The Cryosphere Discussions

The Cryosphere Discuss., referee comment RC1
https://doi.org/10.5194/tc-2021-219-RC1, 2021


# Comment on tc-2021-219

Anonymous Referee #1

Referee comment on "Seismic physics-based characterization of permafrost sites using surface waves" by Hongwei Liu et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-219-RC1, 2021

## General comments

This article proposes an original use of seismic methods to characterize a permafrost area. The main interest of the study lies in the identification and interpretation of two types of Rayleigh waves propagating in a frozen porous medium. The separate inversion of the two dispersion curves provides an hybrid method for determining independently the physical and mechanical properties of the medium, thanks to the difference in the respective sensitivity of these two waves to these properties.

The article invites the use of this method to characterize a permafrost medium, as it appears to be more efficient and requires fewer a priori assumptions about the investigated medium.

The authors mention various applications to the detection and characterization of permafrost, ranging from civil engineering and infrastructure monitoring to the assessment of the potential vulnerability of certain areas to permafrost degradation and associated feedbacks.

The article is well structured and adequately written. A significant contribution is that authors used seismic data collected at a site in Svalbard, and applied their processing to this experiment, to show a real application of their method.

In my opinion the paper deserves publication after minor revisions.

I suggest several edits.

First, the contribution of this study to the current knowledge of seismic waves propagating in permafrost is not very comprehensible to the reader. The lack of references about the poroelastic model and the lack of physical interpretation of the two Rayleigh waves should be corrected.

Also, the authors should include a fuller explanation of their field experiment in Svalbard (with a figure), to clarify what data they have collected and what their real contribution (instrumentation, data processing, ...) to this site.

More generally, there is a lack of references addressing issues which the authors mention. For an example, the applications (early warning systems and permafrost carbon feedback vulnerability) are frequently mentioned, but have to be more documented.

Finally, uncertainties of this new method must be addressed more quantitatively, in order to better assess its benefits and drawbacks over other methods.

**Specific comments**

Applications : early warning systems and permafrost carbon feedback vulnerability -> I suggest to add more details about what could be applied, and more referenced. Otherwise, these applications would be mention with caution only in the discussion part .

Discussion : In Figure 7c is shown the results of the inversion of shear modulus over the offset distance. The reader can observe a huge value of shear modulus in the permafrost layer located at a offset distance from 500m to 600m. Why this order of magnitude much higher than other parts of the whole profile ? To my mind, this results must be addressed in the discussion as well.

L237 : according to this sentence, the ground temperature is deduced from soil temperature among others. How did you get this soil temperature data (modeled, measured on the field ?) ?

Uncertainties : RMS values have to be systematically computed, in order to quantitatively assess the accuracy of all steps of your inversion algorithm. For example, in Figure B3 : why such a misfit between R1 experimental and numerical dispersion curves, comparative to other locations ? I suggest to add a discussion of this issue.

**Technical corrections**

Abstract :

L.5 : the term “relatively” is quite imprecise for an abstract, I suggest to remove it.

L.7 : “Permafrost and soil layers” is inappropriate, since permafrost are considered as soil as well. Maybe replace it by “active and frozen permafrost layers” ?

L.8 : “shear and bulk moduli”

Introduction :

L16 to 19 : I would add some references about permafrost thermal definition and permafrost basics.

L16 : I would replace “upper” by “shallower”

L17 : The expression “freeze-thawing cycles” is more common, maybe replace by it.

L27 : I would add at least one reference for ice wedge definition.

L28 to 37 : For these important applications that you mention, more reference and details are expected. For example, does the terms “thaw-stable” and “thaw-unstable” well documented ?

L29 : I would remove “amount of”

L38 to L50 : for all the geophysical methods on permafrost, maybe more references are expected.

L51 to L60 : a reference for the MASW is expected. For passive methods using ambient seismic noise on permafrost sites, you can add recent references in mountain permafrost (Guillemot 2019, Lindner 2021, Albaric 2021), to develop the state of the art about these methods.

