Dear topical Editor,

We would like to thank you, and the anonymous reviewer for dedicating your time to our manuscript and providing constructive comments. We carefully considered all the remarks and modified the manuscript accordingly. We also provide a point-to-point response to all the remarks.

In the following we provide detailed responses to the comments from the reviewers. In this document, we refer to previous and current version of the manuscript as submitted and revised manuscript, respectively.

We also have uploaded a marked revision manuscript to facilitate the reviewers and editors' assessment.

Best Regards,

Authors

Response to the anonymous reviewer 1

I am glad to review this manuscript "S- and P-wave velocity model estimation from seismic surface-waves" by Khosro Anjom et al. This work presents the application of three different surface wave methods to active-source surface wave data collected in a mining site, and estimates Vs and Vp, respectively. It's an overall good and informative paper. I have only a few comments on the details.

Dear anonymous reviewer 1,

Thank you very much for your time in reviewing our manuscript and giving useful remarks. In the following we respond to your questions and remarks.

Authors,

Comment R1-1: About the velocity perturbation about 7%

To be frank, i feel like the model perturbation is a bit higher. For example, let's say fig14a, the velocity perturbation of the target structure might be smaller than 15% from my guess. Authors may calculate it and prove me wrong. If it's this case, then the uncertainty from different methods is half of local anomaly of the structure which is unacceptable. I suggest authors provide the average velocity variation at different depths and different methods, and add these information into fig 18 and table 2. It will help the reader to understand the relative scales between the uncertainty associated with methods and the real variation associated with targets.

Thank you for this comment. We should clarify that no marker formation is targeted in the estimated models. The interpolated estimated VS model between 20-35 m of depth from WD and SWT methods are 12% different (Figure 9a and Figure 14a of the submitted paper), whereas the lateral variations of the models compared to the average velocity is on average 10% and 17% for SWT and W/D models, respectively. We understand your comment regarding the model misfit and lateral variations, but we have some concerns about connecting lateral variations from the models directly to the model misfit. The model comparisons were carried under many assumptions, and they were meant to show the similarity of the models and continuity of these similarities in depth. The estimated models had completely different parameterization and the comparison was carried out after the interpolation of the estimated models. The W/D method provided the estimations every 10 cm in depth and at the location of the multi-channel DCs in X and Y directions, whereas the SWT model included 300 locations equally spaced in X and Y directions and within 7 layers of 15 m thickness (only first layer 20 m). So, some level of difference in addition to difference due to methods' accuracies is expected. In Figure R1 below, we compare the results of the VS from SWT method between 20-35 m of depth (second layer) and VS from W/D method averaged between 20-35 m. In the northern part of the area, where SWT model is superimposed to WD model, the trend of the variations is well preserved. The artifacts created by the outliers of the W/D method, which is amplified by interpolation, as well as the smooth lateral transition of the SWT model are the main contributors to the model misfits. Since the SWT model is obtained from laterally constrained inversion the transition between the high-velocity and low-velocity zones is smoother, whereas W/D model provides a sharp transition zone. Using lower lateral constraints helps in sharpening the transition zones but at the cost of creating unrealistic perturbations in the model. In the revised manuscript, we explicitly mentioned the impact of the lateral constraints on the transition zone, as well as the effect of model parameterization within the scheme of the three methods.



Figure R1: Comparing the S-wave velocity results from surface wave tomography and W/D for depth range of 20-35 m. a) The estimated VS from SWT. B) The estimated VS from WD. C) North zone from SWT and south zone from WD.

Comment R1-2: About the data error and model error

I know it's challenging to collect high quality seismic data in area with stiff surface. figure 3 shows that the quality of the collected surface wave data is poor. Is it necessary to use some signal-enhance technical to denoise the data? I am afraid the data quality/error might be introduced into the final model error/uncertainty between different methods. because the SWT method uses only two-station pair which will definitely provide lower quality inputs than other two methods who employ the multi-channel inputs. I would expect some necessary discussions about this part.

