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 influence of ambient noise sources on resonance frequencies that we picked. Indeed, temporal variability of these frequencies could be related with source variability. To address and discuss this relevant question, we kept the figure showing the spectrograms with frequency picking from earthquakes, in order to show that resonance frequency that we picked are still often visible even for variable earthquake sources. Besides, we added a new figure of spectrograms from a station near the Laurichard rock glacier, settled on a stable site that could be considered as a reference station.

Furthermore, you suggested additional figures to demonstrate the feasability of our method, showing the temporal evolution of modeled resonance frequencies? Unfortunately, this is out of the scope of this publication to our mind, because of the lack of information concerning thermo-mechanical coupling to accurately address this question. We detailed this point further in our response. Nevertheless, we added a new figure in appendix showing the ground surface temperature data, as you requested.

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 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 2

The study presents a new methodology that uses the resonant frequency of rock glaciers extracted from continuous ambient noise records to monitor the seasonal and interannual changes in the structure of the vibrating glacier. The seismic modal analysis is coupled to a poroelastic model of a 2D porous medium representing the rock glacier, supported by other geophysical measurements. The results indicate the thawing-freezing cycle effects on the resonance of the glacier. This comprehensive study highlights well how the structural changes of rock glaciers can be tracked and monitored with passive seismology, when other geophysical methods are time-consuming and hardly repeatable at such time resolution.

Major comments:

(1) I miss somewhere an explanation of the origin of the ambient seismic noise (at frequency > 1Hz). Many studies agree to say that it takes its origin in fluvial processes when the water flow from ice melting in spring/summer creates transient forces on the Earth at the glacier base or on the surrounding ice in englacial channels. The noise recorded in winter may come from other sources in the area. Many studies on ambient noise suggest that a change in the noise sources could lead to a change in the noise spectrum. This renders monitoring studies difficult, especially for ambient noise correlation method. Here you use a modal analysis which should be less sensitive to noise source changes. But still, there remains an open question: are the seasonal variations of the resonant frequencies influenced by changes in the noise or actual structural changes ? I am quite concerned about the abrupt resonant frequency shift you observe at the onset of the melting. Personally, I am absolutely confident in the interpretation of actual changes in elastic properties of the structure underneath the sensor. However, this question should be raised and discussed.

- ➔ Thanks for this comment, we addressed and discussed this relevant question in the new version. We already studied the noise source variability, because actually melting processes occurring in spring/summer time can affect the nature of ambient noise, and could lead to spurious comparison of spectral content.
- ➔ During all the year, ambient noise is assumed to be mainly produced by weathering (rain, wind) and anthropogenic sources (traffic on the road in Lautaret Pass for Laurichard, and traffic and human activities in Mattertal valley just below the Gugla rock glacier). In melting and summer periods, noise level is generally higher than during winter time, due to the addition of fluvial and groundwater processes in vicinity of our sites.
- ➔ We addressed this question by adding a new paragraph, and a figure (Fig. 19) in a new Appendix (C). We compared raw and normalized spectrograms of seismic recordings on rock glaciers and on a stable site located in the vicinity. We then presented the results for the OGSA station, located at Lautaret pass (2 km from Laurichard rock glacier) in 2019 (see Fig. 19). From these raw spectrograms we observed a seasonal variability of noise level, actually suggesting a seasonal variability of the noise sources (higher in summer than in winter, probably due to intensified road traffic and fluvial processes in summer). From the normalized spectrograms, we noticed that no significant changes of frequency peaks appear within the illuminated spectrum of interest (10-40 Hz). Since the OGSA station (stable reference) is located close to the Laurichard rock glacier, we assume that ambient noise sources are the same for both sites. Therefore we conclude

that seasonal variability of frequency peaks recorded on the rock glacier between 15 and 40 Hz (see Figure 3) is not much influenced by source changes, but rather linked to specific resonance of the rock glacier structure.

