Reviewer 1:

This paper applies three different methods to real seismic data to estimate both Vs and Vp near-surface structures. The wavelength-depth method gives the Vp structure, while all three methods give Vs structures. As a reviewer joined from the revised version, I feel that the qualities of the sentences and figures are sufficient for the publication. Below are some comments that should be addressed before the publication.

Dear anonymous reviewer 1,

We appreciate the time you spent reviewing our manuscript and we think that your remarks were very useful to clarify the scope and the outcomes of our work. Below, we respond point by point to your questions and remarks.

Sincerely,

Authors

Comment R1-1: The title is too broad or general. This title can be used for a variety of studies. There should be more specific words to identify this work.

Thank you for this comment. We change the title to "**Comparison of surface wave techniques to estimate S- and P-wave velocity models from active seismic data**" to provide a clearer explanation of the objectives of the manuscript.

Comment R1-2: The method for estimating Vp should be described more. I guess everyone needs to see Socco and Comina (2017). It is helpful if the data, model parameters (number of layers; yes, I realized that there is no "layers" for the W/D method later, but that was really confusing before realizing it), and outputs are clearly stated in each method.

Thank you for this comment. This study examines the implementation of three methodologies that have been previously documented by different authors. The primary aim of the paper is to demonstrate the capabilities of non-standard methods in seismic surveys for reconstructing the near-surface using data that has an irregular source-receiver layout. The readers are referred to original works for better understanding of the methods. However, as you suggested, we have included additional sentences in the explanation of the W/D method to enhance the flow and understanding of the paper.

Comment R1-3: The disadvantages of W/D method should be described in the introduction section. In my understanding, up to ~10% uncertainty exists for both Vs and Vp estimations.

This uncertainty is significant compared to conventional inversion methods. The W/D method seems like an approximation (transformation) method.

Thank you for this comment. Table 2 in the submitted paper demonstrates that the average difference between the models obtained by the different methods ranges from 3.3% to 7% for S-wave and P-wave velocity models. The near-surface environment is typically characterized by significant heterogeneity in both lateral and vertical directions. Moreover, the inversion of surface-wave data exhibits non-uniqueness, thereby contributing to the differences in the output of the methods. Furthermore, different parameterizations were employed in each of the three models. The W/D, being based on a data transform, provides velocity models with vertical discretization of 1 m intervals, whereas the SWT and LCI give layered models based on the parameterization defined for the inversion. W/D and LCI provided models at the location of the multichannel dispersion curves, while SWT provided the velocity models at the predefined locations of model points. The layer thickness of the models were defined a priori and were fixed in SWT, while LCI algorithm updated also the thicknesses. The models were compared after interpolating them to corresponding spatial voxels. Considering these factors (i.e., heterogeneity of the near-surface, inversion non-uniqueness, different parameterization of the methods and interpolation of the 3D models), average misfit of 3.3% to 7% among the VS and VP from the various methods should not be considered high. We hope this explanation clarifies the extent of the similarities. In the revised manuscript, we have reported this explanation to further clarify the factors that contribute to the differences in the models.

Comment R1-4: The main purpose of this study was not clear and should be clarified in both the abstract and introduction. Why did you compare the three methods? Is it to prove that the W/D method gives Vs structure comparable to conventional methods despite the theoretical uncertainties (and also gives Vp structure in less computational cost)?

Thank you for this question. The objective of the paper are three folds:

1- SWT is a very well-known technique in seismology, but it is rarely used for near-surface high-resolution modelling. The W/D method is a new method that provides the P-wave velocity in addition to S-wave velocity and requires very limited inversion and computational efforts. The LCI is a powerful tool that has not yet been truly exploited for its potential in practical applications. The comparison between the performance of the three methods on a 3D velocity model has never been done to our knowledge and provides an insight in the potential of surface-wave in the estimation of near-surface models.

2- The surface wave methods were rarely used in hard rock sites due to the low signal-to-noise ratio. But we showed that with proper workflow, the three surface wave methods can successfully be applied for reconstruction of the near-surface.

