

Interactive comment on “Resolution analysis of joint inversion of seismic receiver function and surface wave dispersion curves in the “13 BB Star” experiment” by Kajetan Chrapkiewicz et al.

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Received and published: 5 December 2017

The manuscript is about the joint inversion of Rayleigh wave phase velocity dispersion and P receiver function applied to 13 broadband stations that have been recording for 3 years at the south-western margin of the East European Craton. The manuscript wants to give some new input on the application of the linearized inversion, and to give constraints on the mantle structure of the study area. In my opinion this paper is too synthetic;

1. This work is meant to be a counterbalance to the prevailing number of papers which skip over the more thorough technical description of the inversion. The joint inversion

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of RF and SWD is still relatively young and not yet well established method and has some inherent challenges to address which this paper tries to highlight. Furthermore, “synthetic” papers have proved to be valuable (e.g. Ammon et al. 1990), even with no field-data example (e.g. Morgan et al. 2013).

fundamental sections, like results and discussions are superficially written, while they need a longer, accurate and also descriptive argumentation, in order to demonstrate the quality and meaning of the results.

2. Please see the relevant sections in the revised manuscript. In Sect. 5 we present all the results with emphasis on the crustal model which wasn't the main goal of this study. We also describe all the inversion parameters that we used. Mantle structure (target) in turn is discussed in detail in Sect. 6.

In its actual shape the paper is poor and raw, and misses the accurate descriptions needed to deserve publication. In the following I am listing specific problems of the manuscript. 1) The results of the RF inversion only are better (although not yet fully convincing, see next point) than the results of the joint inversion of RF and SWD (Figure 11). Therefore it is not clear why the authors spend their time applying the joint inversion when inversion the RF only could give better results. If the authors want to proof that the joint inversion gives better constraints for unraveling the subsurface structure, then they have to convince the reader by adding examples (and explaining them), and with some argumentation that is lacking at the moment.

3. The data misfit is not the best measure of the result quality in this case. For joint inversion it will always be worse than for a single-data type since the former gives a trade-off between the misfit of both data types in exchange for mitigating their non-uniqueness. Please compare subfigures of Fig. 10 in the original manuscript to see how ambiguity of results of RF-based modeling (b) is reduced by including SWD data (c). This is one of the advantages of the joint inversion which has been shown in numerous other studies (see Sect. 1 for references).

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2) The chosen crustal model (and “frozen”) for the inversion is clearly not correct for the area. The fit between observed and synthetic RF for the initial 5 s is poor. If the paper wants to address the issue of “exploiting a priori knowledge” (as stated in the abstract), then the authors should show what happens in the inversion if the shallow part of the model is free and not “frozen”, and show how their results are improved.

4. To address this problem we incorporated an intermediate step (“first-stage inversion”) to adjust the crust before inverting for a deeper model (please see Subsect. “Exploitation of a priori knowledge” and Sect. “Results” in the revised manuscript). In that step we used prior knowledge about crustal structure from previous studies. The results obtained without a priori information (using homogeneous starting models, no “freezing”) are attached as figures: suppl-fig08 (results), suppl-fig9-10 (misfit), suppl-fig11 (starting models). They are clearly less credible due to trapping by local minima close to the incorrect starting models.

3) In the same way in the text it is not explained how the joint inversion improves (or not improves) the results of the SWD inversion only.

5. Please see Introduction of the revised manuscript (“Compared to inversion of each of these data types alone, it provides better vertical resolution than SWD, and, unlike RF, constrains absolute shear velocities (e.g. Shen et al., 2013).”).

4) The description of the results is almost lacking, it actually consists in listing the number of figures that show the results, such figures are not well described as well, and their meaning and their importance is never mentioned as well.

6. Please see rewritten section in the revised manuscript and point 2 of this response.

5) The discussion section is extremely short and it does not add anything new to previous knowledge, probably because the paper has nothing new to add to the state of the art of both the technique and structural features of the area.

7. Please see rewritten section in the revised manuscript and point 2 of this response.

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As far as we know, there were no other similar studies in this area before, so the results on the mantle structure are undoubtedly a value added to the previous knowledge.

The following “promises” made in the abstract: “Several fundamental issues inherent in the linearised inversion were addressed in this work, including exploitation of a priori knowledge, choice of model’s depth, trapping by local minima associated with non-uniqueness of the misfit- function optimization problem, proper weighting of data sets characterized by different uncertainties, and credibility of the final models” must be explained and discussed in this section.

8. Please see rewritten Sect. Discussion in the revised manuscript.

6) Figure4: the several RF stacks plotted on top of each other are hardly comprehensible. Each stack must be plotted singularly, for seek of clarity.

9. Corrected. Please see Fig. 4a of the revised manuscript.

7) Acronyms such as ASWMS, CPS, FWI must be explicated somewhere in the text

10. Corrected.

8) Figures 10, 11, and 12 deserve a complete caption; the colors in these figures are not explained at all

11. Corrected. Please see Fig. 12-15 of the revised manuscript.

Technical corrections:â”Page 1 line 4: linearised → linearizedâ”Page 2, line 9: covering THE entireâ”Page 2 lines 26-28: the sentence is badly written, and should be rewritten Page 8 Line 3: cover → covers

12. Corrected.

13. References

Ammon et al. 1990, On the Nonuniqueness of Receiver Function Inversions, Journal of Geophysical Research

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Morgan et al. 2013, Next-generation seismic experiments: Wide-angle, multi-azimuth, three-dimensional, full-waveform inversion, Geophysical Journal International

Please also note the supplement to this comment:

<https://www.solid-earth-discuss.net/se-2017-58/se-2017-58-AC2-supplement.zip>

Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2017-58>, 2017.

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