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Solid Earth Discussions

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

Anonymous Referee #2

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This paper intends to apply the global phase seismic interferometry (GloPSI) method to passive seismic data acquired along the EASI profile in the Eastern Alps in order to provide additional constrains on the Moho topography in this region. First, I have to admit that I have never been fully convinced that this method is a very suitable tool to precisely image the crustal structure (compared to receiver functions for example) as it is intrinsically hard to remove artefacts like source-side reverberations (as stated by the authors and in several other published papers). Nevertheless, this is a potentially interesting contribution. However, I consider that the paper needs to be significantly improved and polished before publication. It can be appreciably shortened without losing its interest. Below I indicate, for each part of the paper, my main concerns as

well as other more minor comments.

Introduction

Main comment: This section is unnecessarily long and needs to be significantly shortened. It should also be better organised by clearly separating the presentation of the geodynamic context, the past studies (mostly from CSS and receiver functions) and the questions pending in the area of interest. The first part related to the Moho and Pn/Pg/PmP phases + development of CSS methods worldwide is not necessary. Later on, there are numerous back-and-forth between the presentation of the different seismic profiles, their main results in terms of Moho depths and the geodynamic implications making it very difficult to follow. The focus should be mostly on the available data and previous results in the area of interest, naming along the EASI line.

Other comments: (l19) A proper reference to Mohorovicic (1910) is lacking (l19-20) "in seismic records from intra-crustal earthquake" => lacks a verb (l27) replace "lithosphere" by "domains" (l30) CSS should englobe both refraction and reflection methods (l34) remove "for the Moho topography" (l34-35) Why "anyways"? Why unravelling the Moho beneath the Alps is a challenging task (compared to other regions)? (l45) Replace "well known" by "typical" (l48 and elsewhere) replace "W" by "west" (l46-50) The sentence is too long and should be simplified (l70) Which passive methods are you referring to? Why do you say that it is more challenging to differentiate the Moho from other interfaces from passive methods? (l71) Why such a sentence about the fact that sources and receivers are at the surface in CSS? (l71-79) This discussion on migration and 3D effects is unnecessary long, unclear, and I don't see the link with the present study. (l80-81) Where should be this "Moho triple junction" or "Moho gap"? (l86) Define the acronym "RF" used for receiver functions (l90) "The Moho is not imaged": Add an adjective like "well" (l91-92) "inconclusive converted signals by RF" does not mean anything (l95-96) I don't understand what you mean by "turning passive measurements" (namely earthquake signals) "into deterministic seismic responses". (l96) Which "principle" are you talking about? (l94-98) Unnecessary long (Figure 2): What are the units

of the color scales? + Add orientation on top of Fig 2A (and Fig 3A)

Data and method

Main comment: The presentation of the GloPSI technique should be improved / simplified / better organised. On the one hand, there are unnecessary repetition making it difficult to follow and understand the various processing steps. On the other hand, it lacks more precise information on these various processing steps, how they are implemented and their respective role. For example, the author should remind what is the muting of the delta-pulse and why it is required. Same comment applies for the multiple suppression. Based on Figure 2b and 2c it is unclear to me what is the influence of this processing step (I just barely only see a reduction of the amplitudes between Fig 2b and 2c). Also, the values chosen at each step should be given (for example which filter is applied to remove the "delta pulse") and the effect of modifying these values on the resulting images can be discussed. For example, it would be interesting to test/show the effect of spectral balancing and be more precise about the way it has been implemented (for reproductivity of the study).

Other comments: (l113-114) Unnecessary repetition of the fact that you also use direct P waves (l114) Be more precise about the distance range around 150° you exclude (l115) Replace P by PKP (l116) What do you mean by "The 64 events display a high station coverage"? (rephrase) (l121) replace "result" by "part" + explain what you mean by "muting the delta pulse" (the explanation appears later in the text but should be improved) (l123) There is no moveout correction performed before the stacking? (l124) What are the "spurious phases created"? (probably refers to SSR) (l127-128) The explanation for the "spectral balancing" is unclear. To me the objective is to get closer to the spectrum of a delta-like function (l130) If the autocorrelation is only applied on the phase spectrum then the spectral balancing (which is performed on the amplitude spectrum I imagine) is unnecessary (l134) Be more precise about the static correction you applied and explain the technique used to eliminate the surface-related multiples (l139-140) You already presented the Alp01 profile in the introduction

Results

Main comments: - The selection of the 27 events out of the 64 available ones is still unclear to me. First, I don't see major differences between Figure 2 (with 27 events) and figure S8 (with all the 64 events). Secondly, the authors states in the text (l193-l197), supported by the interpolated reflectivity images (Fig. 4a and 4b), that using the 64 events tends to reduce source-side reverberations in the reflectivity images. But later they favour the results based on the selected 27 events (l201 : "Consequently, we decide to use image obtained with 27 events . . ."). Third, it is unclear to me why some source-side reverberations (SSR) should cancel out in one part of the profile (northern part) and not on the other part (southern part). Is there a physical reason for that?