Thank you for this comment. We completely agree that seismic data quality at stiff sites can be very challenging. Despite these challenges, surface wave analysis has proven to be a reliable tool for near-surface characterization in these sites. Comprehensive examples of surface wave methods application to hard rock sites can be found in Pileggi et al. (2011), hollis et al. (2018), Papadopolou et al. (2020), Da Col et al. (2020), and Colombero et al. (2022).

In test site of the submitted manuscript, thanks to the high energy vibroseis sources and to spectral stackings from different shot locations, we were able to obtain sufficient spectral resolution for DC picking. As you can see in the spectrum in Figure 4 of the submitted paper many shots are used to compute the final spectrum in Figure 4b, and the energy maxima shows continuity along the selected frequency band.

In the used carpet acquisition scheme, dense receiver grids were deployed, and sources were used only in accessible locations (along roads). This irregular source-receiver outline, as well as the exciting of the source in many locations, gave us a perfect opportunity for two-station dispersion measurements. In classical 3D measurements with regular source receiver locations, non-uniform azimuthal distribution is

usually obtained, and the chance of stacking is reduced for many of the two-station configurations. Thanks to the source-receiver geometry created by carpet recording, we considered a high threshold for the number of stackings (at least five recordings from different shots for each of the two receivers). This way, we were able to obtain good signal-to-noise ratio in the two-station analysis even though the data were from a stiff site. Nevertheless, the spectra of the multi-channel analysis provided a better signal-to-noise ratio in the low frequencies. The distribution of the data points based on wavelength (Figure 15a of the submitted paper) reflects these challenges of the two-station method in estimating low frequencies (large wavelength) data points. In the revised manuscript, we included this information.

Comment R1-3: About the imaging point

I have no idea how the authors define the imaging points for the different methods. i guess the middle point will be taken as the imaging point for SWT, then what's the imaging point of the other two? please clarify this point.

Thank you for this comment. The W/D and LCI methods are based on multi-channel analysis. Within the multi-channel method, the computed dispersion curves are associated to the center of the multi-channel spread. Each DC is assumed to carry information about the local 1D properties beneath the receiver spread. In both W/D and LCI methods the models are estimated at the position of these local DCs and coincide with center of the receiver spread (blue square in figure 4a). Since the receiver spread is a moving squared window that is shifted by 1 receiver spacing in both x and y direction, the relevant DC and 1D models have the same density as the receiver grid.

On the other hand, tomographic inversion takes as input the dispersion curves retrieved from two receivers only. These curves show the path-average phase velocity along the path between the two receivers, and for each receiver couple in line with a source we can retrieve a DC. In this case, the retrieved model does not correspond to a single dispersion curve but belongs to a regular grid of 1D models which are estimated to invert all the retrieved two-station DCs at once. In Figure R2 below we have included for your convenience a map-view graphic scheme. On the left one path between two receivers superimposed to the grid of models, on the right we show that for each location, the model is estimated on the basis of many different paths. In the case of Aurignac data, 300 model points equally distant in x and y directions were used to build the model grid. Within the tomographic inversion algorithm, the synthetic path averaged DCs are computed as reciprocal of the average slowness along the paths discretized over the model grid. The phase velocities at the location of the discretized paths are computed by bi-linear interpolation of the phase velocities from adjacent model points (see the models in red in the plot on the left Figure R2). More information on how the paths is discretized and on how the synthetic DCs are computed for regularly spaced 1D models can be found in Khosro Anjom (2021). In the revised manuscript, we described more clearly the distinction between the model structure and parameterization obtained various methods.

For the comparison of the estimated models from the three methods in the discussion of the submitted manuscript, we interpolated the models to obtain matching geometry of the estimated models in voxels of $10 \times 10 \times 0.1 \text{ m}^3$ in x, y and depth directions.



