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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.

Anonymous Referee #2

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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; 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. 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.

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.

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.

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.

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. 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.

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.

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

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

Technical corrections:

Page 1 line 4: linearised → linearized

Page 2, line 9: covering THE entire

Page 2 lines 26-28: the sentence is badly written, and should be rewritten

Page 8 Line 3: cover → covers

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