EGUsphere, author comment AC3
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Reply on RC4

Mohammadkarim Karimpour et al.

Author comment on "Comparison of straight-ray and curved-ray surface wave tomography approaches in near-surface studies" by Mohammadkarim Karimpour et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-279-AC3, 2022

Thank you for your suggestions. We have added more details, particularly in the Method Section, to allow the readers to fully understand the applied methodology and increase the reproducibility of the work.

We have copied your comments below and assigned a number to each of them. We have provided a response for every comment.

Comment#1: Abstract: This is too general. It does not display any highlights on the results presented in the paper

Response#1: As also suggested by Reviewer#2, we have modified the Abstract and better highlighted the key results of the study.

Comment#2: Introduction: In general, two different strategies exist to "convert" surface-wave dispersion curves to a 3-D Vs model. As explained in the manuscript, the first (i) involves the calculation of phase-velocity maps, which are then converted to Vs by carrying out many 1-D inversions. The second, instead, (ii) allows one to invert the dispersion curves directly for the 3-D model. In both cases, the data kernels can be calculated (a) by assuming that the waves travel along the great-circle path connecting a given station pair (ray-theory) or (b) by accounting for ray bending (ray-tracing).

In this study, you carry out a comparison between strategies (a) and (b). Is there a reason why you don't contemplate strategies (i) and (ii)? This would hugely benefit both the paper and the seismological community. Moreover, from the introduction, it is not clear which strategy between (i) and (ii) you intend to focus on, and why.

Response#2: As you mentioned, this study focuses on the comparison between strategies (a) and (b). The comparison between strategies (i) and (ii) is definitely very interesting and can be a topic for a separate study because it is out of the scope of this work. We have reconstructed 3D VS models by direct inversion of DCs. We have clarified it in lines 79-80 of the revised version of the manuscript as:

"For each dataset, 3D VS models from straight- and curved-ray SWT are obtained by direct inversion of DCs, …"

Comment#3: I feel that the motivation for the study should be discussed more in-depth, because the pros and cons of ray-theory vs. ray tracing are well known (for reviews, see Rawlinson & Sambridge 2003, Rawlinson et al. 2010).

Response#3: We have re-written some paragraphs of the Introduction for clarification. The main purpose of this study is to evaluate the performance of straight-ray and curved-ray SWT at the near-surface scale. As stated in lines 52-53 of the revised version of the manuscript, despite seismological studies, such comparison is missing at the near-surface scale. We have also compared our results with previous seismological studies in Section 4.5 of the revised version of the manuscript, and showed that the previous findings in seismology might not be valid at the near-surface scale. Moreover, we have explained the applied criteria to optimize the shot positions for a SWT study using active seismic data at the near-surface scale. This is a key difference between the available seismological studies and near-surface studies using active seismic data. We show the importance of the shot optimization in the obtained dispersion curves coverage and consequently the obtained VS models from the inversions in the Results Section.

Comment#4: General Consideration: This study focuses on very shallow crustal structures; I wonder if the iterative nonlinear-inversion scheme used to convert the dispersion curves to the 3-D Vs model can be considered appropriate in this sense. Wouldn't a globally optimized algorithm be more suited to the solution of a (possibly highly) nonlinear problem such as that of this kind? Adding some consideration on the matter would be useful.

Response#4: We had stated in lines 59-61 of the original version that:

"The computational efficiency is of great importance in seismic near-surface since, compared to seismological studies, the abundance of data at active seismic near-surface projects can increase the computational cost significantly."

The computational cost is a key factor for professionals particularly in the near-surface studies with high amount of data. We had shown in Table 3 of the original version, the cost of the SWT using the employed deterministic approach which can be as much of 80 $. This number can increase drastically by using stochastic methods.

Moreover, we have shown in Table 4 of the revised version of the manuscript that the final VS model obtained from the iterative non-linear inversion can be quite accurate even though the inversions started from homogeneous initial models.

Comment#5: Method: The method, and the related assumptions, should be explained more clearly (see also the points below). In principle, since this is supposed to be a technical work, I believe this part should be highly detailed, so as to make your work reproducible. I suggest that you expand largely this section, and possibly subdivide it into several subsections. It would be good to explain (i) the calculation of dispersion curves, (ii) the ray-theory vs. the ray-tracing algorithm (with the latter one meriting more consideration, especially if you implemented it on your own), (iii) the forward solver that allows you to measure predicted data from a given Vs model, and (iv) the inversion for Vs. In (iv), it would be good to say something about the calculation of sensitivity kernels at different periods. Other points are found below.