Figure R2: Schematic representation of surface wave tomography model grid definition and synthetic dispersion curve computation. a) the solid line represents the path between two stations A and B. The phase slowness of any point i along the path is determined from the values at four surrounding grid points using bilinear interpolation. b) the inversion scheme links each 1D VS profile to all experimental raypaths (solid lines) crossing its neighboring area (grey area).

Comment R1-4: why SWT can't provide VS model at the southern zone?

Thank you for this comment. The SWT can definitely provide a model for the southern part similar to the northern part, but we did not apply SWT to both zones due to computational capacity and memory restrictions.

Including the southern part to the surface wave tomography analysis would have meant inverting more than 3000 DCs simultaneously, given that only the north zone, which is the smaller zone, provided 1301 DCs. With more area to cover, we should have considered more model points for the inversion for a high-resolution inversion. The substantial growth in both model size and data volume would have required greater computer memory and computational capacity than what our workstations could provide. We could have under sampled the data and used a lower number of model points, but this would have impacted our study, as we wanted to perform a high-resolution inversion. As a result, we decided to focus the SWT only on the northern zone. We hope this clarifies the absence of SWT in the southern zone. In the revised manuscript, we mentioned the computational constraints in performing simultaneous tomographic inversion of both zones.

Comment R1-5: 5. page 8, line 170, "Fig. 6b and c, as well as Fig. 6b and c, we show the", a typo!

Thank you. In the revised manuscript, we fixed this error.

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Response to the topical Editor

This paper compares three different surface wave measurement and dispersion methods and applies them to real data. The methods include: (1) a data transformation method based on the wavelength/depth (W/D) relationship that was developed and presented by the first author in another paper; (2) a laterally constrained inversion; (3) a traditional dispersion curve analysis based on interstation measurements. I am only familiar with the third method. The advantages of the first one is that it is a lot faster than the other two and it enables to constrain VP. Overall, the paper needs some reorganization. Too many sections and subsections that are too short. Some will need to be expanded (there is very little motivational background about the region where the methods are applied, description of the models, or geological implications). Considering the new method was already presented in a separate paper, this paper should not be yet another technique paper and needs to be more than that. It is fine to compare with other methods (though perhaps it should have been done in the previous paper), but there needs to be more than that here. It would be good to reframe the paper and focus on the geological problem that is tackled and fold the method comparison into it.

Dear topical Editor,

Thank you for the time you spent reviewing our manuscript and for the useful remarks.

This site was selected to perform a research seismic campaign by Gallego Technic Geophysics and TotalEnergies to mainly test the feasibility and efficiency of the carpet recording and also to calibrate the processing tools for this type of survey layout. In a simplified explanation, carpet recording involves the use of dense receiver spread and limited number of shot locations in easily accessible places (Lys et al., 2018). It is noteworthy to mention that the carpet recording is designed for seismic exploration, and the expressions "dense receiver spread" and "limited number of shots" should be interpreted within this context. The carpet recording workflow was created to facilitate seismic measurements in remote areas such as forests and foothills. An example of carpet recording with irregular receiver grid has been also shown in (Khosro Anjom et al., 2019). The use of sources only in accessible locations makes the acquisition extremely efficient but creates irregular source-receiver layout that demands revalidation of the processing workflow. The primary aim of this study was to fine-tune, adapt, and assess the workflows for characterizing near-surface of a hard rock site when seismic data is collected through a carpet recording scheme. The possibility of retrieving reliable near-surface velocity models is important in the view of seismic processing that aims at correcting the deep exploration data for the effect of weathering layer, such as static corrections (Marsden, 1993; Cox, 1999). For this purpose, we applied three surface wave methods and compared their results with each other and with the available outcrop map. We adapted the multi-channel dispersion curve estimation to irregular source-receiver layout from the carpet recording. These dispersion curves were the inputs to WD and LCI methods. On the other hand, the irregular geometry of the source-receivers' were advantages toward the surface wave tomography and resulted in homogenous and dense azimuthal coverage, an important property for high-resolution tomographic inversion. In the revised manuscript, we clarified in detail the motivation behind this research.