3- Finally, most surface wave methods have been customized for regular 2D acquisition setup where source and receiver locations are inline. In this study, we show that the multi-channel methods (i.e., LCI and W/D) can successfully be applied to data with irregular source-receiver locations which are not common in near surface applications. Also, this irregularity is to the advantage of the SWT data that usually suffers from low and inhomogeneous data coverage originated by in-line source location footprint.

We modified the abstract and introduction of the manuscript to better highlight these motivations for the manuscript.

Comment R1-5: The spatial distribution of Poisson's ratio should be shown in addition to the Vs and Vp structures. It is not clear whether the estimated ratio gives meaningful a priori information to the conventional surface-wave analyses.

Thank you for this comment. Prior research has demonstrated the efficiency of clustering in identifying lateral variations (**Khosro Anjom et al., 2017; Khosro Anjom 2021**), as well as the ability to accurately predict the VP models from VS using Poisson's ratio of the cluster (**Socco and Comina, 2017; Khosro Anjom et al., 2019**). Nevertheless, in the revised manuscript, we include a subplot (Figure 13e) of the 3D Poisson's ratio estimation from W/D method. The uncertainties for the reference layered Poisson's ratio in Figures 13c and 13d were computed from the 3D Poisson's ratio in Figure 13e.

Comment R1-6: Why are the differences in three Vp models larger than that of Vs even though a priori information is common?

Thank you for highlighting this aspect. The a priori Poisson's ratios are utilized in LCI and SWT. Consequently, a nearly identical difference is observed when comparing the VS and VP models from LCI and SWT techniques (4.52% and 4.74%; Table 2 of the manuscript). The slight difference is due to the interpolation of the 3D models into similar spatial voxels and different parameterization during inversion.

In the case of comparison of WD results with either LCI or SWT, the difference between VP models is slightly more (~1%) than VS models. The reason is that in the W/D method the VP models are obtained using the apparent Poisson's ratio. The apparent Poisson's ratios retrieved from the W/D sensitivity analysis are applied to estimate the time-average VP and then the interval VPs are estimated through a regularized DIX-type equation. The fact that this process is data transform creates more fluctuating results compared to layered results of the LCI and W/D. These factors and also the interpolation of the models lead to a slightly higher difference (about 1%) between the VP comparison of the W/D with respect to LCI and SWT. In the discussion of the revised manuscript, we highlight clearly this aspect.

Comment R1-7: Some words were not familiar to me: carpet, "interval" models, and "time-average" models. Maybe because I'm not a native speaker and/or the research field is slightly different.

Carpet recording is a new paradigm of data collection introduced by TotalEnergies. Basically, in this acquisition scheme, in contrast to classical 3D seismic exploration acquisitions, the receivers are placed over a regular grid, and the sources are used only in accessible locations. This approach creates an irregular source-receiver layout that needs to be tested for the surface wave methods applications. In fact, one of the objectives of this study is to show the application of the three surface wave methods to the data acquired in this manner. Since the second reviewer also asked for more details about the "carpet recording" terminology, and for more simplicity, we remove the term “carpet recording” in the revised manuscript. Instead, we use irregular receiver-source acquisition layout.

The concept of interval velocity is a standard concept in seismic reflection processing. According to the definition given in Applied Geophysics Dictionary by Sheriff (2002) the interval velocity is described as: “The velocity of an interval in the subsurface measured by determining the traveltime over a depth interval along some raypath. The interval velocity is often used for velocity calculated by the Dix Formula”. In a locally 1D velocity model the interval velocity is the velocity of a layer between two depth levels within which the velocity is considered constant. In our manuscript we use the term interval velocity to identify the velocity of the 1m intervals used to discretize the W/D models. We instead use the term layered velocity for the velocity of the layers of layered models used in the inversion of SWT and LCI inversion.

The time-averaged velocity at a specific depth refers to the average velocity of either an S-wave or a P-wave from the surface to that particular depth and can be directly utilized to calculate the time required to travel from the surface to that depth along a vertical path. The term "time-average" refers to the fact that the travel time within each layer is used as a weight for arithmetic averaging of the velocity of all the layer down to the depth of interest. An exemplification of the rationale behind the term "time-average velocity" can be demonstrated as follows:

$$VSZ\ (z) = \frac{\sum_n h_i}{\sum_n \frac{h_i}{VS_i}} = \frac{\sum_n VS_i . t_i}{\sum_n t_i}, \qquad \text{(R1)}$$

where, h_i, VS_i, and t_i are the thickness VS and traveltime at the ith layer. We hope this explanation clarifies the time-average velocity concept.

