General comments

The main contribution of this article is the identification of two wave modes that interact sensitively with the porous and mechanical properties of a layered poro-elastic medium, respectively. In addition the authors use a multiphase poro-mechanical foundation to build a global matrix model of a layered poro-elastic medium. This approach has significant novelty compared to the typical global matrix models based on partial wave amplitudes and nicely emphasizes the connection between surface waves and ground physical properties. The specific application to permafrost demonstrates the relevance of this manuscript to The Cryosphere, although certain aspects of the methodology also have broader relevance to the field of near surface geophysics and non-destructive testing of engineering materials.

The weaker side of the manuscript is that findings that are a direct result of the data examples presented are not adequately separated from other applications that remain essentially hypothetical (these are detailed under "specific comments"). The manuscript could also be improved substantially by giving a more complete description of the two Rayleigh wave modes so that those reading the manuscript may better understand their propagation and interpret their broader relevance to the field of surface wave seismic investigation. Furthermore, I have some concerns that the inversion results are overly sensitive to the frequency range of the dispersion curves that constrain the inversion (likely due to a mismatch between the shape of the experimental and inverted dispersion curves). The anomalous result at 360-480 m in the physical data example is not convincing and appears more likely to reflect a weakness in the inversion methodology than real lateral variation in physical properties. Furthermore, it is generally difficult to assess the true accuracy of the results owing to a lack of comparison to ground truth observations of physical properties and interface depths or comparison with other geophysical datasets (both of which appear to exist in the published literature). I believe it should be possible for the authors to address these concerns in a revised manuscript, that will then make a useful contribution to The Cryosphere.

Specific comments

1. The authors claim that their methodology can be used to "characterize a permafrost site more accurately" (line 12). However, since we have no baseline for comparison it is difficult to assess to what extent this is the case. The Glazer (2020) study that is already cited by the authors could be used as a direct comparison in order to place the results of the present study in context. The ERT results of Glazer (2020) may provide a means of independent validation, while the MASW results of Glazer (2020) use more conventional processing of the same dataset as used in this study and could provide an excellent benchmark to highlight the benefits of the new hybrid inversion approach.

   Szymański et al. (2013) have published direct sampling results of 34 soil pits for the Fuglebekken area. It would be highly valuable for the authors to refer to this study in order to place their results in a geological context. Significantly, Szymański et al. (2013) describe the area as consisting of crystalline bedrock covered by marine deposits with thickness up to 4-5 m. Is there a possibility that the interface between the active layer and the ice bearing permafrost that the authors place at ~4 m depth is really the sediment-bedrock interface?

Szymański, W., Skiba, S., & Wojtuń, B. (2013). Distribution, genesis, and properties of Arctic soils: a case study from the Fuglebekken catchment, Spitsbergen. *Polish Polar Research*, 289-304.

2. The dispersion spectra for the R2 wave (Fig. 1b) looks very similar to the Rayleigh-Lamb waves described by Ryden & Park (2004) for pavements and also shown to occur in permafrost settings by Romeyn et al. (2021), although the experimental data in the present study does not resolve the higher order modes. It would be beneficial for the authors to refer to this work, particularly since the global matrix method employed by Ryden & Park (2004) is similar to the theoretical development of this manuscript. It may be purely an issue of terminology, but the authors could also consider that the surface waves identified in the manuscript may be more accurately considered Rayleigh-Lamb waves, since the stiff ice-bearing permafrost layer likely acts as a waveguide to some extent. The following passage from Ryden & Park (2004) provides some perspective on this topic:

> *"It is usually assumed that Rayleigh waves are the prevailing type of waves generated, with a depth penetration of about one wavelength (Viktorov 1967). However, it has been reported that this assumption holds strictly only at sites where the stiffness increases smoothly as a function of depth (Foti 2000). At sites with a velocity reversal (i.e. stiffness decreases with depth), the nature of surface-wave propagation has been reported as more complicated than at sites with normal dispersion. Several studies have indicated that a measured dispersion curve where the phase velocity increases with frequency, i.e. inverse dispersion, is actually built up by small portions of higher modes (Gucunski and Woods 1992; Tokimatzu et al. 1992; Forbriger 2003; Foti et al. 2003; Ryden et al. 2004)."*

Rydén, N., & Park, C. B. (2004). Surface waves in inversely dispersive media. *Near surface geophysics*, *2*(4), 187-197.

