



EGUsphere, author comment AC2
<https://doi.org/10.5194/egusphere-2022-279-AC2>, 2022
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Reply on RC2

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-AC2>, 2022

Thank you for your comments and suggestions. We have replied to all your comments below. In the following, we have copied your comments and assigned a number to each of them (for clarification) and provided the explanations for every comment.

Remarks

Comment#1: In the introduction, a few other studies are mentioned that also apply straight and/or curved-ray tomography, but it is not clear for most of these studies whether they performed a comparison of the two approaches. There should be an overview of what the conclusions from other authors on the topic were. I am also missing a (short) discussion at the end of the manuscript on whether the results agree with existing literature.

Response#1: We have modified the Introduction and added the following sentences to clarify this issue and provide an overview of what the conclusion from other authors on the topic were in the lines 37-53 of the revised version of the manuscript as:

"SWT has been used in seismological studies for decades and different SWT approaches have been compared by seismologists. For instance, Laske (1995) studied deviations from straight line in the propagation of long-period surface waves and concluded that they usually have small effects on the propagation phase. Spetzler et al. (2001) applied both straight-ray and curved-ray SWT methods. They computed the maximum deviations of ray paths from straight lines and pointed out that this maximum is typically below the estimated resolution, except for long paths at short periods. Some studies showed that a more complex forward modelling in SWT did not improve the results (Sieminski et al., 2004; Levshin et al., 2005) while other studies reported obtaining better results (Ritzwoller et al., 2002; Yoshizawa and Kennett, 2004; Zhou et al., 2005). Trampert and Spetzler (2006) pointed out that the choice of regularization has a major impact on SWT results. They studied SWT methods based on ray theory (straight-ray and curved-ray) and scattering theory in which the integral along the ray path is replaced by the integral over an influence zone. They showed that these methods are statistically alike and any model from one method can be obtained by the other one by changing the value of the regularization. They concluded that the only option to increase the resolution of the model is to increase and homogenize the data coverage. Bozdag and Trampert (2008) compared straight-ray and curved-ray SWT methods in their study and mentioned that performing ray tracing could be so time-consuming that the potential gain in crustal corrections on a

global scale might not be worth the additional computational effort imposed by ray tracing. Despite seismological studies, a comparison between the performance of straight-ray and curved-ray SWT at the near-surface scale is missing."

We have also added Section 4.5 to compare the results of our study with previous studies.

Comment#2: The abstract needs to be rewritten. It gives a very brief introduction and motivation for the study. But an abstract should summarize the key results of the study. The same applies to the conclusions section which should be rewritten.

One of your key results is that in a scenario with low data coverage, the curved-ray approach performs significantly better. But your synthetic examples do not prove that since you run no example with low coverage. I would suggest that you take one of your two synthetic tests and test whether this conclusion holds.

Response#2: We have rewritten the Abstract and Conclusions Sections. We have considered to better highlight the motivation and the key results of the study in the revised version. We have added several lines (14-19 of the revised version of the manuscript) at the end of the Abstract to highlight the key results as:

"In three examples we optimise the shot positions to obtain acquisition layout which can produce high coverage of dispersion curves. In the other example, the data have been acquired using a typical seismic exploration 3D acquisition scheme. We show that if the source positions are optimised, the straight-ray can produce S-wave velocity models similar to the curved-ray SWT but with lower computational cost. Otherwise, the improvement of inversion results from curved-ray SWT can be significant."

We have rewritten the whole Conclusions Section as well.

Regarding the data coverage, we have made several changes to explain this issue more clearly. As we have stated in line 14-18 of the revised version of the manuscript, for three examples in this study (the Blocky model, Sand Bar mode, and Pijnacker example) we have optimized the shot positions to obtain high data coverage. We have devoted Section 2.1 to describe the employed algorithm to optimize the source positions. For case of the CNR example, the acquisition layout mimics at a smaller scale the classical seismic exploration 3D cross-spread acquisition scheme with orthogonal lines of sources and receivers. This dataset, not being optimised will help analysing the criticalities introduced by a non-optimal acquisition scheme. We believe that the subject of the data coverage has been more clearly explained in the revised version and therefore, we did not repeat a test with one of the synthetic examples with lower data coverage (because the problem is not only about having a low data coverage. It's about the low data coverage that may be obtained from a typical exploration acquisition scheme)

Comment#3: l. 10 "exact paths" - the term 'exact' is quite vague. In this case you calculate Eikonal paths since they are the solution to the Eikonal equation. These represent an approximation and not necessarily the true/exact paths.

