

Interactive comment on “Moho topography beneath the Eastern European Alps by global phase seismic interferometry” by Irene Bianchi et al.

Anonymous Referee #1

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Remarks to the Authors:

The authors of "Moho topography beneath the Eastern European Alps by global phase seismic interferometry" image the Moho depth along the transect of the EASI temporary seismic network in the Eastern Alps with GloPSI, which is a technique to extract coherent phase from the stacked auto-correlations of teleseismic waves. The authors found a simple crustal structure and hereby a clear Moho reflection phase in the northern part of the transect. While in the southern part under the higher crest of the Alps, the results indicate a more complex velocity structure with multiple strong impedance contrast in the crust and an ambiguous crust-mantle boundary. The results confirm ob-

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servations in previous CSS and RF analysis. Overall, I think this manuscript is written well. However, the manuscript currently leaves room for improvement. This probably requires a minor revision. My comments consist mostly as requests for clarifications of the methodology. I hope they will be useful to transform this work into a seminal paper.

Comments and Questions:

1. The organization of the introduction section is somehow confusing, the two long paragraphs are rather tedious, making it hard to follow. I'd rather split into short paragraphs, and each short paragraph discusses just one main idea. For instance, the general introduction of 'Moho', the current research status, the main research interest, the choice of method (its advantages compared to CSS and RF) could be separate paragraphs.

2. line 34, "Anyways ...": this sentence does not seem to connect with the context.

3. line 41, "The wide-angle ...": recent seismic tomography studies give pretty reliable estimates of the Moho depth (such as Lu et al. 2018; Lu et al. 2020; Qorbani et al. 2020). I think it would be good complementary info, at least should be mentioned, in spite of the relative weak sensitivity of seismic wave traveltimes to interfaces.

4. lines 85-105: could you further clarify the motivations of using GloPSI? I have difficulty in understanding why the GloPSI could provide new info beyond the RF analysis, as for instance, the influence of a complex crustal structure would affect the two methods in imaging the Moho in a similar fashion.

5. line 116, "Our ...": could you clarify the reasons for using the time range -10 to 80 s around the P-wave onset? This might involve two subquestions: i) why not use S-waves? I would also expect a clear cross-term between S and S reflection phase from auto-correlations, combined with that of P-wave, could help to interpret the final results; ii) why use a long lag time until 80 s after the P-wave onset? Does this mean that the long P-coda also contributes to the recovered cross-term between P and P

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reflection phase?

6. line 124, "For ... ray parameter 0 to 0.06 s/km": I do not see the reasons for using such a ray parameter range. I roughly calculated the arrival time for different phases of the receiver side, and it seems to me that the chosen ray parameter range would not help to cancel out the "spurious arrivals", such as cross-terms between P-waves and its later reverberations (depth phases).

7. line 126, "After ...": could you clarify the choice of frequency range? I think the low-frequency content will not contribute to the final results since it is less sensitive to the interface due to finite-frequency effects. Moreover, I am afraid it will result in artifacts in the later processing procedures.

8. line 127, "spectral balancing ...": could you further explain the motivation of applying spectral balancing? This might also recap comment 7 on the usage of low-frequency content.

9. line 148, "We ...": this might be a fundamental concern: by checking fig. S1 to S8 in the supplementary materials, I have the feeling that the result highly depends on the choice of the pool of events used for imaging. In this way, the results will be more subjective and less convincing.

10. line 165: the use of "clearly visible" is somehow overrated.

11. lines 165-173: It is not clear to me the reasons behind these observations. I guess the difference between crustal features (positive, red) and mantle features (negative, blue) in the BAR image is coming from the low-frequency content in the auto-correlations, as the low-frequency representation of the reflection response. The removal of the low-frequency content leads to the change from a single impulse to blue-red-blue phase alternation in DPR image.

12. lines 180, "We also": I have difficulty in understanding the absolute values of std in Fig 3. If it is std of the amplitude, I would suggest having an additional assess-

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ment of the depth uncertainty of this cross-term between the P and P reflection phase associated with the Moho interface.

13. lines 194, "This is ...": why the SSR signals are still visible seeing that they are much less constructive than the RSR signals?

14. lines 215, "Unfortunately": I would recall comment 3, the results from recent tomographic studies.

15. lines 222, "The suggested ...": I think the GloPSI method has difficulties in imaging the Moho interface (in spite of its geometry) in the presence of a complex crustal structure (see also comment 3). As a consequence, it is hard to conclude that there exists a complex Moho topography. In other words, it is simply not imagined. This might concern the interpretation throughout the MS.

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