Romeyn, R., Hanssen, A., Ruud, B. O., Stemland, H. M., & Johansen, T. A. (2021). Passive seismic recording of cryoseisms in Adventdalen, Svalbard. *The Cryosphere*, *15*(1), 283-302.

3. The proposed application in "early detection and warning systems to monitor infrastructure impacted by permafrost-related geohazards" (line 12) is not sufficiently developed. It should be established by data, reference to other studies or at least step-by-step logic that precursor change in physical properties could be detected by the proposed monitoring methodology in advance of changes that result in structural damage, for example. If such evidence does not exist, this application should be limited to a briefly describing that this early detection system is a goal that will be pursued in future studies.

4. The possibility to "detect the presence of layers vulnerable to permafrost carbon feedback and emission of greenhouse gases into the atmosphere" (line 13) is not convincingly demonstrated and may overemphasize the direct relevance of this study to assessment of the global carbon budget. If one reads carefully through the manuscript, this statement comes down to the hypothesized ability of the study methodology to detect the presence of peat in the subsurface. The authors have not argued how distinct the physical parameters of peat are from other soil

types and to what level of confidence it could be detected in practice. Ideally, there should be at least one real data example of a known peat layer being detected by proposed methodology. Synthetic data could also be usefully employed to demonstrate the hypothesized application. I would further suggest that the authors describe specifically that there are two steps, 1) detection of peat layers 2) estimation of carbon content. It should otherwise be clearly demonstrated that the variation of organic carbon content of soils that are otherwise similar leads to a detectible variation in mechanical properties using Rayleigh wave modes 1 & 2.

5. Line 16-19. Missing reference. In particular "The thickness of the active layer depends on local geological and climate conditions such as vegetation, soil composition, air temperature, solar radiation and wind speed" should be supported with one or more references. The active layer undergoes seasonal freeze and thaw cycles by definition so to say "may undergo" seems strange (I assume this was an oversight and have added a technical correction).

6. Line 24. Missing reference. Please add one or more references that support the excessive deformation in frost-susceptible soils caused by segregated ice formation.

7. Line 27. Missing reference. Please add one or more references that describe the ice-wedge formation process and its timescale.

8. Line 33. Missing reference. Please add one or more references that describe thaw settlement and associated loss of strength.

9. Line 56. Missing reference. It is stated that it is common practice to associate anomalously high shear wave velocities with permafrost, but no references are given to studies that are examples of this practice.

10. Line 64. Similar to point 3. It is not sufficiently clear what the authors mean by "we can also predict the soil type and the sensitivity of the permafrost layer to permafrost carbon feedback and emission of greenhouse gases to the atmosphere". It seems natural to guess that this means the ability to quantify the amount of organic carbon present in the soil, but it is not clear to what extent the proposed methodology is capable of this.

11. Figure 2. The layer stiffness matrices should be defined in the caption. Perhaps this figure should be dropped entirely since it is minimally illustrative when the layer matrices are only given in the appendices.

12. Line 108. "The global stiffness matrix for the R1 wave can be decomposed into the components related only to the P1 and S1 wave velocities." There should be a reference to an equation associated with this statement.

13. Line 109. “proved that the R1 wave is generated by the interaction between the P1 and S1 waves.” Include a reference to section 3 where this argument is made so that the reader does not get lost here.

14. Line 151. “The seismic measurements shown in Figure 3a are indeed a combination of both R1 and R2 waves.” These are not seismic measurements; they are synthetic data. It would perhaps be better to say “The surface waves shown in…”. Is the conclusion that Fig. 3a shows R1 and R2 waves based on the velocity match? It could be clearer what the authors are trying to convey with this sentence.