- ➔ In addition, we chose to keep Figure 18 in Appendix B, showing spectrograms and frequency picking from several earthquakes signals (hence several sources), in order to show that resonance frequency that we picked are still often visible, even for variable earthquake sources.
- ➔ We detailed all these observations in the new version: "*The spectral content of seismic recordings can be affected by temporal variations of ambient seismic noise sources. For the two sites, these sources are assumed to originate from stable human activities located in the nearby valley and from weathering, but they could also be partly related to hydrological processes via melting water in spring and summer time. This source variability has to be addressed, in order to eliminate any spurious interpretation of actual changes in elastic properties. We then compare raw and normalized spectrograms of the reference station OGSA over one year (see Fig. 19 in Appendix C) to track any variation of the spectrum content which would prevent further comparison of frequency peaks observed on the rock glacier over time. No significant temporal changes of PSD appears within the illuminated spectrum of this stable station located near Laurichard rock glacier. Another obvious fact to highlight is that frequencies which were picked from ambient noise are also often visible when earthquakes signals are considered (see Appendix B). These two observations strengthen the direct link between these frequency peaks and rock glacier resonance".*
- ➔ In this way we hope that this question of ambient noise sources variability is now correctly addressed and discussed.

(2) As a proof of concept and to approve the interpretation of the results, I suggest to try to actually reproduce the seasonal variations of the resonant frequencies with the poroelastic model (as stated in the conclusion Line 542 but wrong). This would strengthen the discussion and the capability of the method. The interpretation is finally based on Fig 14 which represents measured resonant frequencies as a function of the temperature of the ground surface, while this temperature does not tell the whole story on what is happening at depth. So having a hint on the evolution of the active layer and the rigidity of the structure thanks to the best-fitting model output would definitely strengthen your conclusions. In addition, this figure would be very great to track the interannual changes. Finally, I miss a figure showing the temperature time-series (in this additional « proof of concept » figure, or in Fig 3 for example). This would ease the reading of Fig 14 and also highlight melting seasons versus winter.

- ➔ Yes, 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 needed in order to simulate the propagation of the heat wave, but diffusivity properties of rock glaciers 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 model the time series of resonance frequency by the 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. Nevertheless, we decided to add a new appendix with a figure showing the temperature time-series in Laurichard, as you requested.

(3) Sometimes, I think that the structure of the sentences is not smooth enough and lead to confusing statements in English. I have listed some in the comments below which need to be reformulate. In general you should try to keep the sentences short.

- ➔ Yes, we tried to reformulate and shorten the sentences as possible, together with the corrections of some statements you raised below.

Other comments:

L48: in depth -> at depth ?

- ➔ Ok.

L57-59: References are needed here for both methods (ambient noise vs microseismicity). I understand this is the output of the study by Guillemot et al 2020 as detailed in the following sentence, but other studies have also proven this in glaciated environment.

- ➔ Yes, we understand the requirement. We completed the references in the new version as follow: "*Passive seismic monitoring systems have the potential to overcome these difficulties on debris slope (Samuel Weber et al., 2018; S. Weber et al., 2018), glaciated (Mordret et al., 2016; Preiswerk and Walter, 2018) and permafrost environments (James et al., 2019; Köhler and Weidle, 2019; Kula et al., 2018), also recently illustrated on the Gugla rock glacier (Guillemot et al., 2020)*."

L89: I suggest to specify « France » after the « Laurichard catchment » Figure 1d: Please indicate the flow direction with an arrow.

- ➔ Ok.

Section 2.1.3: How were the seismometers maintained (leveling, orientation) ? Are they threecomponent sensors ?

- ➔ We only used one-component seismometers that measure vertical movement (Rayleigh waves are supposed to dominate surface waves). We precise in the new version: "*The seismometers (Mark Products L4C, one vertical component)*".

You should also specify somewhere here the glacier thickness beneath the stations (ice thickness was also not specified in the previous section)

- ➔ We guess you addressed the bedrock depth rather than the ice thickness, since rock glaciers are rocky debris bodies. For each sensor and sites (Gugla and Laurichard) we estimated the bedrock depth from different methods (borehole data for Gugla, geophysics for Laurichard) much detailed in the following parts.

L137: « The long periods » -> the longest periods ?

- ➔ Ok.

L139: What is the distance between these two stations ?

- ➔ We precise in the text : "*Therefore we decided here to present results from only these two locations, separated by around 80 m*".

L140: It could be interesting to have the results for the seismic measurements at the other stations in the appendix.

- ➔ Yes, we agree. But 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 another appendix.

L144: I suggest to specify « Switzerland » after « Wallis Alps »

- ➔ Ok.

L157: Please specify here if the geophones are one or 3 components. Are they deployed directly on the ice ? Were they maintained ?