The time-averaged velocity is extensively utilized for various purposes. In seismic reflection, the time-average velocity is employed for computing static corrections. The data is regularly corrected to eliminate the impact of the weathering layer, which can introduce significant noise due to its heterogeneities (Marsden, 1993; Cox, 1999). Within the framework of seismic hazard estimation, the time-average velocity, also known as harmonic velocity in certain literature, serves as a proxy for peak ground acceleration. For example, VS30, which represents the time-average velocity at a depth of 30 meters, is a widely used standard parameter in national and international regulations.

In exploration seismic these terms are common. In order to target a wider range of readers, we included additional explanatory phrases in the introduction to enhance comprehension and differentiation of the layered, interval, and time-average velocities.

Reviewer 2

This paper compares and contrasts three different surface-wave methodologies applied to the same study area and dataset. The description of each method is extensive and most of the required detail is included in the manuscript, however, the link to the geological implications for the study area itself appears to be lacking. What has each result revealed about the study area? Based on the analysis is there a preferred technique for tackling active-source surface-wave imaging in studies such as these? Overall, the manuscript is well-written and informative. Below I detail some mostly minor comments.

Dear Anonymous reviewer 2,

We appreciate the time you dedicated to evaluating our manuscript and we think that your remarks helped improve the manuscript. As previously stated in the initial round of revision, the geological data for the site is confined to the outcrop map presented in the paper. The manuscript aims to demonstrate the efficiency of utilizing three-surface wave methods for analyzing data obtained through non-standard acquisition layouts. None of the proposed methods, even though already presented in publications, are standard in near-surface applications and their comparison on a fairly large dataset and a 3D velocity model was never carried out to our knowledge. In active surface wave analysis, moreover, the P-wave velocity model, which is typically not retrieved, can be obtained using the W/D method due to its sensitivity to Poisson's ratio.

A comprehensive comparison of the methods is provided in the discussion. We prefer not to choose a specific method, but instead, we highlight the benefits and drawbacks that can aid in the selection of a method based on the site, acquisition, and resources. The W/D method is cost effective and also provides VP. The other two methods can provide VP only when a priori Poisson's ratio is provided. On the downside, W/D is a data transform method and the noise in the data directly affect the results. SWT yields high-resolution models when accompanied by extensive data coverage. If the data is restricted to only a few receivers or if there is a limitation on expert resources for two-station picking, the performance of the SWT application can be significantly reduced. The use of LCI is highly effective in generating a laterally consistent model from limited number of dispersion curves. However, when dealing with media with significant lateral heterogeneities, the LCI is more susceptible to excessive smoothing. In the discussion of the revised manuscript, we included these aspects about the performance of the three methods.

In the following, we respond point-by-point to your comments/questions in the following.

Regards,

Authors

Comment R2-1: In the introduction it is not clear if group or phase velocities are used in the surface-wave tomography, though this is made clear further down in the method sections. I would suggest clarifying this in the introduction too, perhaps on Page 2, Line 30.

We appreciate this comment. We clarified in page two of the revised manuscript that we use phase velocity of surface waves.

Comment R2-2: Is it possible to plot sensitivity kernels for surface-waves as a function of depth? E.g., phase velocity sensitivity as a function of period/frequency and depth? This would help indicate what depths the dispersion curves are sensitive to. Have a look at Figure 4c in Darbyshire et al., (2013; EPSL).