Response#3: We have removed the term "exact" and modified the sentence in lines 9-10 to:

"Alternatively, curved-ray SWT can be employed by computing the paths between the receiver pairs using a ray-tracing algorithm."

Comment#4: l. 35 So did Trampert and Spetzler find a difference between the ray-based and the finite-frequency approach?

Response#4: We have added the following explanation to show the highlights of the study by Trampert and Spetzler (2006) in lines 44-49 of the revised version of the manuscript:

"Trampert and Spetzler (2006) pointed out that the choice of regularization has a major impact on SWT results. They studied SWT methods based on ray theory (straight-ray and curved-ray) and scattering theory in which the integral along the ray path is replaced by the integral over an influence zone. They showed that these methods are statistically alike and any model from one method can be obtained by the other one by changing the value of the regularization. They concluded that the only option to increase the resolution of the model is to increase and homogenize the data coverage."

Comment#5: I. 38 It sounds like Gouédard did not perform any comparison between straight and Eikonal ray based models. So why cite them here?

Response#5: Following this suggestion, that work (Gouédard et al., 2010) has been deleted from the Introduction in the revised version of the manuscript.

Comment#6: I. 50-56 You present several studies where some applied some form of ray tracing and others didn't. But what is your point? Did these studies find any advantage in ray-tracing? If your point is that researchers have applied different methods to approximate the rays but no-one has done a systematic study, then you should write it like that.

Response#6: We have re-written the Introduction and devoted a paragraph to refer to the previous seismological studies where these two approaches were compared. We have clarified in lines 52-53 of the revised version of the manuscript that such comparison is missing at the near-surface scale. This is one of the key points of this study to compare the two methods at the near-surface scale.

Comment#7: I. 70 "shots are defined as a regular grid" sounds wrong to me. I would rather write something like "shot locations located on a regular grid are tested by calculating the number of aligned receivers for each location."

Response#7: Following this comment, we have removed "shots are defined as a regular grid". In the revised version of the manuscript, the Method Section has been expanded largely as suggested by Reviewer#3. We have dedicated Section 2.1 to explain the employed procedure to pick the shot positions.

Comment#8: I. 71 How many shots are picked, what are the criteria for the number of picked shot locations?

Response#8: As mentioned in the response to the previous comment, we have explained the criteria to pick the shot positions in Section 2.1 of the revised version of the manuscript.

Comment#9: I. 71 I assume this approach only applies to receiver layouts on a grid and not in case of irregular/random receiver locations? Maybe you should say so.

Response#9: It can be applied also to irregular/ random receiver locations. We have used the guidelines proposed by Da Col et al. (2020). In that study, the receivers are not put as a regular grid.

We have clarified it in Section 2.1, lines 87-88 of the revised version of the manuscript:

"For a given (random or regular) array configuration, we can optimise the locations of shots to ensure having high coverage DCs with minimum number of shots based on the

guidelines by Da Col et al. (2020)."

Comment#10: l. 75 Which values for Vp and rho are you assuming in your study? How are these values chosen?

Response#10: As requested by Reviewer#1, we have changed Vp to Poisson ratio for the sake of consistency. For the synthetic examples, we have used the true values of Poisson ratio and densities, as mentioned in lines 216-217 and 247-248 of the revised version of the manuscript. For the Pijnacker example, we have added the following clarification in lines 279-280 of the revised version of the manuscript as:

"Since the medium was (almost) saturated, a high v value (0.45) was chosen for the initial model. The p values in the medium were assumed to be low (1700 kg m^{-3}) because it consisted of unconsolidated materials."