- ➔ We only used one-component seismometers that measure vertical movement (Rayleigh waves are supposed to dominate surface waves). We precise in the new version: "*The seismometers (Mark Products L4C, one vertical component)"*. These sensors are settled on top of relatively large, stable and flat boulders, and sheltered by a plastic tube to shield off any influence of rain, wind and snow. They are connected together with wires insulated by sheath to protect for weather and rockfalls. All these details about instrumentation are in the manuscript.

L169-171: This is the right place to be more specific for glacier microseismicity and glaciological applications (location of crevasses, basal interface and asperities, water channels) with appropriate referencing. For ambient noise studies on glaciers, you could refer to the studies of Preiswerk and Walter 2018, Sergeant et al 2020 (for the imaging part) and Lindner et al 2018 (for the monitoring part)

Preiswerk, L. E., & Walter, F. (2018). High-Frequency (> 2 Hz) Ambient Seismic Noise on High- Melt Glaciers: Green's Function Estimation and Source Characterization. *Journal of Geophysical Research: Earth Surface*, *123*(8), 1667-1681.

Sergeant, Amandine, et al. "On the Green's function emergence from interferometry of seismic wave fields generated in high-melt glaciers: implications for passive imaging and monitoring." *The Cryosphere* 14.3 (2020): 1139-1171.

Lindner, F., Weemstra, C., Walter, F., & Hadziioannou, C. (2018). Towards monitoring the englacial fracture state using virtual-reflector seismology. *Geophysical Journal International*, *214*(2), 825-844.

- ➔ Thanks a lot for sharing these references. We added several of them in order to complete the former state of art: "*Passive seismic monitoring systems have the potential to overcome these difficulties on debris slope (Samuel Weber et al., 2018; S. Weber et al., 2018), glaciated (Mordret et al., 2016; Preiswerk and Walter, 2018) and permafrost environments (James et al., 2019; Köhler and Weidle, 2019; Kula et al., 2018)*" But since rock glaciers behave rather like rocky landslides than glaciers, we wouldn't favor references on glaciological applications rather than others. We focus more on references about spectral analysis of ambient noise recordings on glacial and periglacial environments.

L171: add « to » before monitor.

- ➔ Ok.

L174: « on it » -> « on site »

- ➔ Ok.

L176: « tracking dynamic parameters » -> « tracking the evolution of »

- ➔ Ok.

L180: « The seismic noise averaging properties » What does this refer to ?

- ➔ We modify the sentence to precise what property is in question: "*Through stacking source and trajectories over time and space, seismic noise allows considering the illuminated frequency spectrum as large and stable enough to overcome these respective effects, particularly when monitoring is considered.* "

L182: « The PSD is simply defined by averaging the intensity of the FFT » You average it by what ? The (inverse of) time of integration ? I do not see any averaging in the proposed equation.

- ➔ We wanted to mention that we stacked the seismic frequency content over time (hourly or daily) before computing the PSD ; that's why we considered a time-averaging. But we already precised that we process hourly raw data in the previous version, so that we remove this mention here : "*The power spectrum density (PSD) is simply defined by computing the intensity of the fast Fourier transform (FFT) of the seismic record*".

L185. Preiswerk et al 2019 also investigated the resonant glaciers with different geometries which imply 1D, 2D and 3D resonances through HVSR, time-frequency dependent polarization and modal analysis. This study should be cited, here maybe or elsewhere.

Preiswerk, L. E., Michel, C., Walter, F., & Fäh, D. (2019). Effects of geometry on the seismic wavefield of Alpine glaciers. *Annals of Glaciology*, *60*(79), 112-124.

- ➔ Thanks for sharing these relevant reference. We actually mentioned it twice in the new version (in methods of spectral analysis of seismic data (part. 3.1) "*The estimation of bed geometry properties from spectral analysis of seismic noise has already been studied on alpine glaciers (Preiswerk et al., 2019)*", and modal analysis and frequency response of the rock glacier (part 4.4) "*Similar to the fundamental mode of an unstable rock mass (Burjánek et al., 2012) and avalanche glaciers (Preiswerk et al., 2019), the measured polarization is almost linear*".

L191: « in absence of geometrical changes » maybe specify here « (i.e. ablation/accumulation) »

- ➔ Yes, we agree. In a case of rock glacier, the potential geometrical changes may be related rather to an intensive creeping movement (or a destabilization), than to accumulation or ablation (like glaciers). Then we precise this with accurate words for rock glaciers: "*Extending this approach to a rock glacier shows that in absence of* *geometrical changes (no significant destabilization), resonance frequency variations can be related to evolution of its rigidity, through Young's modulus and density*".