Thank you for this comment. We believe the checkerboard test shows the sufficient sensitivity of the data to the investigation depth. Since we prefer to keep the flow of the manuscript, we provide the sensitivity plot you requested here (Figure R1). We performed sensitivity kernel for a random 1D model at last iteration of the SWT, for different periods (frequencies) between 0.25 s to 0.04 s (4 Hz to ~30 Hz). Similarly in Figure R2, we show an image of the sensitivity Kernel between 4 Hz to 30 Hz normalized at each frequency. Confirming the results of the checkerboard test, the sensitivity kernel shows good sensitivity both for shallow and deeper portions. As was also depicted by the checkerboard test, the sensitivity decreases in deeper portions. We prefer not adding these figures to the manuscript because we do not think they will add information with respect to the checkerboard test and they would make the paper lengthy. We leave to the topic editor the decision if these figures are needed.

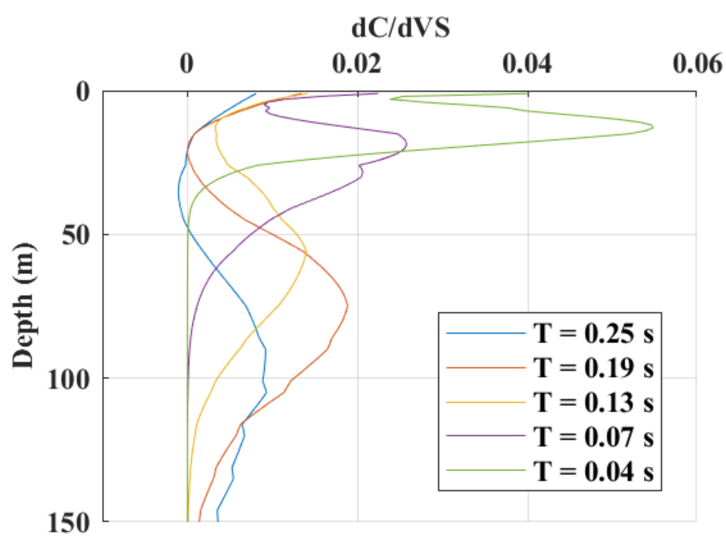

Figure R1: Surface wave sensitivity Kernel for various periods corresponding to frequencies between 4 Hz to 30 Hz.

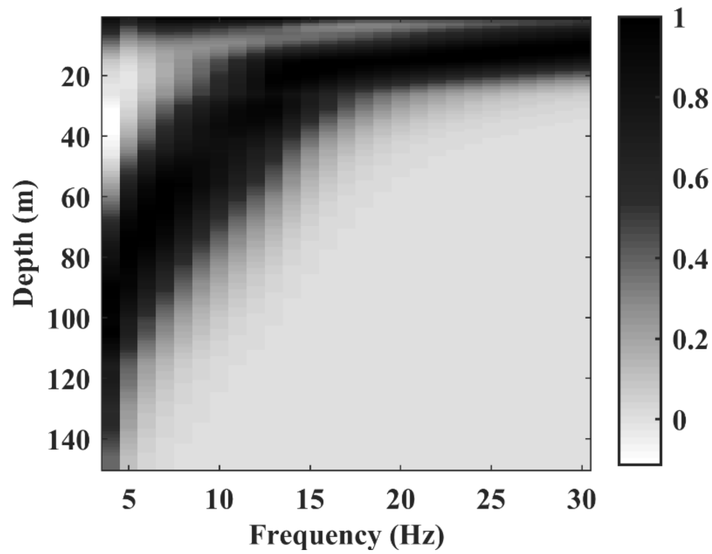

Figure R2: Surface wave sensitivity Kernel for frequencies between 4 Hz to 30 Hz, matching the frequency range of the data. The sensitivities are normalized at each frequency

Comment R2-3: My background is in passive seismology so I'm not quite clear on what a carpet recording method. A sentence or two to explain what this is in more depth would be useful. Perhaps on Page 2, Line 44.

Carpet recording acquisition technique was created by TotalEnergies to enable active data acquisition in remote areas such as foothills or densely vegetated forests. Basically, in this technique, in contrast to classical 3D active data acquisition, the receivers are deployed over dense regular or irregular grid and the sources are limited to accessible locations (Lys et al., 2018). In the case of Aurignac data, the grid of receivers was regular with variable spacing. However, due to the fact that the sources were only used along the roadsides, an irregular layout of receivers and sources was obtained. In fact, one of the objectives of the study is to adapt and test surface-wave methods in situations where the layout of source and receivers are irregular. Since reviewer one also had remarks about the term "carpet recording", we removed it entirely from the manuscript and replaced it with irregular receiver-source layout in the manuscript.