15. Figure 3. There is a lot of wasted space with zero amplitudes in Fig. 3a. While it shows the velocity moveout, it fails to illustrate the detailed waveforms of the R1 and R2 waves. It would be very useful to include at least one detailed timeseries example to illustrate the waveforms of these waves. The identification of the R1 and R2 waves is a key part of the main contribution of this article and I do not think the current figure is adequate to illustrate their characteristics. There is also no colour scale on Fig. 3b-c.

16. Line 176. How are the experimental dispersion spectra obtained? One assumes the phase shift method of Park (1999), but it is not stated. Are the dispersion curves manually picked or fitted to the spectral peak by a semi-automatic method? The only detail given is “The R1 and R2 Rayleigh waves are identified by visual inspection to obtain the experimental dispersion relations”. This seems radically insufficient given that the experimental dispersion relations are the key inversion constraint. One would expect that every detail surrounding the dispersion curve picking should be fully accounted for given their importance to the manuscript.

Park, C.B., Miller, R.D., and Xia, J., 1999, Multichannel analysis of surface waves (MASW): Geophysics, 64, 800–808.

17. Line 185. “The unfrozen ground is believed to have a degree of saturation of unfrozen water of about 100% (fully saturated)” is this based on previous studies? Please give a reference to support the assumption.

18. Line 194: It does not seem clear that the soil type, it’s mineral composition or it’s organic carbon content have been resolved in the present study. It seems defensible that the presence of an ice rich layer has been demonstrated but the authors seem to be speculating far beyond this and these speculations should be either moderated or backed up with real or synthetic data examples to illustrate that they are feasible.

19. Line 201: “Given the high ice-to-water ratio, we therefore interpret the permafrost is currently in a stable frozen state.” The logic is unclear or not fully developed here. Unstable permafrost is distinguished by whether the ice and water saturation exceed the total pore volume of the ground in an unfrozen state. It seems that 22% water plus 91% ice (maximum values of uncertainty ranges) could exceed the pore volume in the permafrost layer and could therefore be considered unstable, though on the lower end 8.8% water plus 77% ice would be less than

the pore volume in the frozen state of the permafrost. However, the frozen permafrost has elevated porosity compared to both the overlying and underlying layers. If we consider that water plus ice saturation in the permafrost is at least 86% of a frozen pore volume of 0.43-0.46, then it will likely be more than 100% saturation if the porosity in the unfrozen state is approximated by the porosity of the overlying active layer and underlying unfrozen ground (represented as 0.34-0.36 if we take the zone of overlap between the two porosity estimates). I think the authors should explicitly step through their assumptions and calculations in determining the stability of the permafrost, because this is a significant application area of their methodology and could be a major strength of the work if developed to its full potential.

20. Figure 4. The dispersion spectra use a colour scale that has an orange colour which appears in two different amplitude ranges making interpretation of the spectra ambiguous. I also don't understand why the manually picked dispersion curves are not overlain on the spectra. This is particularly important because the spectra are somewhat poorly resolved and identification of the precise dispersion relation is far from straightforward from these data.

21. Figure 5. It is concerning that the prediction envelope of the R2 dispersion relation is concave down at frequencies below 20 Hz, while the experimental measurement is concave up. Looking at the experimental dispersion image for the R2 wave in Fig. 4d it seems quite clear that low frequencies should trend towards high phase velocities. The implication here is that some important parameter of the system is not resolved by the inversion. I will come back to this, but it may be the root cause of the anomalous result reported for the 360-480m section.

22. Line 225. "the permafrost table is generally located at about 4 m below the ground surface, except at the offset distance from 360 m to 480 m where the permafrost table is located at 1.1 m below the ground surface." Is there a geologic or geomorphologic explanation for this variation, e.g., topography, vegetation, surface-water etc.? Is there otherwise some other geophysical dataset that could corroborate this? It seems rather implausible that the permafrost table is so dramatically elevated at one anomalous location. If the anomalous result at 360-480 m cannot be explained from a reasonable physical or geological perspective then it rather points towards a significant degree of uncertainty or instability in the inversion.