L191: missing comma after Young's modulus.

- ➔ Ok.

L195: Which component ?

- ➔ We precise in the new version: "we pre-processed hourly raw seismic traces from vertical-component seismometers".

L200-204: The picking method is not clearly described. Additionally, please specify why you expect to have frequency peaks above 10 Hz on the glacier ? Is it related to the glacier morphology and more specifically to the ice thickness ?

- ➔ We rewrite this part to clarify some points. We precise the values of threshold : "*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)*."
- ➔ We don't expect to see any resonance frequency of the rock glacier below 10 Hz, after viewing the spectrograms of all sensors on the two sites. Furthermore, for a simplified 1D model with soft rock glacier on hard bedrock, the frequency of the fundamental mode would be f0=Vs/4h (mentioned in the article). With h= 10m and Vs=600m/s (orders of magnitude), peaks below 10 Hz would be unlikely for specific resonance of the whole rock glacier body.

Figure 2d: It seems that you still see the 23 Hz anthropogenic peak at station C05 (as expected) but it is not obvious because of the normalization. Maybe indicate this in the caption.

- ➔ Yes, we indicated this in the caption.

L212: « The results of PSD » -> « The spectrograms of PSDs »

- ➔ Ok.

L218: « no subglacial resonating water-filled cavities was known on the site ». I suspect you refer to moulins which extend from the glacier surface to (probably) the glacier base (this is what it sounds as you cite Röösli et al 2016) ? If yes, the part of the sentence Line 2018 should me modified to point out to moulins. If no, how do you know that there is absolutely no channels in the glacier ?

- ➔ Yes, we agree with that. We then modified the sentences to clarify the two hypotheses (hydrological because groundwater can exist, even if depth and location are unknown, and anthropological) : "*Since this frequency peak is fully stable over time, we interpret its origin as either hydrological or anthropogenic. It may be generated either by groundwater flow within the rock glacier (Roeoesli et al., 2016), or by a pressure pipe located 400 m downstream or from road traffic coupled with a tunnel near the Lautaret pass (see Fig. 1). This frequency peak is also visible on spectrograms of station OGSA (see black arrow in Fig. 2d) located at Col du Lautaret on a stable site, suggesting a potential anthropogenic source. The spectral content of these recordings exhibits the same peak at 23-24 Hz (see red curve in Fig. 2d), implying it is not directly related to* *the rock glacier resonance. Then this frequency peak is hereafter excluded from the analysis".*

L225: « resonating structure of the sensor » -> « beneath the sensor » ? Do you refer to the glacier or the shelter around the sensor ?

- ➔ We referred to the shelter around and above the sensor (precise in the new version).

Section 3.2: see major comment: I miss somewhere an explanation of the origin of the ambient seismic noise (at frequency > 1Hz). Many studies agree to say that it takes its origin in fluvial processes when the water flow from ice melting in spring/summer creates transient forces on the Earth at the glacier base or on the surrounding ice in englacial channels. You say later (Line 224) that water filling of the resonating structure could be responsible for changes in PSD. Then you say Line 229 that no water was present in the sensor settlement. In fact, the presence of meltwater inside the glacier does affect the seismic noise with strong noise generated in spring/summer and little englacial noise in winter. The noise recorded in winter may come from other sources in the area. This should appear on non-normalized spectrograms. In this case, there remains an open question on the cause of the resonating frequency shift from winter to summer. Is it related to source effects or structural changes ? I am confident in the interpretation of actual changes in elastic properties of the structure underneath the sensor. However, this question should be raised at the end of this section or in the discussion.

- ➔ Yes, we address this question (see major comment above). The new paragraph that we added is : "*The spectral content of seismic recordings can be affected by temporal variations of ambient seismic noise sources. For the two sites, these sources are assumed to originate from stable human activities located in the nearby valley and from weathering, but they could also be partly related to hydrological processes via melting water in spring and summer time. This source variability has to be addressed, in order to eliminate any spurious interpretation of actual changes in elastic properties. We then compare raw and normalized spectrograms of the reference station OGSA over one year (see Fig. 19 in Appendix C) to track any variation of the spectrum content which would prevent further comparison of frequency peaks observed on the rock glacier over time. No significant temporal changes of PSD appears within the illuminated spectrum of this stable station located near Laurichard rock glacier. Another obvious fact to highlight is that frequencies which were picked from ambient noise are also often visible when earthquakes signals are considered (see Appendix B). These two observations strengthen the direct link between these frequency peaks and rock glacier resonance.*"
- ➔ ". We also added a new Appendix (B) with a figure to show and clarify this point.