Comment R2-4: I was going to suggest adding latitude/longitude values to Figure 1 but noticed the authors response to reviewer 2's similar comment. Nevertheless, is there a way to better link Figures b and d? Do they correspond to exactly the same area? Perhaps add a box outline in figure 1b to show what region figure 1d corresponds to.

Thank you for the comment. As previously stated, it is not possible to include the coordinates in the figures. However, as you suggested, we included a box that shows the boundaries of the acquisition area over the outcrop map (Figure 1c of the revised manuscript).

Comment R2-5: Page 4, Line 104: “918 receivers”. Might be useful to include some more instrumentation detail here. What kind of receivers?

The information about the receivers including the type of receiver, sampling rate and time window are summarized in Table 1. Five Hz vertical geophones were used, and the data was sampled every 2 ms for a duration of 5 seconds. In addition, 24-ton vibrator source was excited in 1077 locations. Each sweep lasted for 24 s, between 3 to 110 Hz, with 5 s of listening time. All this information is available in the site description and field data set section (above Table 1) of the manuscript.

Comment R2-6: Page 5, Line 126: “The data exhibits a low signal-to-noise ratio as expected for hard rock sites.” I would have thought soft rock sits have a low signal-to-noise ratio, whereas hard rocks allow for high signal-to-noise-ratio because of less attenuation? Perhaps add a citation for this statement since you mention this is ‘expected’.

Rayleigh waves tend to develop more in media with regular vertical velocity gradient than in stiff materials characterized by local heterogeneities that can create scattering and exhibit poor dispersion. Hence, in soft soils, dispersion curves are usually smooth and broad band with clear mode separation. In rock sites they tend to be noisy and narrow banded. The two examples of f-k spectra and relevant dispersion curves in Figures R3 and R4 were produced from a seismic line in a desert area where the acquisition line crosses both sand dunes and fractured rock outcrops. The acquisition was carried out with a heavy vibroseis. The data from the loose sand zone provide very high-quality dispersion curves while the data from the outcrop zone generate very noisy dispersion curves. Similar results have been obtained in several cases of acquisition on hard rock sites where the dispersion curves are poorly dispersive and very noisy. This aspect has been extensively addressed for instance in the PhD thesis of Papadopoulou (2021). We added this reference to the revised manuscript to support our claim.

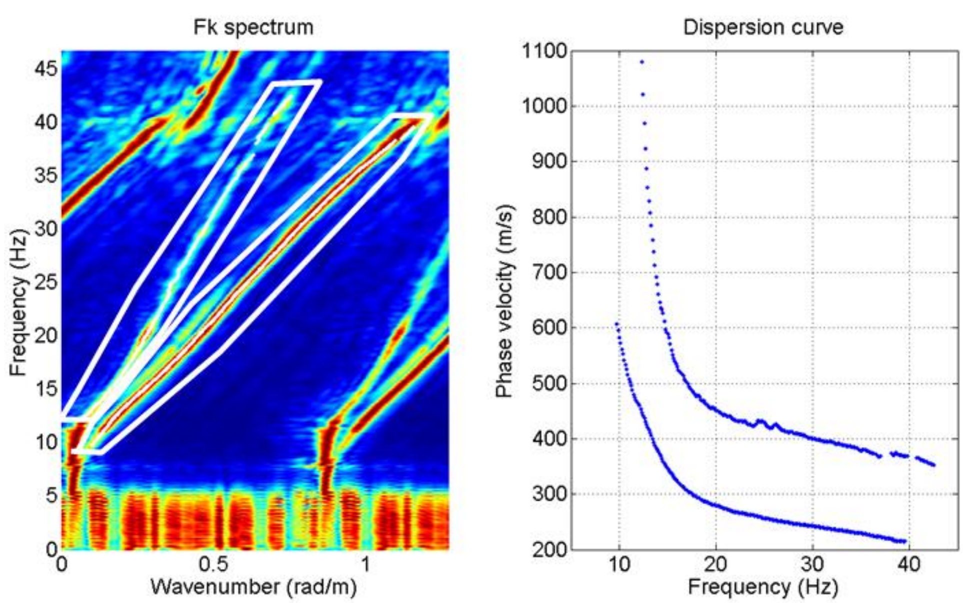

Figure R3: Example recorded surface-waves from soft loose sand (sand dunes): (left) f-k spectrum. (right) multimodal dispersion curves