23. Line 233. "Sufficient agreement exists between the numerical and experimental dispersion relations for the R2 wave (Figure 7d) which confirms the acceptance of the predicted values for the volumetric ice content (calculated as the product of porosity and the degree of saturation of ice) and porosity ". I find it difficult to agree with this statement. The model and experimental dispersion curves have notably poorer correspondence for the 360-480 m section, which is the only section that gives a significantly different inversion result. This points towards model misfit rather than physical reality.

24. Figure 7. It is not convincing that the anomalously shallow permafrost table, high ice content result at 360-480 m reflects a real variation in ground structure/properties. The experimental dispersion curves look quite similar in the overlapping frequency ranges, but the 360-480 m dispersion curve extends to lower frequencies than the others do. It would be beneficial to examine a figure plotting all dispersion curves on a shared axis so the reader can see where and by how much they really vary (but this is of course up to the authors discretion). In all cases, it looks like the experimental dispersion curves are concave up at low frequencies and the R2 prediction envelopes are concave down. This mismatch is exacerbated for the 360-480 m section, which extends to lower frequencies than the others and therefore leads to the anomalous result. It is difficult to say which result is closer to reality because of a lack of comparison with ground truth observations or other geophysical data sets. The frequency range from ~13-20 Hz is where the phase velocity of the R2 wave varies most significantly (Fig 4d) so it is concerning that the inversion seems to have problems matching the experimental curve in exactly this part of the frequency spectrum.

25. Line 238. “at the offset distance from 360 m to 480 m the coldest temperature of about -12 ◦C (Figure 7e) occurs in the permafrost layer, which is highly related to the high ice content in this section.” Again, a more nuanced interpretation is required. It is difficult to accept that the anomalous data section, with the poorest correspondence between model and experimental dispersion curves can simply be interpreted as a real physical effect without giving a supporting physical explanation.

26. Line 253. “the mechanical properties of the solid skeletal frame can reveal the type of soil”. How much overlap in mechanical properties is there for different types of soils and how does the estimation compare with the soil pit sampling study of, e.g., Szymański et al. (2013) which covers the Fuglebekken area?

27. Line 256. “if the mechanical properties of the solid skeletal frame correspond to the ones for peat we can perform more detailed investigation to assess the sensitivity of the permafrost to greenhouse gases emission.” It is important to communicate that this application remains hypothetical, since the ability to resolve the presence of a peat layer has not been demonstrated in this study. Perhaps the authors would consider adding a synthetic data example including a peat layer if they feel this is an important application to emphasize.

28. Line 260. “we can reasonably consider the permafrost layer at the offset distance from 360 m to 480 m to be ice-rich and ice segregation layers are expected to contribute to its relatively higher volumetric ice content.” This seems to require an assumption of the porosity in the unfrozen state, which is not given explicitly but should be, so that the reader can follow the authors line of reasoning. It would also be valuable to discuss if it is physically reasonable for a change to occur at this location alone, while all other locations consistently gave a different result.

29. Line 267. “The uncertainty originates from the non-uniqueness in the inverse analysis (local minima problem) and the limited number of constraints in the inversion analysis”. The sensitivity to small changes in the experimental dispersion curves is not adequately covered in the manuscript. For example, the 360-480 m section has an experimental dispersion curve that appears quite similar to the other sections, but extends to a lower frequency range and gives a substantially different inversion result. More generally, there is always some uncertainty in picking the dispersion curve from experimental data and it is unclear how this uncertainty may propagate through the inversion. How do the results differ for a set of dispersion curves that are indistinguishably close from an experimental perspective? The R1 dispersion spectra in particular is quite poorly resolved (Fig. 4c) so one must assume some degree of uncertainty is associated with the picked dispersion curve.

30. Line 268. “recommended to use other geophysical methods to improve the resolution and reduce uncertainty of the permafrost mapping.” Why are the inversion results of the field example not discussed in the context of existing geophysical and direct sampling results? This is a crucial step in qualifying the validity of the proposed methodology.