Figures 3/4: The blue boxes indicating the melting season are present only for spring. On what observations is it based on ? Generally in Alpine glaciers, the melting seasons spread from spring (April/May) to the end of summer (september/october).This would also be consistent with the seasonal cycle you observe for the seismic dominant frequency. Also related to this comment, I would say in Line 233: « a sudden drop of frequency occurs at the time when melting processes [start to occur in spring and stay stable to lower frequency over the course of the summer]. »

- ➔ Melting periods are defined based on thermal data acquired on thermistors located at the ground (around 10 cm depth) surface around the rock glacier. They are limited to the period when the "zero-curtain effect" (temperature constant and equals to the freezing point) is visible, which highlights the partial refreezing of melting water. We added a new figure showing these data in Appendix (Figure 16) in order to clarify this point. When the temperature goes up to positive values, we consider that the melting period is finished, since active layer is totally thawed until the surface. But of course, melting water from upstream still remains, extending the "melting period" to the end of summer, in a sense that you suggest.

Figures 3-4: I would here display the time series of air or ground temperature + periods of snow cover. This would ease the interpretation in the discussion and Fig 14

- ➔ Yes, we decided to add a new appendix with a figure showing the temperature time-series in Laurichard (Figure 16), as you requested.

Figure 4: Nice observation !

Line 247: I would add « at [spring and] summer time »

- ➔ Ok.

Line 261: Do you have a reference for the software ?

- ➔ Yes, we added a reference as bottom of page : " COMSOL Multiphysics® v. 5.4, www.comsol.com, ComsolLab, Stockholm, Sweden."

Figures 5-7: I suggest to move these figures to the appendix section as I think that the study should be focused on the modal analys/modeling methodology.

- ➔ Ok, we agree. These figures 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.

Line 391: « appling » -> applying

- ➔ Ok.

Line 446: « as shown in Fig 13 for Laurichard and not presented here but similar for Gugla » Results for both glaciers are presented in Fig 13, so you should revise this sentence.

- ➔ Ok, we removed this sentence.

Line 449: Section number missing (and elsewhere)

- ➔ Ok, checked.

Line 450: « and compare them to the maximum values of the observed ones ». You should indicate here that data are indicated by squares and rephrase this as the text alone is confusing. Say that you represent here the maximum and minimum bounds of the resonance frequencies you measure for each mode, for the two years - if I understand correctly.

- ➔ Yes, you understand correctly the Figure 13. But we clarify the text to prevent confusion : "*We compare them to the maximum (in winter) and minimum (in summer) values observed on all sites (depicted in squares in Fig. 13, values from Fig. 3 for Laurichard and Fig. 4 for Gugla).* "

Line 452-453: « Resonance frequencies of these modes match the freq band of measurements below 50 Hz, and generally decrease with thawing ». There is no relation between the two parts of this sentence so you should split it in two. The first part is a bit redundant with the previous sentence of the text. In general I think that section 4.5 could be better rewritten and reorganized with clearer description of the glacier seismic behavior.

- ➔ Yes, we reorganized a bit this paragraph. This sentence has been moved before, as it deals with general statements about expected resonance frequency.

Line 454: I would add at the end after « for all cases » « i.e. Gugla and Laurichard »

- ➔ Ok, added.

Line 455: You should remind here that C05 is towards the glacier tongue (on thinner ice H=8m according to your velocity model) and C00 is more upstream (H=14 m).

- ➔ It is not about ice thickness (unknown here), but rather about bedrock depth. This major difference between sensors C00 and C05 is reminded in this paragraph.

Figure 13: You could add arrows: one which points toward higher depths of thawing indicating summer, one which points toward 0 indicating winter

- ➔ Ok, we modified Figure 13 as requested.

Line 465: I would replace « melting periods » by « at the onset of the melting period in spring » (summer is also a melting period).

- ➔ Yes, we replaced this expression by: "*a sudden drop at the onset of melting periods in spring and lower values during summers*".

Line 471: « observed seasonal freq variations » -> observed freq seasonal variations

- ➔ Ok.

Lines 487-488: « in 2019 … frequency is lower than is 2018 » Give values !!