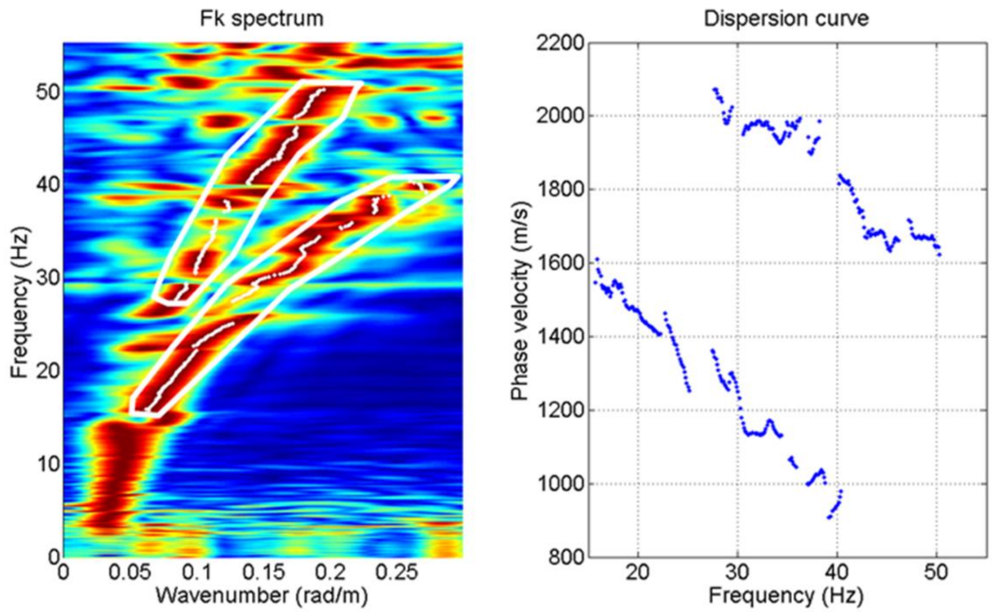

Figure R4: Example recorded surface-waves from the same site of Figure R3 (same acquisition line) but on an outcrop: (left) f-k spectrum. (right) multimodal dispersion curves

Comment R2-7: Page 8, Lines 179-180 and Figure 6b:

It still looks like there is almost double the paths coming from 0-40 degrees and 140-180 degrees than all the other azimuths. Perhaps run a surface-wave tomographic inversion excluding the excess paths in those directions and see if the result is similar. I.e., remove a subset of paths from the most sampled directions and see how that changes the final surface-wave tomography models.

Thank you for this comment. We believe the level of inhomogeneity in the Aurignac dispersion curve data coverage does not have any significant effect on the inversion process, as indicated by the absence of any directional pattern in the checkerboard (Figure 17 of the revised manuscript). We have previously analyzed data that exhibited a strongly inhomogeneous azimuthal illumination. In these instances, both the checkerboard test and the models demonstrate directionality. Figure R5 displays an illustrative example extracted from a previously published study by Khosro Anjom et al. (2021). The limited data coverage between 20 to 140 degrees (Figure R5a) resulted in reduced weights within these directions in the inversion process and led to the creation of a directional model (Figure R5b). Similar directionality can be observed in the checkerboard test on the same data (Figure R6). The tomographic inversion was sensitive to the dominant direction, while model showed poor sensitivity to directions with low data coverage.