The area was chosen as a suitable place to test the carpet recording. To our knowledge, the geological information about the area is limited to the outcrop map in the manuscript that we obtained from French geological survey platform (BRGM). In the revised manuscript, we further analyzed the results and drew a more comprehensive comparison to the geological map.

In the beginning, we created the outline of the manuscript so that the three methods can be easily followed by the reader. We realize that some of the chapters were short, and it would have been better to merge certain sections and add more information. These changes are implemented in the revised manuscript.

In the following, we provide point-to-point response to all your remarks and comments.

Best Regards,

Authors

Comment R2-1: Section 2 is a little short. It would be useful to give some more background about the location. What previous studies have been conducted? What have they found? What are the remaining issues you can solve with your technique?

Thank you for your useful comment. As we mentioned above, this test site was selected to carry out the acquisition of a research experimental seismic survey mainly to test the feasibility and the efficiency of the carpet recording. To our knowledge besides the outcrop map in the submitted manuscript no other information is available and further geophysical measurements are not in reach. In the revised manuscript, we clarified the objectives of this survey and what is the main target of the research.

Comment R2-2: Figs. 1 and 2: Please, add latitude and longitude markers on the figures:

Thank you for this comment. According to our agreement for the use of the data, we cannot disclose the exact latitude and altitude of the location, and of the source-receiver positions. Regarding the model estimations, we also had to scale the UTM coordinates to a new reference system for confidentiality.

Comment R2-3: Section 4.2:

• **Comment R2-3a:** Please, specify if the Monte Carlo inversion is linear or non-linear. If it is linearized, please comment on how this can affect the solution.

We use the 1D Monte Carlo algorithm developed by Socco and Boiero (2008). In this method, the VS, Poisson's ratio, and thicknesses are uniformly sampled within the defined model space, and the best fitting models are selected according to a statistical Fisher test. It is noteworthy to mention that the algorithm developed by Socco and Boiero (2008) takes advantage of scaling properties of dispersion curves. The synthetic DCs of the random models are computed and shifted as close as possible to the experimental DC. Then, the scaling factor is obtained from the DC shift and the models are scaled. These scaling steps, which are performed in a fully automatic manner, highly optimize the model space sampling, and reduce the number of required simulations to find the inversion results. The best fitting models are then selected according to a statistical Fisher test with a certain level of confidence. No linearization is introduced in any step of the MC inversion. Since the MC inversion is just a way to carry out one step of the workflow and the MC algorithm is widely described in the original paper, we decided not to go into the details about it.

- Comment R2-3b: Density is considered a priori. How? Please describe what kind of constraint is imposed. A fixed value? A fixed ratio? Something else? Justify the choice.
 It has been shown in many previous studies that even though density is a model parameter according to Haskell and Thompson's forward modelling of the surface wave dispersion curve, it has a minor impact on the simulated phase velocity (Xia et al., 1999; Foti and Strobbia, 2002; Pan et al., 2018). We assume the density according to the available information of the site. The Aurignac site is characterized by hard rock. We assumed density of 2000 kg/m³ for the first layer and 2200 kg/m³ for the other layers.
- **Comment R2-3c:** Line 145: "wide model space" is a vague statement. Please quantify by giving the bounds of your model space. Have you tried to modify the bounds of the model space and see how it affects the solution?

Thank you for this insightful comment. In the revised manuscript, we provided the upper and lower boundaries of the VS model. The Poisson's ratio of the layers was sampled between 0.1 to 0.45 for all layers, and the density was fixed as mentioned in Comment R2-3b.

Yes, we have tested various boundaries not only for this data set but for other data sets in Khosro Anjom et al. (2019), Khosro Anjom (2021). The algorithm is very efficient given that it scales the sampled models according to the misfit between experimental and synthetic dispersion curves (Socco and Boiero, 2008). Hence, if the model space boundaries are inappropriate the sampling automatically gets out of them. Figure R3 below shows an example of how the models are automatically moved beyond the initial boundaries when the boundaries were selected poorly with respect to the data. Moreover, thanks to the parallel computing of the code, we sampled a large number of models within reasonable time (1,000,000 in this manuscript), which further increases the independence of the final results with respect to defined model space.