- ➔ We precise: "*Similarly for the Gugla site, the winter resonance frequency was significantly lower in 2017 (19 Hz) than in the others years (around 23 Hz).*"

Line 488: « In addition to an earlier snow cover period in 2019 than 2018 … » I would reformulate. Did snow falls started earlier in 2019 or was the period for snow cover shifted earlier in time ? « frozen » -> « refrozen » ?

- ➔ We reformulated : "*earlier and longer snow cover in 2019 than in 2018*".

Lines 475-489: I think this paragraph could be improved with some reorganization. You should highlight your conclusion which is that, on average, resonance frequencies are more sensitive to the intensity of internal thawing (which is influenced by …) than to the air temperature as … Also you use both the present and the past to describe the observations. You should homogenize this.

- ➔ We homogenized this paragraph and tried to clarify the examples together with the main message in conclusion.

Could you explain those differences with your model ? Can different level of porosity and depth of thawing mimic different intensity of thawing ? It would be very great to have a figure showing the time series of the observed resonance frequencies with the model results for different states of thawing.

- ➔ This request is already discussed as a major comment above. 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 (which state of freezing/thawing correspond to which time ?). We focus more of the extreme values (low values in summer, high values in winter), as depicted in the modified Figure 13.

Lines 489-498: I suggest to remove this paragraph from the discussion which is focused on the thawing-freezing cycle.

- ➔ We agree that the discussion deals mostly with freezing-thawing cycle, but we wanted to highlight the fruitful benefit of GPR results on the bedrock depth estimation, in combination with passive seismic measurements. Indeed, the difference of bedrock between the two sensors for Laurichard is the unique reason for the gap of resonance frequency amplitude between them. Thus, an accurately estimated bedrock depth is required to finely analyze and model the effect of seasonal thermal forcing on our seismic measurements. Let's us know if you still want to remove this paragraph from the discussion, in this case it can be moved to the appendix section.

Line 499: I would remove « non-linear ».

- ➔ Ok.

Line 500: « over the year » -> along the year

- ➔ Ok.

Line 501: I would remove « dry » as we do not know if you are referring to the air or the rock glacier.

- ➔ Ok.

Lines 521-522: « for ambient noise correlation method, the theoretical relative velocity change of the Rayleigh wave is computed by dispersion curve difference using the Geopsy package» -> I would say « for … corr method, we compute the dispersion curves of the Rayleigh wave using the Geopsy software. The theoretical relative velocity changes are computed by measuring the differences of the dispersion curve with the reference one at each frequency. »

- ➔ We reformulated: "*for ambient noise correlation method: we compute the dispersion curves of the Rayleigh waves using the Geopsy package (Wathelet et al., 2004). The theoretical relative velocity changes (dV/V) are computed by measuring the difference between the modified dispersion curve with the reference one at each frequency*".

Figure 15: For the modal analysis, you say « first mode » while everywhere in the text you refer to the fundamental mode. This could be confusing. You should specify the resonance frequency, and also in the main text.

- ➔ Yes, we changed the "first mode" to "mode 0". Mode 0 is already mentioned in Figure 13 and is commonly used for referring the fundamental mode.

Line 538: I would say « continuous seismic noise measurements »

- ➔ Ok.

Line 539: This is minor but I would say something like « These freq show seasonal variations that are to be related with changes in elastic properties of the structure underneath the recording sensor, due to freeze-thawing effects ».

- ➔ Ok, we modified by: "*These frequencies show seasonal variations, related with changes in elastic properties of the structure underneath the recording sensor, due to freeze-thawing effects*".

Line 541: « which fit well the recorded frequencies » I would specify here or next sentence on what the resonance freq depend in the poroelastic model. Basically the take-home message I keep from your analysis is that the freq depend on the maximum depth of thawing in a porous medium.

- ➔ For us this sentence is a general statement for the good agreement between modeled and measured extreme values of resonance frequency. We detailed below this take-home message (see next comment).

Line 542: « we have reproduced the observed seasonal variations » This is a strong statement, you did not reproduced the seasonal cycle exactly but only reproduce the maximum freq in winter due to maximum freezing and minimum freq in summer due to maximum thawing. See my major comment.

- ➔ Yes, we precise : "*we have reproduced the observed higher values in winter due to maximum freezing, and lowest values in summer due to maximum thawing*".

Line 551: « insight to other deeper processes » -> into other processes at greater depth

- ➔ Ok, replaced.