The estimated model and checkerboard test (Figure 15 and 17 of the revised manuscript) for the Aurignac data show no indication of directionality caused by nonuniformity of the data coverage. Therefore, in this scenario, we believe the act of decreasing the data for the purpose of achieving more uniform data coverage would not yield any advantages and is likely to diminish the level of detail in the model. Due to the limited time available for revision, we are unable to carry out another inversion to demonstrate this aspect. However, if the topical editor believes this test is crucial, we can carry it out the requested inversion within an appropriate revision timeframe.

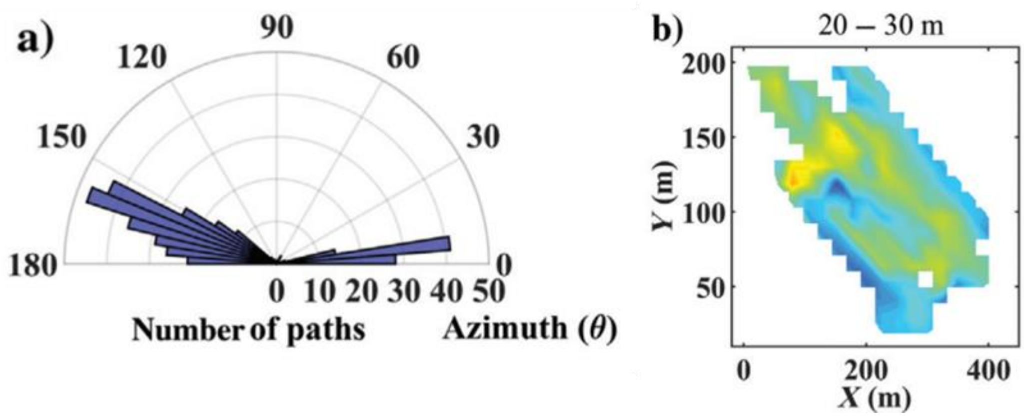

Figure R5: The impact of significantly nonuniform azimuthal illumination on the tomographic inversion (reproduced from Khosro Anjom et al., 2021): (a) Azimuthal illumination. (b) Estimated VS model between 20 m to 30 m of depth.

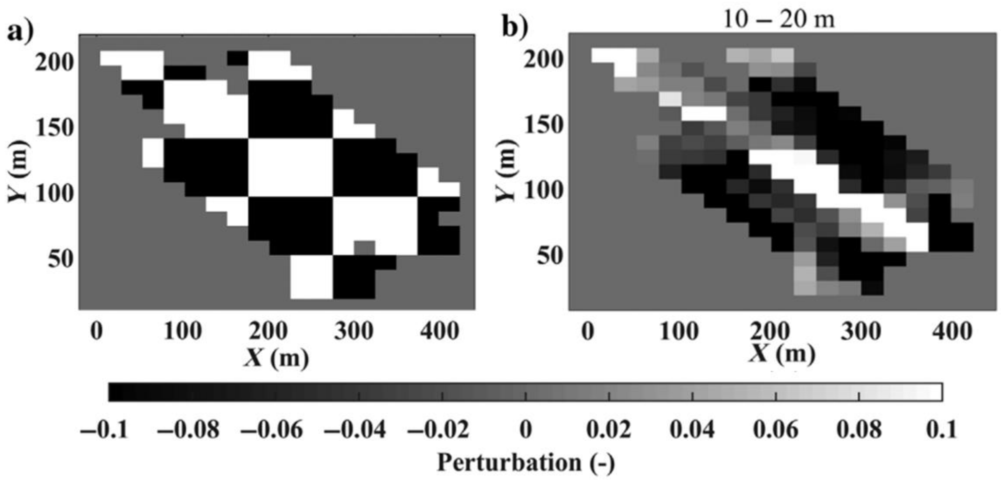

Figure R6: The checkerboard test performed on the data from figure R5 (reproduced from Khosro Anjom et al., 2021): (a) The perturbation pattern. (b) The recovered perturbation after inversion.

Comment R2-8: Page 11, Lines 249-251: Is there a figure for this? Like a trade-off curve showing data misfit versus level of constraint, for example, and the ideal model chosen along the curve?

