Reviewer#1: Prof. Comina

I've read with pleasure the thesis “Individual and joint inversion of surface wave tomography for near-surface applications” by Mohammadkarim Karimpour. The thesis is quite well written and very clear in scopes and objectives and contains interesting and valuable data and elaborations both on synthetic and real seismic data, with interesting results obtained. Also, introduction and literature references on the topic, particularly on Surface Wave Tomography are appropriate and exhaustive and set well the field within which the thesis is developed.
My judgement with respect to the need for minor revision is mainly related to the part of the thesis where data elaboration strategies and inversion softwares are presented, in particularly:

- 1-It is commented in the text that “We showed that the shot positions can be optimised and the coverage of the DCs can be checked before carrying out the simulation or field data acquisition”, but indeed the part on the optimization of the DC coverage is not particularly covered in the text nore this is shown anywhere in the text… It is not clear if this part of the work was developed within the thesis or if the thesis was mainly focused on the elaboration of already available datasets. One of the two should be better focused, by presenting more in depth the optimization procedure, if this was part of the work, or by less stressing this aspect within the thesis if this is not;

- 2-The joint inversion algorithm should be better presented, possibly by providing some numerical examples and conceptual figures already in chapter 3.2, since it is not completely clear the link between the SWT and BWT in joint inversion through Poisson’s ratio. It is not completely clear if the VP information is used in the SWT forward and updated through the iterations or not, it is not completely clear the role of VS in BWT, that should be more commented even if minor or absent. It seems that Poisson’s ratio, as used, is more a constraint than a proper link in the inversion. This uncertainty arises also from some unclear comments with respect to Poisson’s ratio in the introduction: “The distribution of Poisson’s ratio … can be considered a proxy for the liquefaction risk in liquefaction prone areas”, I do not get this link, or “VP and VS models from the joint inversion might be better than individual inversions”, better in which sense? Usually the fitting of individual inversions is better instead.
- 3-It is commented at the end of chapter 3 that “We also illustrated the differences between the forward operators of straight-ray and curved-ray approaches in 3D”, this is not exhaustively discussed within the chapter and could be better focused since it is indeed an interesting part of the work. Only later in the text some example results are provided and commented and also there the emerged differences could be more stressed. Comments on that: Information of ray paths is used somewhere in the inversion as a potential information or constraint? What happens to model cells not covered by rays in curved ray inversion, they are not updated?
- 4-Additional non mandatory comments that could be discussed, eventual as further developments, related to the misfit function adopted: I’ve noticed significative differences among the global misfit adopted in the inversion and the local misfit of the DCs… could this information be used somewhere? Or the misfit changed to provide better/different results

Dear Prof. Comina, thank you for your time to review my thesis and making valuable suggestions. I have considered them to improve the quality of my thesis. Here are listed the modifications of my thesis.

1) I have used the guidelines suggested by Varangoulis (2014) and the developed codes explained in Da Col et al. (2020) for the optimisation of the survey design. I have clarified it in page 10 of the thesis as:

"In a 3D SWT study, the acquisition layout should be designed carefully to make sure that high coverage of data can be extracted from the recordings. Varangoulis (2014) proposed a procedure to optimise the source positions for acquisition setups with regular grids of receivers. Da Col et al. (2020) optimised positions of (irregular) receivers for a set of pre-defined source positions. In our 3D examples, we use the former method in which the azimuthal coverage and the number of in-line receiver pairs with a source are used to optimise the source positions for a regular grid of receivers. In the following, we briefly explain the employed procedure to optimise the shot positions.

It should be noted that to optimise the theoretical DC coverage, we assume that a DC can be estimate between every receiver pair aligned with a source, and the ray paths between every receiver pair are straight lines. The actual coverage of DCs is obtained after processing the raw data (explained in Section 2.2) and it depends on the quality of the data, the frequency of the retrieved DCs, and the velocity distribution of the medium, which can perturb the ray paths from the straight lines".

Following your suggestion, I have less stressed about the optimisation of shot positions and replaced the following comment "We showed that the shot positions can be optimised and the coverage of the DCs can be checked before carrying out the simulation or field data acquisition" by "We explained the employed procedure to optimise the source positions in a SWT study".

