04) only in personal communication for the moment (see the figures joined). For us it is not necessary to add them into the article, since these data are not interpretable due to tilting and technical instrumentation issues (specified in the text of the manuscript). Please let us know if you want to include them in an additional appendix.

Line 167: "Continuous seismic monitoring systems are composed …" (?)

- ➔ We change the verb: "*Continuous seismic monitoring requires autonomous operating systems composed of...*".

Line 171: "… between several sensors and to monitor …"

- ➔ Ok.

Line 195: I suppose the vertical component is used?

- ➔ Yes, we used seismometers with only the vertical component. We precise it: "*We pre-processed hourly raw seismic traces from vertical component of seismometers*".

Line 200: This is unclear. Please rephrase to describe how peaks are picked automatically.

- ➔ Ok, we specified exact values of threshold and tried to explain better the picking : "*We selected automatically significant and sharp peaks of the spectrum by using different threshold values for local maxima picking (minimum of peak frequency at 10Hz, minimum of inter-peak distance at 4 Hz, maximum of width of 8 Hz, minimum of peak height at 0.2 and minimum of prominence at 0.3 for normalized spectra).*"

Line 261: Just to avoid misunderstanding: Is the modelling of resonance frequencies done in full 3D, 2D or for a particular location and 1D model below?

- ➔ In this article we handle the modelling of rock glacier in 1D for particular location underneath seismic sensors. The 2D and 3D effects are quite difficult to address (see Preiswerk, L.E., Michel, C., Walter, F., Fäh, D., 2019. Effects of geometry on the seismic wavefield of Alpine glaciers. Annals of Glaciology 60, 112–124. https://doi.org/10.1017/aog.2018.27). But we checked that resonance frequency of fundamental mode of the 2D model is similar to the one of our simplified 1D models. We detailed this methodology further (part 4.4).

Line 267: "cost-effective" See above.

- ➔ We replaced by "*cost-intensive*".

Lines 376, 409, 449, and 452: paragraph/section numbers wrong

- ➔ Ok.

Line 476: "inter-annual climate variability" Is "climate" the right word here? I guess the constant climate at a particular site includes the inter-annual variability of temperatures.

- ➔ Yes, we agree with this statement. We then modified the first sentences of this paragraph in order to precise what we handle : "*By tracking resonance frequencies in long term, we are able to detect an inter-annual variability. Interestingly, the freezing process appears to correlate with annual minimum of resonance frequency: as an example,* (...)"

Line 491: "observed gap" Do you mean "observed difference"?

- ➔ Yes, replaced.

Line 496: “in combination with”?

- ➔ Ok.

Line 498: Remove “Furthermore”

- ➔ Ok.

Line 515: Sentence “Frequency resonance focuses on …”: I am not sure if this is correctly formulated. The resonance frequency in general is also an effect of seismic waves propagating through the whole structure. It just depends on the considered frequency band. Here, I agree with your conclusion that resonance works well at high frequencies (and thus for shallower depths) where most of the changes occur, while noise correlation does not work so well due to lack of correlation at high frequencies if the inter-station distance is too large. So, the different sensitivities are mainly because of the nature of the ambient noise wavefield and the sensor network set-up, not because of the depth sensitivity of both methods as such. See for example the study of James et al, where noise correlation could be used to measure very shallow variability with closely located sensors.

- ➔ We specified these statements into the paragraph : "*Furthermore, ambient noise correlation may provide less stable results at high frequencies (up to 14 Hz, for the Gugla study (Guillemot et al., 2020)), preventing any interpretation of the chaotic results due to the lack of high frequency noise in the cross-correlation large inter-sensor distance. Hence, the sensitivity of the different methods depends also on the nature of the ambient noise wavefield together with the sensor network setup. According to the site and its instrumentation, the two passive seismic methods may be combined to obtain stable results along the whole depth of the rock glacier.* "

Line 542: geophysical measurements

- ➔ Ok.

Appendix A: I am not sure if it necessary to include the results of earthquakes since the results are not much discussed. If they are included, one would like to know where the discrepancy compared to noise comes from.

- ➔ We decided to keep this figure in Appendix B, because these results are also used as an argument for another discussion. Indeed, we added a new paragraph (in part. 3.2) to handle the question raised about the noise source variability, and its influence on frequency temporal variability.

Figure 2: Showing the noise waveform record is not necessary in my opinion. Instead, please also add a plot like (d) for Gugla.

- ➔ On our opinion, the Figure 2 eases to show the methodology of frequency picking from a spectrogram of an earthquake. This shows only an example of waveform from an earthquake occurred near the Laurichard rock glacier. The principle for Gugla is strictly the same, therefore a plot for Gugla may overload the figure. In addition, the substantial number of figures and appendixes in this article may be limited, but let us know if you still require this added plot.