Response to Editor

Thank you for your prompt submission of revisions. In my opinion, you have largely satisfied reviewer concerns and suggestions. However, there are one or two details that should be addressed before we can accept your manuscript for publication.

1. You now provide a detailed explanation of the colors representing the response spectrum as requested by the reviewers. I think it would be helpful to include a simplified version of this explanation in each relevant caption.

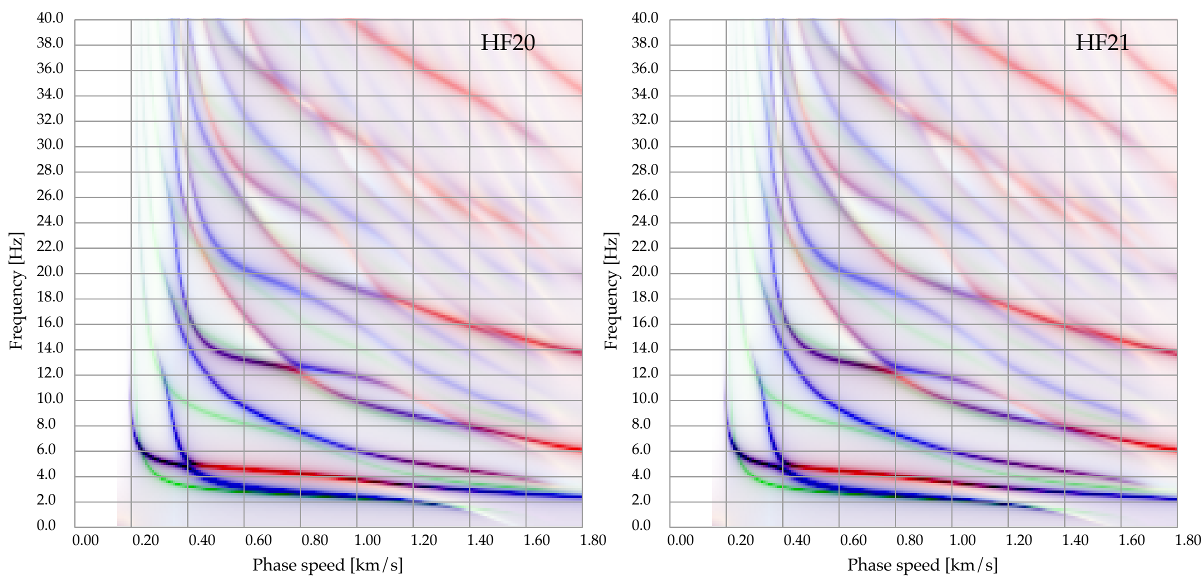
The sentences:
"The maximum plotted amplitude for all components is set the same and so the relative excitation of the modal branches can be assessed by the apparent width of the mode branch in the plot. The dispersion is the same for the two components of the Rayleigh wave and so the apparent colour changes with the surface ellipticity."
have been added to each of the captions for the response spectra.

2. Reviewer C asked for a citation for equation 2. It sounds like the expression is based on unpublished work? If so, it would be appropriate to provide a short explanation of how it was derived, along with some example density values.

Thank you for raising the issue of the density again. After checking my models, I found that I have used the relation $\rho(z) = 1.0 + 0.2\alpha(z)$ in all the examples used in the paper (the other version was used in preliminary work). This relation has a closer relation to published work. An explanation and link to the work of Brocher (2005) is now supplied.