In case of the CNR example, the following clarification has been added in lines 309-310 of the revised version of the manuscript:

"... v is approximated based on a previous study (Khosro Anjom et al., 2019) on the site and fixed at 0.33, and density is fixed at 2000 kg m^{-3} since the site mainly consists of loose sand material."

Comment#11: l. 76-80 I think the way you describe the procedure is a bit complicated. Basically, you take your 3D model defined by Vs, Vp and rho and extract 1D depth profiles at each point of the model. You then calculate the phase dispersion curve for each profile and join all the dispersion curves to get 2D phase-slowness maps at a set of periods (at how many periods, how do you choose the periods?). The ray tracing is then done in each of the phase-slowness maps separately. I would suggest to rewrite this paragraph.

Response#11: We have deleted this paragraph from the manuscript. Instead, we have expanded the Method Section as suggested by Reviewer#3. We have provided more details and clarifications in the Method Section of the revised version of the manuscript.

Comment#12: l. 99, 102 "uneven sampling", "non-uniform sampling"; it would be helpful to your readers if you could say more precisely what you mean. If I understand correctly, it is that the number of samples is not the same at different wavelengths, i.e. periods?

Response#12: We did not mean that the number of samples is not the same at different wavelength. We estimated the DCs in the frequency domain and each DC is sampled uniformly in frequency (Δ_f is constant). This means that for each DC, the adjacent sampled points have the same " Δ_f " but not the same wavelength difference. In fact, the portion of the DC with lower wavelength are more sampled than the portion with higher wavelength.

We have modified this part of the manuscript and added the following explanation to clarify this subject in lines 172-175 of the revised version of the manuscript:

"To estimate the DCs from raw data, we have used the auto-picking code (Papadopoulou, 2021) in which the DCs are sampled uniformly in frequency. This means that each DC is non-uniformly sampled in terms of wavelength which can drive the inversion algorithms to the shallowest part of the subsurface without any significant updates in the deeper portion of the initial velocity model (Khosro Anjom and Socco, 2019)."

Comment#13: You should also mention how you sample your dispersion curves. From the images, it looks like you have a uniform sampling in frequency. This means that you implicitly put a higher weight on high frequencies (a uniform sampling in period would

imply a higher weight on low frequencies). Many researchers therefore apply a log-spaced sampling.

Response#13: We have mentioned in lines 172-173 of the revised version of the manuscript that we sample each DC uniformly in frequency:

"... we have used the auto-picking code (Papadopoulou, 2021) in which the DCs are sampled uniformly in frequency."

We have added explanations regarding the employed processing tool (the auto-picking code described in Papadopoulou, 2021) and devoted a subsection (2.2) to explain the applied methodology to estimate the DCs from the raw data. As this code samples each DC uniformly in frequency, we impose the wavelength-based weights to increase the weights of the points with lower wavelength.

Comment#14: l. 105 "sigma_{i,j} is the standard deviation of the ith data point of the jth dispersion curve". I think there is a mistake in that description. In your matrix, only the trace is non-zero. Your sentence would then imply that for each dispersion curve, you only have a single measurement. Instead, I think that you have several measurements (at a set of periods) for each dispersion curve, so that the measurements from disp curve 1 have standard deviations sigma_{1,1} to sigma_{n,n}, and from disp curve 2 from sigma_{n+1,n+1} to sigma_{n+m,n+m}, and so on...

Response#14: In fact, we agree that the Eq. 4 in the original version (equivalent of Eq. (12) in the revised version of the manuscript) could be confusing. To clarify, we have modified the previous equation to:

Comment#15: What is meant by "closest data point"? Please explain. Also, why the delta in delta lambda_{j,max} if it is the maximum wavelength? (from the delta I would expect a difference)

Response#15: The weights in Eq. (13) in the revised version of the manuscript, are computed separately for each DC. For each point of the DC, a wavelength value can be computed based on its phase velocity and frequency. For the generic ith point of the jth DC, the "closest data point" is defined as the point from which the ith point has the smallest wavelength distance. delta lambda_{j,max} represents the maximum computed wavelength difference for the jth DC. We have clarified these terms in lines 182-183 of the revised version of the manuscript as:

"where delta lambda_{i,j} represents the wavelength difference between the data point i of the jth DC and the data point with the smallest wavelength distance from i, and delta lambda_{j,max} is the maximum computed wavelength difference for the jth DC."