Figure R3: A synthetic example showing the impact of model scaling in the final results of the MCI (from Socco and Boiero, 2008). (a) and (b) 5,000 best fitting models from 200,000 sampled models for a three-layer model (magenta): (a) without scaling; (b) application of the scale properties; the homogeneous boundaries (red lines) have been on purpose selected not to contain the true model; (c) and (d) best fitting dispersion curve compared with the experimental one: (c) no scaling; (d) application of the scale properties.

Comment R2-3d: What criteria did you apply to select the models with the Fisher test?

Each synthetic dispersion curve has a misfit with respect to the experimental DC computed as (Socco and Boiero, 2008):

$$S = \frac{\sum_{i=1}^{l} \left[\left(v_{s,i} - v_{e,i} \right)^2 \sigma_{e,i}^2 \right]}{l - (3n - 1)},\tag{R1}$$

where $v_{s,i}$ and $v_{e,i}$ are the elements of synthetic and experimental phase velocities, $\sigma_{e,i}$ are the elements of data uncertainty vector obtained in DC picking stage, l is the number of data points, and n is the number of layers in the inversion. Since the VS, Poisson's ratio and thickness are the variable of the inversion the model parameters are equal to 3n - 1. The Fisher test is applied to these misfits to select the best fitting models, considering a level of confidence. We used a low level of confidence equal to 0.001 to include in the results all geological settings that are providing a good fitting with the



experimental dispersion curves. In Figure R4 below, an example of three different levels of confidence for the inversion of an experimental dispersion curve is shown (from Socco and Boiero 2008).

Figure R4: The selected models from the Monte Carlo inversion of an experimental dispersion curve using different levels of confidence equal to a) 0.25, b) 0.01, and c) 0.001. The figure is taken from Socco and Boiero (2008).

• **Comment R2-3e:** "The experimental W/D relationship is significantly sensitive to Poisson's ratio." Please, demonstrate or provide a reference to justify this statement.

Thank you for this comment. Numerous tests have been performed to show the sensitivity of the W/D relationship to Poisson's ratio (Comprehensive analysis has been previously published in Socco and Comina, 2017; Khosro Anjom, 2021; Khosro Anjom et al., 2019). In Figure R5 below, as an illustration we show the synthetic simulation Khosro Anjom (2021) performed to evaluate the sensitivity of the wavelength-depth relationship to Poisson's ratio. The VS (Figure R5a) and density of the model were kept constant, and only the Poisson's ratio was changed to evaluate Poisson's ratio's impact on the wavelength-depth relationships. Figure R4b shows significant variation of the estimated wavelength-depth relationships. In this plot the color shows the Poisson's ratio corresponding to each wavelength-depth relationship

In Figure 6d and 7d of the submitted manuscript also sketches the sensitivity of the reference data for cluster 1 and 2 with respect to Poisson's ratio. In the revised manuscript, we provided more references to this aspect.



Figure R5: A synthetic study to evaluate the sensitivity of wavelength-depth relationship with respect to variations in Poisson's ratio (from Khosro Anjom, 2021). a) The S-wave velocity of the synthetic model. b) the computed wavelength-depth relationships from different Poisson's ratio.

• Comment R2-3f: It would be very useful to see a plot of sensitivity curves at the measured periods

Thank you for the comment. Within the scope of the W/D data transform no inversion is carried out and the dispersion data are directly transformed to shear wave velocity models. Let us know if we misunderstood your remark or you intended this comment about the sensitivity matrices of LCI or SWT.

• Comment R2-3g: In Figs. 6 and 7, VSZ was not defined

Thank you for this comment. The VSZ is the time-average VS. In the revised manuscript we replaced it with time-average VS that is used in the manuscript. We better clarified that time-average VS profiles are computed from the layered velocities.