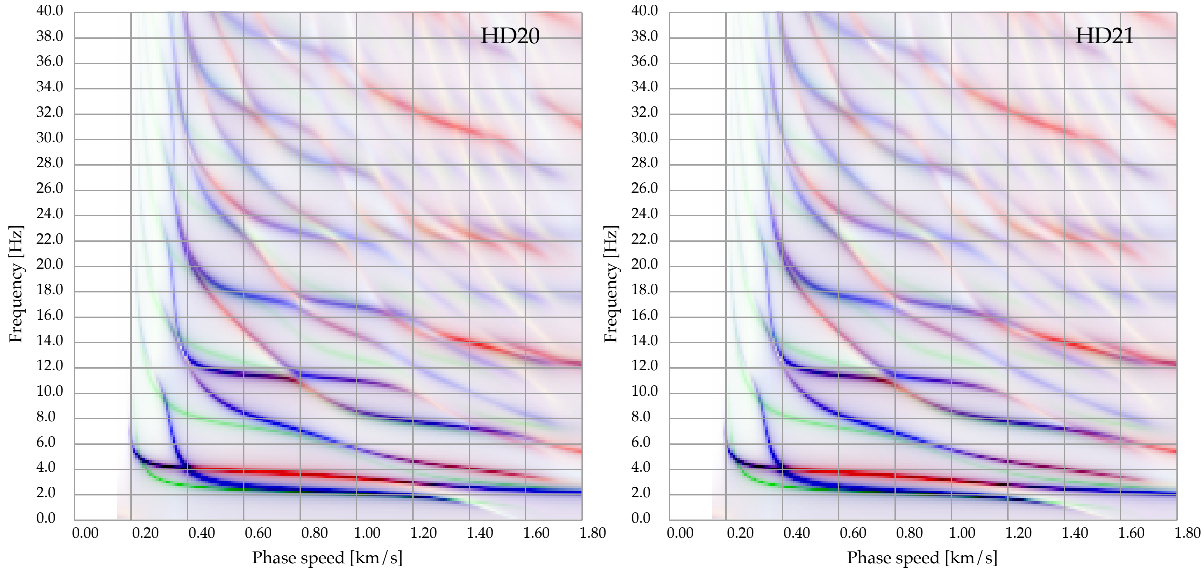
The values of density employed have little impact on the dispersion spectra and the displayed equation (2) gives too much prominence to the values used, so I have moved the correct equation into the textline.

I have checked the insensitivity of the dispersion spectra to the density by replacing the density profile by a constant surface value. This is illustrated for the equivalent of model H202 below. The left-hand panel has varying density and the right constant density



As can be seen the effect of density change is essentially invisible.

It might be expected that larger impact would come with a large wavespeed jump because of the impendence change and consequent modification of reflection coefficients. The equivalents of model H200 are therefore also shown with constant density on the right.



Once again, the impact of modifying the density profile is extremely small.

After addressing these details, please upload updated manuscript and marked-up manuscript files.

A revised manuscript and marked up files have been provided (marked as “rev2”)

Response to review:

Editor

Reviewer B feels that the manuscript is well-written and concise. They would like to see some additional discussion of model parameterization. Specifically, the use of functions of depth to prescribe velocity rather than homogeneous layers. Reviewer C indicates that while the theoretical work is strong, they would like to see an expanded discussion about the practical implications of the work. That reviewer would also like to see more detailed summaries of the major takeaways in both the conclusion and abstract sections. Your figures are gorgeous, but both reviewers mentioned the labelling and colormaps would benefit from some clarification. It took me a minute to realize that the gray shading in the legend in Figure 1 corresponded to shading in the green and red curves. Both reviewers provide some line-specific suggestions that I believe you will find helpful as you revise.

The manuscript has been extensively revised, particularly in the *Introduction* and *Discussion and Conclusions* sections, to try to meet the issues raised in the very helpful reviews. I have strived to retain the clarity that impressed the reviewers.
I have responded to each of the points raised by the reviewers below.

Extra explanation is provided for the interpretation of colour in the various figures, e.g., in the caption of Figure 1. The contrasts between models have also been enhanced as requested by Reviewer C.

I have recognised the helpful reviews in the Acknowledgements.

In the revised version new material is indicated in blue and modified (or moved) material is shown in red.

Reviewer B:

The manuscript analyses the influence of gradients and their parametrization on the dispersion characteristics of Rayleigh and Love wave modes, including several overtones. The focus is on shallow subsurface studies of sediments and ice. It is clearly written, easy to read, well illustrated and to the point. How different gradients in the subsurface influence the details of the dispersion characteristics is shown with different examples and can be useful for data interpretation and inversion. The last section with Figure 10 showing the difference in mode observability with 3-components, vertical only or DAS recordings is particularly insightful.

My main comment concerns the choice of parametrizing the model with homogeneous layers when trying to infer the influence of gradients. This gives the impression that using homogeneous layers is the only way of calculating the wavefield. It is possible to integrate the seismic wave equation with depth numerically in models with continuously varying profiles, dating back to Takeuchi and Saito (1972). In a paper about gradients, this should at least be pointed out so that readers are made aware of the option of parametrizing with for example linear functions in different depth ranges, removing the problem discussed in section 4.
A new paragraph describing the use of differential equations to calculate dispersion for arbitrary dispersion profiles (following Takeuchi & Saito) has been added, with a contrast to the use of discrete layers in the frequency-phase speed spectra used to capture all modes at once.