The approach involves performing multiple constrained and unconstrained inversions in order to obtain a constraint level that provides consistent model, while achieves a data misfit not significantly higher than unconstrained inversion. This approach (Boiero and Socco, 2011) serves as a crucial measure for reducing the excessive smoothing in models. In the case of LCI inversion, the unconstrained inversion yielded an L2 weighted misfit of 23.4, whereas the selected constrained inversion resulted in a misfit of 23.9. This indicates that the constrained inversion does not excessively smooth the model. Similarly, the unconstrained and selected constrained tomographic inversions yielded 42.1 and 43 L2 misfits, respectively. It is noteworthy to mention that the misfits are computed based on equation R1 (please see response to comment R2-9).

We do not employ the technique of cross-cutting between unconstrained and constrained inversion misfits to determine the level of constraint. Instead, we compare the misfit values of the desired constrained model with the unconstrained model to check if it is not excessively smoothened. Therefore, we prefer not to include an additional plot of misfit. Nevertheless, in the updated manuscript, we report the misfit values obtained from both unconstrained and selected constrained inversions.

Comment R2-9: Was regularization used such as damping and smoothing in the inversions? It isn't immediately clear in the method section if this was done.

Thank you for this comment. We incorporate lateral constraints within damp least square inversion scheme (Marquart,1963). According to this inversion scheme that is the same for LCI and SWT, the misfit is computed as:

$$Q = \left[(\mathbf{d_{obs}} - \mathbf{d}(\mathbf{m}))^{\mathbf{T}}\, \mathbf{C_{obs}^{-1}}\, (\mathbf{d_{obs}} - \mathbf{d}(\mathbf{m})\right] + \left[\left(-\mathbf{R_p}\mathbf{m}\right)^{\mathbf{T}} \mathbf{C_{Rp}^{-1}}(-\mathbf{R_p}\mathbf{m})\right], \qquad \text{(R1)}$$

where the first term determines the misfit between the experimental data $\mathbf{d_{obs}}$ and synthetic data $\mathbf{d}(\mathbf{m})$. $\mathbf{m}$ is the vector of the model parameters and $\mathbf{C_{obs}^{-1}}$ is the reciprocal of the covariance matrix. The second term defines the lateral regularization of the velocities and thicknesses, where $\mathbf{R_p}$ is the regularization matrix composed of values 1 and -1 for the constrained parameters and zeros elsewhere. The strength of the regularization is determined by the covariance matrix $\mathbf{C_{Rp}}$.

$$\mathbf{m_{n+1}} = \mathbf{m_n} + \left(\left[\mathbf{G^T C_{obs}^{-1} G} + \mathbf{R_p^T C_{Rp}^{-1} R_p} + \boldsymbol{\lambda}\mathbf{I}\right]^{-1} \times \left[\mathbf{G^T C_{obs}^{-1}}(\mathbf{p_{obs}} - \mathbf{p}(\mathbf{m})) + \mathbf{R_p^T C_{Rp}^{-1}}(-\mathbf{R_p m_n})\right]\right), \qquad (R2)$$

where $\mathbf{G}$ the Jacobian matrix, evaluates the sensitivity of the dispersion curves to the model parameters. $\mathbf{m_n}$ and $\mathbf{m_{n+1}}$ are the previous and updated model vectors, respectively.

We hope this comment clarifies the inversion method. We rather no to include this information in the revised manuscript as the method is published. The details for the LCI inversion can be found in Socco et al. (2009), while Boiero (2009) and Khosro Anjom (2021) provide comprehensive explanation regarding the tomographic inversion formulation. Nevertheless, for both LCI and SWT we clearly mention in a sentence the used inversion approach.

Comment R2-10: Page 17, Line 348: "dense model grid". More description of the model grid is needed. E.g., what is the spacing of the nodes etc. Perhaps including Figure R2 from the response to reviewers in the main text would be beneficial.

Thank you for this comment. The grid density can be observed in the plots of the checkerboard test (Figure 17). To preserve the coherence and continuity of the manuscript, we prefer not to introduce an additional subplot in Figure 15 of the revised manuscript, so as to avoid any unnecessary complexity. Instead, we added a clear sentence in both the caption and explanation of Figure 17 that the circles of the checkerboard test match exactly the dense model grid used for the SWT in Figure 15 of the revised manuscript.

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