Response#5: Following this comment, we have largely expanded the Method Section in the revised version of the manuscript and added way more details and equations. We have also divided this section into:

2.1 Optimisation of source layout

2.2 Estimation of DCs

2.3 1D forward modelling

2.4 Computation of forward response

2.5 Inversion algorithm

We have supported each subsection by related explanations and Equations.

Comment#6: - One can only guess that the inversion strategy chosen by the authors involves the direct inversion of surface-wave velocity for the 3-D structure.

Response#6: We have clarified it in lines 79-80 of the revised version of the manuscript:

"For each dataset, 3D VS models from straight- and curved-ray SWT are obtained by direct inversion of DCs, …"

Comment#7: - Synthetic tests to verify the accuracy of the ray-tracing algorithm should be presented. What is the relative error as a function of distance from the source based on a *homogenous* medium?

Response#7: To test the accuracy of the ray tracing algorithm, we used a homogeneous medium (the Blocky model with constant VS equal to 200 m/s in the whole medium) and computed the ray paths. As an example, we show the computed paths for a DC where the receivers are located at (19 m, 5 m) and (10 m, 5 m) in the figure below:

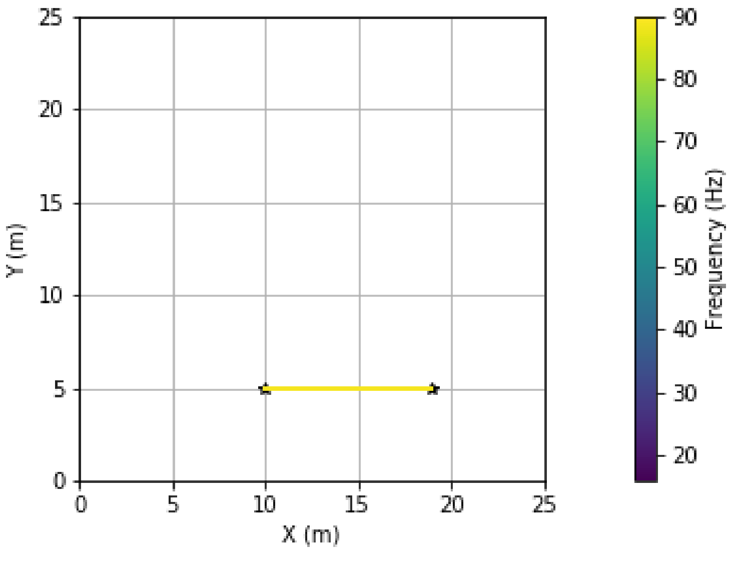

Figure 1. The computed ray paths for different frequency component of the DC with receivers' positions at (19 m, 5 m) and (10 m, 5 m).

The average relative error of the paths (i.e., deviation from straight-line) is almost zero (7.2e-16). We have not shown this figure in the revised manuscript. Nonetheless, we have added the following clarification in lines 129-130 of the revised version of the manuscript:

"To evaluate the accuracy of the ray-tracing algorithm, we have applied it in a to a homogeneous media and noticed that the error (i.e., deviation from straight-line) in this condition is almost zero (not shown here)."

Comment#8: - When you refer to "straight lines", are you referring to great-circle paths?

If not, it should be explained that you designed your experiment in a cartesian coordinate system.

Response#8: We have added the following clarification sentence in line 118 of the revised version of the manuscript:

"We carry out our experiments in a Cartesian coordinate system."

Comment#9: - Each vector and matrix in equations (2) and (3) should be thoroughly explained, and their dimensions be explicit. For example, is d_obs your slowness, or is it the arrival time obtained from slowness and ray-path distance? How do you calculate the roughness operator Rp? Generally, the extent of the roughness is determined by a damping scalar coefficient (e.g., Boschi & Dziewonski 1999, Magrini et al. 2022), but you have the matrix C_{R_{p}}. Can you please be more explicit on its calculation? Can you also provide a reference for your equation (3), or alternatively a derivation for it? As it is, it appears different from, e.g., eqs. (51) and (57) of Rawlinson & Sambridge (2003).

Response#9: We have added the reference for Eq. (3) (equivalent of Eq. 11 in the revised version of the manuscript), which is Boiero (2009).