31. Line 272. “The proposed hybrid inverse and multi-phase poro-mechanical approach can potentially be used for the design of an early warning system for permafrost by means of an active or passive seismic test.” It seems that too much emphasis is placed on this hypothetical future application while the more important topic of qualifying the inversion results in the context of other geophysical methods, direct sampling, geological and geomorphological understanding etc. is lacking. There is no convincing argument that changes in poro-mechanical properties that would be detectable with the current methodology occur in advance of physical surface expressions such as subsidence or cracks in structures. This would presumably be a key requirement of an early warning system.

32. Line 277. “The early warning system can provide long-term tracking of permafrost conditions particularly when the ice content or mechanical properties of permafrost approach critical values.” What are the critical values? Again, either the concept of an early warning system should be developed fully and convincingly, or it should just be mentioned briefly as a goal for future research efforts.

Technical corrections

1. It would be much easier to read if references to appendices were presented in the form “Appendix D” not simply “D” e.g., line 90 “the matrix… are given in D” would become ““the matrices… are given in Appendix D””
2. Line 17 “the active layer, may undergo seasonal thaw and freeze cycles” should be “the active layer, undergoes seasonal thaw and freeze cycles”
3. Line 29 “This distinction is determined by the amount of ice content within the permafrost.” Should be “This distinction is determined by the ice content within the permafrost.” OR “This distinction is determined by the amount of ice within the permafrost.”, amount and content both refer to the same quantity here.
4. Line 30 “Ice-rich permafrost contains ice in excess of its water content at saturation.” Could be modified to “Ice-rich permafrost contains ice in excess of its water content at saturation and is thaw unstable.” In order to improve the flow of argumentation in the surrounding paragraph.
5. Line 47 “GPR has been also used” should be “GPR has also been used”
6. Line 50. “none of the above-mentioned methods characterizes the mechanical properties of permafrost layers.” Should rather be “none of the above-mentioned methods directly characterizes the mechanical properties of the permafrost.”

7. Line 67. “based on an MASW seismic investigation in a field located at SW Spitsbergen, Norway” should rather be “based on a MASW seismic investigation of a field site located on SW Spitsbergen, Svalbard”.
8. Line 77. “A random sample is initially generated to ensure that soil parameters are not affected by a local minimum” makes it sound as if it is a single initial sample. I think the following might be a more correct representation of what the authors mean to express “A set of initial values, randomly selected and spanning the multidimensional parameter space ensures that soil parameters are not affected by a local minimum”. Same comment applies to line 123.
9. Figure 1 caption. “Dispersion relations of R1 and R2 waves” should be “Dispersion image of R1 and R2 waves”. The annotation on figure panel (b) should also be changed since the figure shows dispersion images and not curves.
10. Line 167. It is more geographically descriptive to write SW Spitsbergen, Svalbard (rather than Norway).
11. Line 169. Why not give the number of geophones directly? E.g. “The MASW test was performed by using 60 geophone receivers spaced at regular 2m intervals”.
12. Line 193. “detection of the thin ice lenses using low frequency seismic waves is highly impossible due to the mismatch between the thickness of the ice segregation layers and the wavelength generated in seismic tests”. It is not valid to say “highly impossible”. Why not simply say that ice lenses cannot be detected directly below 1/4 lambda, or whatever fraction of a wavelength is believed to be the correct detection limit here? To describe the phenomenon as a mismatch between wavelength and thickness is rather vague.
13. Line 201. “with a nearly 8.8%-22% of degree of saturation” it does not make sense to say nearly followed by a range, just give the range and omit “nearly”.
14. Line 209. “sufficiently close” is a highly subjective description. “show good visual agreement” is perhaps what the authors intend to convey, but the phrasing should be made more descriptive in any case.
15. Figures 5, 6, B1-B4 and line 258 in text “Saturation degree” should be “degree of saturation” which is the correct terminology and that which is mostly used throughout the text.