The choice of coloring the Rayleigh wave dispersion curves from blue to red to show the ellipticity is very elegant, but the text and the figure caption should be more clear that the color is varying from

blue to red etc, not just blue and red. I guess the intensity of the colour says something about the amplitude of the mode. The sentence line 66 about amplitude says something else, but is not totally clear.
A specific description is now given of the nature of the modal representation and the use of colour.
More discussion of the approach of Kennett (2023) is also provided in the introduction to provide context.

How much attenuation is there in the model? It is a realistic value and how does this influence the sharpness of the images?
The values $Q_\alpha = 100$, $Q_\beta = 50$ are used throughout – this now mentioned in the text.
These Q values are somewhat high for the near surface but allows all modes to be seen.
Lower values would reduce the amplitude of the higher frequencies particularly in the top left-hand corner of the frequency-phase speed diagrams

line 106: I do not see in the figure the feature you are describing here.
A clearer description is now provided.

line 108: "weak radial" --> "dominantly vertical" would be clearer?
Sentence reworked

Typos:
line 36: "in when it"? corrected
line 62: "are vi The"? corrected and expanded
line 177: "every" --> "very" corrected

The associated software is available.

Valerie Maupin

Reviewer C:

The manuscript investigates dispersion characteristics of theoretical velocity profiles, with special interest in cases with a gradient distribution of the wave speed. The study compares theoretical dispersion characteristics for simple one-layer profiles that differ by the gradient slope and layer thickness, as well as more complex two-layer scenarios. In addition, the author shows the influence of the layers' discretization and recording setup on dispersion characteristics. All in all, the conclusion is that higher modes, if they are distinguishable and correctly recognized, could help to better infer velocity structures if the inversion algorithm is adequately parameterized. I think it is a bit of a general and vague conclusion. I think it is an interesting exercise in terms of showing the influence on the dispersion characteristics of the aspects of the velocity profiles that are often ignored; however, the more practical implications of this study are not completely clear. As a practitioner working on inversion for shear wave velocity, I would be interested to know more about how it can be translated into practical solutions.

The structure of the manuscript is incredibly clear and logical; figures are also well-presented. However, it is a very technical manuscript; hence, it may be difficult for readers who are not experts in this topic.
The paper was aimed to have some didactic content to make a complex topic accessible, it is difficult to avoid some level of "technicality".

I think the manuscript should be published with minor revisions. I would suggest adding some more discussion on the practical implications of different findings. In addition, I have some specific comments listed below:
The *Discussion and Conclusions* section has been expanded

• *Introduction*
Line 46 – is the finding that a reduction by a factor of 3–4 is possible, a conclusion of the present study, or is it based on previous work? I think it is the former, so it should not be part of the introduction, but of the discussion or conclusions.
The material has been modified and further points are presented in the *Discussion and Conclusions* section.

Lines 21–24 – are there field examples (for instance, from ice sheets) where higher modes were helpful to invert for the velocity profile with strong gradients? Is it a new idea to use them for his purpose? Are there any other studies that study the sensitivity of the higher modes, in particular, to the gradients?
Until recently higher modes were barely considered. Work was undertaken in the 1970's on higher modes in the context of crustal and mantle structure, but few suitable observations existed. I am not aware of any specific studies of the effect of gradients on higher modes. A mention of work at the South Pole matching multiple modes with a gradient model is now made in the *Discussion*.

Why are higher modes sensitive to gradients than the fundamental mode? Can you add more explanations to this issue?
I have tried to amplify this point.

Extracting and using higher modes to infer shear-wave velocity structure is not as uncommon as the author suggests. There are many studies where higher modes are included in the inversion for the shear wave velocity profile. From my observations, I think it becomes more of a standard than an exception. There is a vast literature that can be found online of case studies where both fundamental modes and higher modes and/or Rayleigh wave ellipticity were used in the inversion.
Line 28 – there are more and more inversion algorithms that do not fix the number of layers. For instance, in this example (https://doi.org/10.1093/gji/ggab116), the inversion problem is formulated in a Bayesian framework, showing the probability distribution of the S and P velocity profile that allows for finding strong gradients
I was aware of an increasing use of higher modes in inversion, but principally in the context of models with a few uniform layers, even when the number is variable. The study was motivated, in part by the question of whether a few layers were adequate to recover structure.
The simplest model used in an effective representation of dispersion on the Greenland icesheet required at least 30 layers to capture variable gradients and was refined from a dispersion inversion using smooth functions of depth.