 Comment R2-3h: Fig. 8 is based on one model only. It would be useful to plot and discuss the uncertainties in each Vs and Vp model and how they propagate into Poisson's ratio uncertainties. Without good error estimates, differences between clusters are not meaningful.

Thank you for this comment. Figure R6a below shows the computed Poisson's ratio cube from estimated VS and VP models of the W/D method. Separately for each cluster, we used these estimated Poisson's to obtain a standard deviation at each depth. In Figure R6b and R6c, we added these standard deviations to the estimated Poisson's ratio for each cluster as you suggested. In the revised manuscript, we added these errorbars to the estimated Poisson's ratios of the Figure 8c and 8d of the submitted manuscript. The estimated Poisson's ratios for the two clusters are used as prior information in the LCI and SWT methods, and for the conversion of the estimated VS models from these methods to VP.



Figure R6: Uncertainty estimation for the reference Poisson's ratios from W/D method. a) 3D view of the obtained Poisson's ratio from W/D method. b and c) Estimated Poisson's ratio from the reference estimated VS and VP model of the W/D method for cluster 1 and 2; The standard deviations were obtained from all estimated Poisson's ratio of each cluster in Figure R5a.

Comment R2-4: Section 4.3 is very short and may need to be expanded or folded into another section.

We had intentionally separated the three-methods application and had kept them short to facilitate the comparison by the reader. In the revised manuscript, we merged the methods together in a single chapter. Thank you for the comment.

Comment R2-5: Section 5.2

• Comment R2-5a: VP and density are fixed a priori: how? Details are needed

As we mentioned in response to comment R2-3b, the density has minor impact on the inversion results. It is a standard approach in surface wave analysis for near-surface characterization to use fixed density based on information of the site. As we mentioned in response to comment R2-3b, we considered density of 2000 kg/m³ for the first layer and 2200 kg/m³ for the rest of the layers.

On the other hand, the VP is not fixed. As we mentioned in response to comment R2-3h, we estimated two reference Poisson's ratio for the two clusters (zones) using the W/D method (Figure R5b and R5c above). We used one of the two estimations of Poisson's ratio for each model point in scheme of LCI and tomographic inversion, based on its location. We clarified these aspects in the revised manuscript.

• **Comment R2-5b:** Line 221: What does "contemporarily" mean in this context? I suspect it is incorrect English usage.

Maybe the word simultaneously was a more proper word as we meant that we invert the dispersion curves all together, opposed to one-by-one 1D inversions. In each iteration a single sensitivity matrix is computed and used to update all model parameters. In the revised manuscript, we rephrased the sentence to eliminate the confusion.

• Comment R2-5c: What are the lateral constraints applied?

We used the same level of lateral constraints equal to 50 m/s in both LCI and SWT methods. Both algorithms are designed to allow lateral and vertical constraints, as well as constraints by a priori information. We only used lateral constraints in these inversions. Both inversions are in scheme of damped least-square deterministic scheme to minimize the misfit. The misfit Q is defined as:

$$Q = \left[(\mathbf{d}_{obs} - \mathbf{d}(\mathbf{m}))^{\mathrm{T}} \mathbf{C}_{obs}^{-1} (\mathbf{d}_{obs} - \mathbf{d}(\mathbf{m})] + \left[\left(-\mathbf{R}_{p} \mathbf{m} \right)^{\mathrm{T}} \mathbf{C}_{\mathbf{R}p}^{-1} (-\mathbf{R}_{p} \mathbf{m}) \right],$$
(R2)

where the first term determines the misfit between the experimental data d_{obs} and synthetic data d(m). **m** is the vector of the model parameters and C_{obs}^{-1} is the reciprocal of the covariance matrix. The second term defines the lateral regularization of the velocities, where 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 C_{Rp} .