2) The VP information from BWT is used in the forward modelling of SWT and it is updated in each iteration. I have added a paragraph and a table (Table 3.2) to list the common parameters, the parameters that are updated during the joint inversion, and the employed physical constraint during the joint inversion process in pages 48-49 of the revised version of the thesis. The added paragraph is:

"Here, we employ the latter method and use the obtained VP model from BWT at each iteration in the forward modelling of SW. We propose a SWT and BWT joint inversion algorithm in which VP and *h* are the common parameters between the two methods. Moreover, we suggest using Poisson's ratio (ν) as the physical constraint in the joint inversion of SWT and BWT. It should be noted that even though the VS model from SWT does not affect the forward modelling of BWT, it is used to compute the ν model at every iteration. In our 3D individual and joint inversions, we consider the *h* values to be constant during the inversion to reduce the computational cost the inversion. However, in 2D the values of *h* are considered as unknowns and update during the inversion process. In Table 3.2, we list the joint inversion parameters and specify which parameters are updated during the joint inversion, which parameters are common between the forward modelling of SWT and BWT, and which parameters act as the inversion constraint".

It is true that individual inversion might sometimes have better fittings, but the model from joint inversion is physically more meaningful and could be more accurate than individual inversions (as discussed in Chapter 5). For clarification, I have replaced the sentence "… VP and VS models from the joint inversion might be better than individual inversions" in the Introduction Chapter to "VP and VS models from the joint inversion might be more accurate than individual inversions".

As you mentioned correctly, the Poisson's ratio has been applied as a physical constraint not as the link, because it is not an unknown of the joint inversion process. To clarify this subject, in the Conclusions Section of Chapter 3, I have replaced the sentence "we illustrated the applied mechanism to **link** the VS model from SWT and the VP model from BWT through a physical constraint (Poisson's ratio)" by "we illustrated the applied mechanism to constrain the VS model from SWT and the VP model from BWT through Poisson's ratio". I have also deleted the word "link" in pages 6, 7, 37, 48, and 58 for further clarification.

3) Following this suggestion, I have discussed in more detail about the differences between the forward operators of straight-ray and curved-ray approaches in Chapter 3. I have modified the paragraph below Equation 3.9 in page 43 as:

"The difference between the straight- and curved-ray SWT inversion approaches is in the computation of inter-station ray paths. In the straight-ray SWT, for every receiver pair, it is assumed that the ray paths for all frequencies are straight lines. On the other hand, in the curved-ray SWT, the path between each receiver pair should be computed for every frequency component of the DC".

Similarly, I have expanded the paragraph after Equation 3.10 in pages 45-46 as:

"Since the ray path ($l_{R_1R_2}$) in Equation 3.10 would be different for the straight-ray and curved-ray approaches (unless the computed ray path in the curved-ray approach is also a straight line), the computed phase slowness from Equation 3.10 would be different in case of the straigh-ray and curved-ray SWT approaches".

The information on the ray paths are not used as constraints in the inversion because the ray paths are computed based on the distribution of the obtained VS values. So, they do not provide independent information.

The velocity of the model cells that are not covered by ray paths are not updated if also the computed sensitivity (Equation 3.16) to that parameter is zero. However, it can happen that some cells are not covered by ray paths but there are some rays cross in their vicinity so that the velocity of those model cells are used in Equation 3.10. In that case, the sensitivity would not be zero and the velocity of those cells are updated.

4) Thank you for your suggestion. Using the difference between global misfit and local misfit of DCs in the inversion would be an interesting topic for further investigation.

I have added a new section in Chapter 3 (Section 3.3) where I have explained the different types of employed misfits in the thesis for further clarification.

Reviewer#2: Prof. Vignoli

The thesis "Individual and joint inversion of surface wave tomography for near-surface applications" is, already in the present version, quite well organized and clear. It discusses the differences in using straight or curved ray paths in surface wave tomography. In addition, the last part of the research is devoted to investigating the possibility of a mutually constrained inversion of Body Wave Tomography and Surface Wave Tomography.