• *Methodology*
Even though it is stated that you follow the approach of Kennett (2023), I think it would be useful to add a concise paragraph introducing the approach, so the reader has an idea what it is about without the need to read another paper.
More information is provided now in the *Introduction*.

Eq.2 – where this equation come from? Reference?
This equation provides suitable values for density in the near surface zone, based on tabulated properties of common materials.

• *Results*

What about adding some more realistic examples – a real multilayer profile to show how complicated the dispersion characteristics would become?
The H-series models in Figures 5, 6 already represent a realistic example in terms of layering and gradients inspired by a real scenario. Already we have complicated dispersion!

In the simple experiments, in Figure 2 and Figure 4, it seems like different cases can be distinguished just based on the fundamental mode; fundamental modes are quite different, in particular in Figure 2 when considering two main scenarios (upper and lower panels – steep and gentle gradient). Are higher modes helpful here to distinguish between the cases? In addition, in the upper cases, higher modes are not strongly excited – so, in real case scenarios, should the lack of higher modes suggest a steep gradient case? In more complex scenarios, like in Figure 6, indeed, the higher modes help to distinguish between cases where the fundamental mode is similar. But how unique is it? Could a similar pattern be produced by a different velocity profile without gradients?
Additional comments are now made in the *Discussion* about the relative information comment between the fundamental and higher modes.

• *Conclusions*
I think that the conclusions, especially lines 179–189, are quite vague; actually, they are repeating to some extent the motivation from the introduction: higher modes can be useful to better infer velocity models, but it is difficult to extract them, identify, parametrize the inversion, etc.

I think it would be useful to add a short summary of the main findings presented in this manuscript in the conclusions. Such a short summary is also missing in the abstract. Moreover, I would suggest expanding the discussion about practical aspects, for instance, in which case the higher modes are the most sensitive, and when they may be the most beneficial, etc.
The *Discussion and Conclusions* section has been enhanced to address these issues. The abstract has also been expanded.

Lines 114–117, 182–187 – are there any indications of how this approach could be implemented in real-world scenarios involving multilayered velocity profiles? In such cases, how feasible is it to identify a sufficient number of higher modes with adequate quality to distinguish between velocity gradients and strong impedance contrasts? In some of the plots, the difference was the most visible at the 4th or 5th higher mode, which is rarely clear in real cases. For instance, in Figure 9, the difference between different discretisation is visible, but it is clearest for high orders of higher modes. I would argue that it would not be distinguishable in a real case study. What order of higher modes can be inferred in ice sheet studies? Is it more applicable in that field?
A specific mention is now made of recent ice-sheet work using DAS.

Lines 190–194 – I am not sure if it should be in this section; maybe it should be in the introduction, as it sounds like a part of the motivation of this study
Moved to *Introduction*

Line 195–200 – I am not sure what the implications of including horizontal variations would be –would it mask the sensitivity to gradients completely?
The discussion of the effects of lateral heterogeneity is now expanded. Slow variation in surface properties will have almost no effect. Impacts are strongest around osculation points that are associated with large contrast jumps in wavespeed.

• *Figures, editorial Notes*
Figure 10 – typo in the caption
Typo corrected

Line 62 – part of the sentence is missing, possibly the missing part is in line 67
Paragraph reformulated to include more detail about attenuation.

While in Figure 1, this type of legend (grey in legend indicating different shades of colours) is clear, it is not so clear anymore in the next figures with fewer colour shades. I think it would work better if the contrast between extreme cases is higher in those figures.
The caption of the figures now indicates the sequence of models. The contrasts have been enhanced, so hopefully clearer.

In all plots showing dispersion curves, I would clearly mark which is the case with the velocity jump, so it is easier to navigate between cases without going back to the previous plots or the text.
Clearer indications of the nature of the models are provided in the captions

Sometimes dashes in frequency–phase are used with, sometimes without spaces – the correct version is without spaces.
There is now consistent usage