Comment R2-6: Section 5.3: Please expand and describe your models

Thank you for this comment. The estimated VS model from LCI, similar to W/D estimated VS model shows a significant change in the trend of the velocity from east to west in the first 87.5 m shown in Figure 11, which is in-line with the geological information from the site (the outcrop map in Figure 19. The high velocity in the east is due to the marl and limestones formations of the Sparnecian and lower Thanetian, whereas the shallow part of the western area is covered by loose material. In the deeper portion of the model below 87.5 m the contrast almost disappears and higher velocity up to 1500 m/s is reached,

probably by reaching Danian formation that is known to have higher velocity than Sparnecian and lower Thanetian. In the revised manuscript, we merged the three methods application sections to facilitate the description of the models, while avoiding repetitions.

Comment R2-7: Section 6.3: The results need to be described. A paper section should have more than one sentence.

Thank you for this comment. Similar to the estimated models from LCI and W/D methods (comment R2-6), the model from the SWT method depicted significant lateral variation between the east and west within the shallow layers. Nevertheless, the SWT model provides a smoother transition zone between east and west, which is mainly caused by the lateral constraints of the inversion. The level of constraint is a compromise between the sharpness of lateral variations and unrealistic perturbations and inconsistency in the estimated model. A weaker laterally constrained inversion will definitely enhance the sharpness of the transition zone but create unrealistic perturbations to the model (Boiero and Socco, 2010). It is noteworthy to mention that even though the constraints of the LCI and the SWT applications are similar, the SWT provides a smoother model, mainly due to the differences in the covariance matrix of the dispersion data and to the difference in the forward modelling of the synthetic dispersion curves.

Like the other two methods, the estimated VS model from SWT shows no contrast between west and east below 95 m where it reaches the Danian formation. In the revised manuscript, we provided more description of the estimated models.

Comment R2-8: Section 7:

• **Comment R2-8a:** Again, model comparison would be better with error bars on the individual models.

Thank you for this comment. For two reasons we believe the boxplots are a more reasonable choice to sketch the misfit compared to errorbars between the models from the three methods. First, they provide detailed comparison within different depth and between each two methods. Also, in each location and each depth, we have only three results that are from the three methods. As a result, obtaining an errorbar (standard deviation) for each model point at each depth may be statistically invalid. We prefer to maintain the differences between the models within the scope of the boxplots. If the topical editor believes otherwise provided these explanations, we prepared the revisions accordingly.

• **Comment R2-8b:** Fig. 18: the caption should explain the difference between blue and red symbols.

In the submitted manuscript, we had provided the explanation of the standard boxplot in the text of the discussion above the figures. We understand that figure caption is a better location to explain the boxplots. Basically, the box plot is defined by three lines showing the 25th percentile, median and 75th percentile of the residual's distribution, and whisker lines extending from the box's edges up to 1.5

times the distance between the edges of the box. The rest of the data are considered as outliers and are shown with "+". We moved this explanation accordingly in the revised manuscript.

• **Comment R2-8c:** A discussion of the geological implications of the results is needed. The short paragraph at the end of the section needs to be expanded.

Thank you for this comment. All three methods depict a contrast between the limestone rich area in the west and loose material in the east. These observations are corroborated by the geological information as was mentioned in comment R2-6 and R2-7. Given the site is an open-cast limestone mining site, the estimated models can offer valuable insights for strategizing the expansion and excavation of the quarry in the investigated zone to support the nearby cement production facility. In the revised manuscript, we discussed these implications.

Comment R2-9: Section 8: I think saying a method is "a great breakthrough" may be an overstatement. It would be better to use the word "advantageous" or something like that instead.

Thank you for your comment. By "As a result, the automation of the DC picking can be considered a **great breakthrough in industrialization** of these methods, which enables their fast applications to even larger data sets than the one used in this paper.", we meant a robust automatic picking algorithm in the future can give a significant boost to the industrialization of the surface wave algorithms. To eliminate any confusion, we rephrased the sentence as:

"As a result, the automation of DC picking can be viewed as an important milestone in the industrialization of the surface wave methods, facilitating their swift application to data sets even larger than the one used in this study."