As general remarks,

1) I would suggest the candidate go through the thesis trying to correct some of the most evident (and inevitable) typos (e.g.: already in the Abstract, most likely, "SW are dispersive, meaning that in a vertically homogeneous medium" should be "SW are dispersive, meaning that in a horizontally homogeneous medium"; on page 3, "demonstrated the reliability of the noise cross-corelation" should be "demonstrated the reliability of the noise cross-correlation"; on page 10, "The input data in this thesis are fist-arrival " is probably meant to be "The input data in this thesis are first-arrival", and so on).

2) The author often mentions that the acquisition design has been optimized across the different tests, but, unfortunately, I could not find how this optimization has been performed. Increasing the value of the retrieved information based on the acquisition set-up is clearly a very interesting topic (worth of a PhD thesis by itself). So, maybe, the candidate should be more explicit about what has been actually done in preparation for the thesis in this respect.

3) Mr. Karimpour admits that the adopted spatially constrained inversion scheme was made practically ineffective by choosing the spatial constraint parameter extremely loose. Clearly, this is consistent with the necessity of focusing on the effects of curved/straight ray paths and on the effects of the joint inversion. On the other hand, the quite limited number of model parameters already plays a (crucial) regularization effect.
Of course, at this stage, I do not expect the author to run tests on the impact of spatial regularization (for example concerning smooth spatially constrained regularization, but, also, sharp/sparse spatially constrained regularization) on the results (by definition, if properly applied, the above-mentioned regularizations should enhance - in one way or another - the spatial coherence of the results, improving the overall quality of the final results), however, I am pretty sure that adding a few lines about this would be beneficial to the readers

4) If I am not mistaken, the joint inversion consists "merely" of a check on the consistency of the value of the Poisson's ratio. Probably, some more direct lines on the absence of an explicit regularization term coupling the two original individual problems should be included in the text. And, maybe, the possibility of incorporating such an explicit term should be investigated in future works by Mr. Karimpour.

I believe the thesis deals with very interesting topics and solutions, which might significantly impact the geophysical community.
Hopefully, these comments from my side may contribute to improving its already overall good quality further.

Best regards,
Giulio Vignol

Dear Prof. Giulio Vignoli,

Thank you for reviewing my thesis. I believe that your comments have helped me a lot to improve the quality of my thesis. My responses to your comments, and the list of the modifications can be found in the following.

1) Thank you for your suggestion. I have corrected the typos.

2) Following this suggestion, and also the first comment of Prof. Comina, I have modified the text of the thesis and clarified that I have used the guidelines by Varangoulis (2014) and Da Col et al. (2020). So, as suggested by Prof. Comina, I have modified the text and have less stressed about the shot optimisation process. I have explained the applied modification to the text the in the response to that comment.

3) I agree with your suggestion. As I have mentioned in the first paragraph of Section 6.2 (Suggestions for future works), exploring other methods for spatial regularization would be an interesting topic for further research. I have added a new figure (Figure 4.6) in Chapter 4, and have explained that applying stronger regularization constraint (using the 'standard' regularization method) could lower the quality of the inversion results. I have added the following two paragraphs before and after Figure 4.6:

"As mentioned earlier, we impose very weak spatial regularization in our SWT inversions by setting the values of $\boldsymbol{C_R}$ equal to 10^6. To illustrate the impact of stronger regularization constraint on the inversion process, we perform another straight-ray SWT inversion where the VS difference between adjacent cells is set to 50 m/s (equivalent of setting $\boldsymbol{C_R}$ equal to 2500). The obtained VS model is compared with the true model and the VS model with weak regularization in Figure 4.6."

"We see in Figure 4.6 that using a weak regularization constraint (Figure 4.6d-f) has retrieved the blocks of velocity anomaly and the area around them (shown as red arrows) more accurately than using a stronger constraint (Figure 4.6g-i). Therefore, in all the following examples, we carry out the inversion using a weak regularization".

4) From this comment and the second comment of Prof. Comina, I have understood that the description of the joint inversion algorithm had not been very clear. So, as I also discussed in the response to the second comment of Prof. Comina, I have added Table 3.2 and a paragraph in pages 48-49 of the revised version of the thesis to clarify the joint inversion algorithm.