L61 : In this paragraph, I would add some sentences to define shortly but precisely all the four terms that you mention in your approach : "hybrid", "inverse", multi-phase" and "poromechanical".

L64-L65 : I would remove these sentence about potential applications, since you already mention them above. Maybe you can even suggest these application in the discussion and/or conclusion parts.

L70 : remove the article "the" in "for the assessment"

Methods:

L74 : change "the overview" to "an overview"

L75 : "surface wave measurements" Maybe you must develop the technique used in details, or precise if these seismic tests are active or passive.

L100-102 : Are this statement and this equation for extracting Rayleigh wave dispersion relation ? If yes, please precise explicitly.

L103 : I would replace "a constant frequency" by "one given frequency"

L199 : I would add "respectively" in this sentence

L122 : please, precise what are the two tuning parameters. Are they chosen among the optimization variables mentioned above ?

L138 : the term “Here” is not clear, you must precise if you mind “in our model” or more focused on one layer of your model.

L147 to L159 : This paragraph would be improved by adding some references or figures that illustrates your statements. Actually, it is not very clear for the readers whether the elements are your contribution, or from the current state of the art. For example : the existence of two Rayleigh waves, the respective dependency of R1 and R2 waves to parameters (mechanical and physical). If references exist about these questions, you must add them here. Overall, some physical interpretations will be appreciated : for example, is the higher R1 velocity  than R2 velocity easily interpretable in a physical point of view ? Is the difference of sensitivity to physical and mechanical properties between R1 and R2 surprising or expectable ? Why ?

L174 : if you can, precise the type of geophones (type, natural frequency)

L181 : why “almost completely frozen” ? Precise why you choose the value 85% for the degree of saturation of unfrozen water.

L195 : I would add a reference for illustrating this statement

L210 : the term “sufficiently close” must be completed by a quantitative assessment (RMS ?).

L227 : “We also predicted”

L239 : I suggest to replace “is highly related” by “could highly related”

Discussion and conclusions:

L249 : “makes the analysis more efficient” : you must tell more about this statement : What do you compare this method to ? And, have you done a quantitative assessment to support this discussion?

L255 to 257 : this sentence must be documented by at least one reference.

L276 : for the case of a potential early warning system, how do you plan to deal with the seasonal variations (ex: freeze-thawing cycles of the active layer) that you would measure over one year? Do you have any idea how to model and remove such environmental influences that are not related to damage? And how to fix critical values ? If you have any ideas on this issues, you would be welcome to mention them, in order to strengthen your discussion on this potential application.

Figures:

Figure 1: I would precise in the legend that variable (n, Sr, H, K, G) are defined for each layer (layer 1, layer 2, layer 3).

Figure 2 : this figure is not very explicit. Please clarify the definition of G, m, h.

Figure 3 : colorbar ?

Figure 4 : a map with geographic location of the study site in Svalbard, and another focusing on the location of all different profiles of geophones that we used for this study would be appreciated.

Figure 5-6 : the vertical-horizontal ratio scaling to modify in graphs ?

Figure 7 : You show results of some outputs of your inversion, but what about the results of bulk modulus ? For (d), scaling has to be modified. And also for the sake of simplicity, the predicted average soil temperature distribution may be removed from this figure, since this variable do not seem to be useful for the study.

Appendices:

Appendices A, C and D: there is a lack of references in these parts. I suggest to add at least Carcione & Seriani (2001) and Leclaire (1994).

Appendix B : in all figures you show both saturation degree of unfrozen water and saturation degree of ice, but only one seems to be useful, since the two variables are directly linked together. Furthermore, what about the results of the layer thickness from this surface wave inversion ? It could be appropriate to show them as well. Again, for R1 and R2 experimental and numerical dispersion curves, it should be good to precise misfits through RMS values.

L297 : I suggest to add “respectively”

L382 : “Convention” instead of “convection”

L396 : “The values of each component” instead of “The value of each components”

Appendix D : L432 : I suggest to remove “the matrix formed by” for consistency