Comment R2-10: Minor comments

Comment R2-10a: Overall, a lot of indices, exponents, and subscripts need to be fixed (e.g. VS, VP, density units, etc).

Thank you for this comment. We fixed these in the revised manuscript.

- Comment R2-10b: There are too many subsections that are very short and probably should be combined into bigger sections or be expanded significantly.
 We reorganized the sections and removed many of the subsections in the revised manuscript.
- **Comment R2-10c:** Line 38: reference to Socco et al. (2017) needs reformatting Thank you. We fixed these in the revised manuscript.
- Comment R2-10d: I am not familiar with the type of study this is applied to. Why do you calculate a time-average velocity? This needs a bit of context. Do you mean Vs measured at different times? Why would Vs depend on time?
 Thank you for this comment. The time-average velocity at a given depth is the average velocity of the wave (either S- or P-wave) from the surface down to that depth. The time average velocity can be used directly to obtain the traveltime of a wave from the surface down to the specific depth. The term time-average comes from the fact that to obtain an average velocity at a depth, the

traveltime within each layer should be used as the weight to the arithmetic averaging. An illustration of why it is called time-average velocity can be shown as:

$$VSZ(z) = \frac{\sum_{n} h_i}{\sum_{n} \frac{h_i}{VS_i}} = \frac{\sum_{n} VS_i \cdot t_i}{\sum_{n} t_i},$$
(R3)

where, h_i , VS_i , and t_i are the thickness VS and traveltime at the *i*th layer. We hope this explanation clarifies the time-average velocity concept. In the revised manuscript, we explained more clearly the concept of time-average velocity.

The time average velocity is directly used for many purposes. For instance, in the context of seismic reflection the time average velocity is used for static corrections. These corrections are routinely applied to remove the effect of the weathering layer from the data, whose heterogeneities may represent a significant source of noise (Marsden, 1993; Cox, 1999). In the context of seismic hazard estimation, the time average velocity, also called harmonic velocity by some authors, is a proxy for local seismic response. For instance, VS₃₀, the time average velocity at 30 m depth, is a standard parameter, used in many national and international regulations, to classify sites according to seismic response classes, thanks to the correlation between VS₃₀ and PGA (peak goring acceleration). In the W/D method, to retrieve the W/D relationship, the layered model is transformed into its corresponding time-average model (equation R3) and compared with the DC plotted as a function of wavelength.

- Comment R2-10e: Line 39: Rephrase "showed with synthetic and real tests" to "showed with synthetic tests and tests on real data" Thank you for this comment. We implemented the change in the revised manuscript.
- **Comment R2-10f:** Line 50: mantel should be mantle Thank you for this comment. We implemented the change in the revised manuscript.
- Comment R2-10g: Line 53: application of SWT for the near-surface characterization → application of SWT for near-surface characterization Thank you for this comment. We implemented the change in the revised manuscript.
- Comment R2-10h: Line 54: In literature → In the literature
 Thank you for this comment. We implemented the change in the revised manuscript.
- Comment R2-10i: Line 60:
 - VS should have been defined much earlier in the text
 - o "the" S-wave velocity model
 - Remove "the recordings of". It is the receivers that are aligned with the event. Also, please specify that the alignment is approximately along the great-circle path and thus implies ray theory I applied.

Thank you for this comment. We implemented the change in the revised manuscript.

• **Comment R2-10j:** Line 66: "on two-station ones." à "on two-station methods." Thank you for this comment. We implemented the change in the revised manuscript.

- **Comment R2-10k:** Line 71: Incorrect English wording for "that is in advantage of SWT." Thank you for this comment. We implemented the change in the revised manuscript.
- **Comment R2-10I:** Line 8: "the" south of France Thank you for this comment. We implemented the change in the revised manuscript.
- **Comment R2-10m:** Fig. 7b: vertical axis label needs to be moved Thank you for this comment. We implemented the change in the revised manuscript.
- Comment R2-10n: Line 288: analyse → analyze
 Thank you for this comment. We implemented the change in the revised manuscript.

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