

Interactive comment on “Lithospheric image of the Central Iberian Zone (Iberian Massif) using Global-Phase Seismic Interferometry” by Juvenal Andrés et al.

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After reading the manuscript comments, I warmly suggest to address the points raised by the reviewers. The Global phase interferometry is lately being considered as an established technique, and I recognize its potential, anyways figures like Figure 4 and its interpretation might be misleading for inexperienced readers, and for those who want to produce similar experiments. In particular I agree with R2, which states that the results seem to be over-interpreted.

I will consider the manuscript for publication after the authors will address the comments of the reviewers and the following:

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First of all, some sentences on the data quality are missing. Far distance events with $5 \leq M \leq 5.5$ might contain very low energy, the authors have to state if they have made some selection among the recorded data and how; moreover, showing one example of $M = 5.2$ is not enough, considering that its epicentral distance from the array is not mentioned. At least the authors have to show that events with $M \leq 5.5$ occurred at the largest possible epicentral distances still retain the energy necessary for this kind of analysis. The authors have to show that their data selection is the best as possible, a way of proving it is to show how the image would look like if only events with $M \geq 6$ are used.

Then, the several steps of how the GloPSI image has been created must be shown, in particular, the image before muting the delta pulse at $t=0$ and (as suggested by R2) the results after the deconvolution of the virtual source-time function and subtraction of the average time function (such image might go in the supplementary material). I would also ask how the authors are dealing with the multiple suppression, and if there are any multiples recognized in the produced image.

I understand that the main argument for discerning between crust and mantle is the different reflectivity of the two, while concerning the boundary between upper and lower crust the authors are referring to the different wavelength of the reflections. Honestly, as a not-expert eye, the choice of which of the reflectors has been interpreted seems arbitrary. The different amplitudes might be due to the different events used for imaging the three segments of the profiles. The authors have to clearly show that what they interpret is not an artifact but rather a robust feature, moreover the authors have to show clearly if they have used prior information on the local structure for their interpretation.

Finally, there are several interpreted features in the mantel (outlined by green dashed line in Figure 5). As far as I understand the GloPSI can be applied to very long arrays, due to the fact that this method is valid for interpreting phases that are constantly recognized along the array itself; therefore such short (in space) reflectors might be artifacts and their interpretation misleading. The authors have to show how they can

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distinguish between real signals and artifacts in such an image.

Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2019-107>, 2019.

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