*We have extended the Method Section largely and added way more details in this section. We explained in line 112 of the revised version of the manuscript that the dimension of the vector of the experimental data (**d_obs**) is (phase) velocity. We have also clarified in lines 160-162 of the revised version of the manuscript that we have assigned a large value (10^6) to the covariance matrix of the spatial regularization matrix:*

"To reduce the impact of spatial regularization on the inversion results, in all four examples in this study, a large value (10^6) is assigned to C_{Rp}. It means that the VS difference between the neighbouring cells is constrained to 1000 m/s."

Comment#10: - Is your stopping criterion compatible with previous studies?

Response#10: In some surface waves studies, the inversion stops when the misfit function reduces less than 1% with respect to the value at the previous iteration (e.g., Garofalo et al., 2015). Since our computational facilities have been improved compared to before, we have defined a lower threshold in our study (0.01%) to make sure that the inversion reaches a local minima. As can be seen in Fig. 6b of the revised version of the manuscript, the misfit value of straight-ray inversion shows a sudden decrease at iteration 27, while the inversion would have stopped at iteration 26 if a higher threshold had been chosen as the stopping criterion.

Comment#11: Results: An important point that does not seem to be discussed is the choice of damping in the two different inversions. Slightly different values of damping can produce slightly different results. I believe it would be important to discuss in some depth this choice, and to demonstrate, to some degree, that the result of your comparisons is not biased by improper use of regularization. (Note that a given value of roughness damping might be ideal for the ray-theory case but not for the ray-tracing case, and vice versa).

Response#11: We have clarified the choice of regularization 160-162 of the revised version of the manuscript. As pointed out by Trampert and Spetzler (2006), the choice of regularization has a major impact on SWT results. Therefore, we have assigned a very large number (10^6) to the regularization values so that the final VS model is not biased by the regularization values and the comparison between straight-ray and curved-ray methods are fair.

Comment#12: - For example, consider Fig 10: I have the feeling that the large differences between the two inversions might derive from the choice of the roughness damping (the straight-ray tomography seems slightly underdamped). Have the authors experimented with different values?

Response#12: We have provided a detailed response in the response to the comments of Reviewer#1. The observed difference is due to the 'wrong' ray paths in the straight-ray approach. At the edges of the velocity anomaly, the assumed paths by the straight-ray are shorter than the true paths and therefore the velocities are (wrongly) high.

All experiments have been done with the same regularization values. As stated in lines 160-162 of the revised version of the manuscript.

Comment#13: - Are your synthetic data generated with the mentioned 3-D finite-difference code only in the Case study 1 or also in the Case study 2? Eventually, a brief explanation of this code could go in the Method section.

Response#13: The same 3D finite difference code has been used to generate the synthetic data in both case studies 1 and 2. We have added the following sentence in line 238-239 of the revised version of the manuscript for clarification:

"The same finite difference code used for the Blocky model was used to obtain the Sand Bar synthetic dataset …"

We have added a brief explanation of this code (SOFI3D) in lines 197-201 *of the revised version of the manuscript as:*

"*The code is an FD modelling program based on the FD approach described by Virieux (1986) and Levander (1988) with some extensions. It can consider viscoelastic wave propagation effects such as attenuation and dispersion, employ higher order FD operators, 200 apply perfectly matched layer (PML) boundary conditions at the edges of the model, and it works in message passing interface (MPI) parallel environment which reduces the running time of the simulations*"

Comment#14: - I am struggling to understand the meaning of the red arrows/letters in Fig. 8, and the caption is not helping me. Probably I am missing something simple, but this suggests to me that the explanation in the caption should be extended or made clearer.

Response#14: Since Reviewer#1 had the same struggle, we have removed the red arrows from the reconstructed VS models from the inversion (Figures 4, 7, 10, and 13 of the revised version of the manuscript).

Comment#15: Discussion: The relative misfit in equation (6) is a function of **fw(m_final)**. May you please be more explicit on the forward calculation of your dispersion curves (predicted data) based on a given model? Did you use SOFI3D?

Response#15: No, we have not used SOFI3D for this purpose. Following your previous comments on the Method Section, the process for the forward calculation of the dispersion curves have been thoroughly explained in Section 2.4 of the revised version of the manuscript.

Comment#16:- In the introduction, you refer to Boschi and Dziewonski (1999) while speaking of seismic ambient noise. Clearly, in 1999 ambient-noise tomography did not exist

Response#16: We have put it its right place, that is at line 24 of the revised version of the manuscript.