Comment#16: l. 110 Did you add any error to your synthetic measurements? Please mention in the text.

Response#16: We have not added any error to the synthetic data. We have clarified it in lines 197 and 239 of the revised version of the manuscript.

Comment#17: l. 150 It would be good to give a more quantitative measure for the quality of reconstruction, for example by providing the variance reduction or simply the misfit to the input model. (I just saw that you did in table 3, I would suggest that you write down these values here or refer to table 3).

Response#17: We have added the reference to Table 3 (equivalent of Table 4 in the revised version of the manuscript).

Comment#18: l. 199 Are the values of nu and rho in your Table 2 fixed during the inversion? Please mention somewhere in the text. What influence do you expect from the potential errors in these values?

Response#18: Yes, the values of nu and rho are fixed during the inversion. We have explained it in lines 187-191 of the revised version of the manuscript:

"It should be noted that only VS values are updated during the inversion and the other parameters (h , v , and p) are fixed. In case of the synthetic examples, the true values of v and p are used in the inversion. For the field examples, v and p are approximated based on the available a priori information. Having erroneous values of v and p can induce errors in the inversion results even though the sensitivity of surface waves to VS is more than v (and way more than p)."

Comment#19: l. 214 What value for the data standard deviation (sigma, eq 4) do you assume in your synthetic tests and in the real data examples? Is sigma individually determined for each measurement?

Response#19: We have added Eq. (2) from which the standard deviation of phase velocities are computed in lines 114-115 of the revised version of the manuscript:

The proposed equation by Passeri (2019) is used to approximate the standard deviation (σ_{V_j}) of generic j^{th} element of the phase velocity vector (V_j) at its corresponding frequency (f_j) as ...

Comment#20: l 324 You weighting is based on the wavelength of the signals, but at the same time you argue with the lower number of data at long wavelengths. So should the weight not rather be based on the number of measurements at each frequency? Or, putting the question differently, if I have a dataset with exactly the same number of measurements at each frequency (as is probably the case in your synthetic experiment), do I still need the weighting?

Response#20: The weighting is different for each DC. The weighting is not based on the number of measurements at each frequency but rather based on the wavelength of each point of a DC. So, even in case of synthetic example, each DC is non-uniformly sampled in terms of wavelength and the wavelength-based weighting can be applied to compensate this non-uniformity. We believe this concept has been better explained in the lines 172-176 of the revised version of the manuscript as:

"To estimate the DCs from raw data, we have used the auto-picking code (Papadopoulou, 2021) in which the DCs are sampled uniformly in frequency. This means that each DC is non-uniformly sampled in terms of wavelength which can drive the inversion algorithms to the shallowest part of the subsurface without any significant updates in the deeper portion of the initial velocity model (Khosro Anjom and Socco, 2019). To address this issue, a wavelength-based weighting scheme was applied in the inversion process to compensate for this non-uniformity (see Khosro Anjom et al., 2021, for details)."

Comment#21: I would like to refer again to my previous comment on the importance of the sampling of your dispersion curves. If you use a uniform sampling in frequency, it is clear to me that the low frequency measurements are underweighted. Maybe run a test with log sampling and compare to the results in Fig. 14.

Response#21: As we have explained in the Section 2.2 of the revised version of the manuscript, we have used a processing tool uniformly samples the DCs in frequency. We believe that with the provided clarifying explanations on the procedure of weighting in the revised version of the manuscript, the process is described much clearer than the original

version. We did not run a test with log sampling because as explained earlier, our processing tool samples DCs uniformly in frequency.

Comment#22: Fig. 7 There should be a scale on (b). Why is the panel in (d) cropped? It seems that some dispersion curves go also to values slower than 50 m/s.

Response#22: We have added the scale for the Pijnacker's acquisition scheme (Fig. 8b of the revised version of the manuscript). We have modified Figure 8b and decreased the lower limit of the y-axis to avoid confusion.