- Crust/Moho signature: I don't understand why in the BAR images the crust should (physically) correspond to positive (red) features and the mantle to negative (blue) ones. Is it an effect of the high pass filter applied to the initial data? Why (physically) the Moho would appear as a blue-red-blue triplet after muting the delta pulse? Moreover, such triplet is not always well seen (or with a symmetric shape) like for stations $\sim$15 to 25 on Fig. 2.

- Phase at 12s for the southern stations (l177-180): I don't understand the argument bring by the author to consider this phase as an artefact for the southern stations but not for the northern stations (this phase is also seen for stations $\sim$17 to $\sim$30). If it is a source-side reverberation it should appear for all the stations (except if the authors selected some events only for the southern stations . . . which should be avoided).

- Standard deviation (Fig. 3 and S9): It is difficult to compare both Fig. 3 and S9 as the scale is different. Personally, I don't see a major difference between the images . . . Moreover, the higher std's are observed for time ranges where Moho reflected phases are expected. Therefore, can we really interpret the reflectivity images in this time range? (the authors states at lines 185-186: "The time location of larger standard deviation [. . .] should not be interpreted geometrically" !)

- Finally, the authors say that “it is quite likely that the entire [...] signal below the Alps is dominated by artefacts” (l200-201) but later they often interpret several features (Moho, intra-crustal structure) in this area (cf. l206, l215, l222-223 + Discussion part and Figure 5).

Other comments: (l144) The Fresnel extension should depend on the frequency used and the depth (l145-147) Unclear sentence. Please rephrase (l146) Why do you say that “only the Moho fulfills these requirements”? What about a continuous intra-crustal or upper-mantle reflector? (l156) The fact that the authors have “more” phases available than Ruigrok and Wapenaar (2012) is due to different selection criteria (Ruigrok and Wapenaar used only M>6 events and PKiKP and PKIKP) (l162) In Figures 2a, S7a, S8a (BAR images), the Moho rather corresponds to the limit between positive signals and negative signals (although I don’t really understand why) rather than “a strong positive signal”. (l165-169) Explain the reason why the Moho should correspond to a blue-red-blue feature + avoid the repetitions among the various sentences (l169-l171) Give a physical reason why the crustal “features” should be positive (red) and the mantle “features” should be negative (blue) in the BAR images. (l187-l191) Various part of this paragraph are unclear (“source of wave energy are the selected earthquake”, “the subset 27 events that are closer clustered in space”, “the second source denotes the targeted structure”, ...) and should be rephrased. (l204) Why do the authors choose to “pick the lower zero crossing (within the blue-red-blue triplet)” as the Moho and not the central positive pick?

(Figure 4) what type of interpolation is used? What is the unit of the amplitude color scale? Why is it 5 times higher than on figure 2?

Discussion and conclusion

Main comments: - The authors compare they Moho depth estimates to other studies and challenge these previous results (especially the ones from Hetenyi et al. (2018)). But how sensitive is their migrated image (and corresponding estimation of Moho depth

/ topography) to the uncertainties on the velocity model they use for the migration? In the conclusion they mention a potential anomalously high-velocity lowermost crust beneath the Bohemian massif but it is unclear to me if they have it (or Hetenyi et al. (2018)) in their velocity model and what would be the effect to include/remove it.

- Both in the introduction and the conclusion the authors mention “opposing” views and geodynamic interpretations of the seismic profile in the area in the literature. In this section it would be good to better indicate which of these previous views are supported (and which ones are not) by the results of their study.

Other comments: (l260) Change “not supported” by “not seen” (l280-ll282) Based on this sentence it is still unclear to me why the GloPSI results differ from the RF results from Hetenyi et al. (2018) between 150 and 300km. Both are based on the same EASI stations. Do you mean that the “lateral velocity variations in the crust” (l282) are not properly taken into account in Hetenyi et al. (2018)’s velocity model used in their migration (as stated before l256-l257)? (l303) replace “Europe” by “European plate”. (Figure 5) Legend of Figure 5 indicates that “the Moho signal disappears where the Moho steeply dips beneath the central part of Eastern Alps” => But if the Moho disappears you cannot say that it is steeply dipping!! Please rephrase.

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