Review of manuscript se-2021-58

Dear Editor,

The manuscript entitled "*Imaging structure and geometry of slabs in the greater Alpine area - A P-wave traveltime tomography using AlpArray Seismic Network data*" presents the first *P*-wave traveltime tomography derived from the entire Alparray experiment dataset. Given the importance of this experiment for the understanding of the Alps, this thing in itself would argue for a prompt publication of this work!

The manuscript is well written, the figures are nice and informative. The structure is ok but I think that the description of the datased should arrive before the methods. The discussion section would also deserve clearly defined subsections. I regret that the dataset and its processing is not fully detailed in this document, though I acknowledge that the processing of such a large dataset deserves a dedicated publication. I thus propose (below) to the authors to add some supplementary information to increase the self-consistency of the article. In particular, I would like that the authors describe the quality check performed on the seismograms.

The method used is classical (ray theory traveltime tomography) and generally well described. The problem of the crustal model is however addressed in a novel way by integrating a 3D model based on local earthquakes tomography as a starting model for the 80 first kilometres of the model.
The consequence of this approach (but I am not sure of it) however caused me some confusion. I have concerns about checkerbord test vertical cross-section shown in FIG. 10. The observed smearing in the first 80 km of the output solution is dramatic and, I think, prevent from any interpretation of this part of the final model. The authors state in the text that this is caused by the geometry of ray coverage (coarser in the lithosphere) but another resolution test in FIG. 15 seems to rule out this proposition (no smearing at all...). It seems to me that the use of a more or less fixed solution in the lithosphere can explain this discrepancy.

Maybe there is something I miss but, given the fact that some important geological/geodynamical implications lie in the upper-most 100 km of the model, I think that the authors must clarify this point.

Given the fact that this point can take some time to be adressed I propose a moderate to major revision of the manuscript. You will find below my detailed comments and questions sequentially organised (not ordered by priority). I would like the authors to address those points in a revised version of the manuscript.

Best regards

Questions and comments

1. l. 86: "the AASN constitutes a massive improvement of observational coverage." Can the authors quantify this improvement ?
2. l. 146: Add the reference just after "FMTOMO".
3. Given the fact that there are OBS at approximately 4 km beneath the sea level and stations at more than 2 km elevation I imagine that the topography is taken into account. Can the authors say few words about that ?
4. section 3: I would suggest to present the dataset before the methods.
5. section 3: I know that data selection and processing is detailed in an other article, but I think that the present manuscript has to be self-consistent. Can the authors gather in a single paragraph the selection

criterions the have used to request the seismograms; for instance and for now, chosen minimum magnitude is indicated at l. 245 and epicentral distances range at l. 251. Can they indicate if the dataset has passed a quality check (and which one, in particular for OBS), as I imagine that some Mw=5.5 quakes can have low signal to noise ratio ?

I also would like the authors to justify their choice of filtering (Butterworth lowpass I guess, poles ?) the seismograms at 0.5 Hz. Given the fact that the majority of the stations are temporary stations, I suspect long-period noise level to be quite high at some sites. A bandpass filtering would have thus appeared more suitable.

6. l. 260, FIG. 6: Use the term "mode" instead of "maximum" to define the value that appears the most in the distribution.

7. Can the authors (quickly) explain how they estimate the pick uncertainties.

8. FIG. 8: Can the authors add the colorbar for panels a-f ?

9. FIG. 9: I do not see the dashed and dotted contour lines indicated in the caption. Indicate or remove the solid and dashed curved that correspond to "faults". Also to be consistent with FIG. 8, it could be more confortable for the reader to place slices that cut through the center of the anomalies to the left panels and the one that cut through unperturbed zones on the right panels. Is it possible to add the colorbar on the first part of the FIG. 9 ?

10. FIG. 10: If I understand well, the upper part of the initial model is composed by a linear combination of two 3D models (Diehl et al. (2009) and Tesauro et al. (2008)) and a 1D model. The $\pm 5\%$ checkerboard anomalies are thus imposed to this 3D model. Am I ok ?

    Again, if I understand well, the "crustal" model is designed to be possibly (slightly ? l. 215) modified during the inversion. I thus wonder if the smearing that we see in the resolution test is not mostly caused by this parametrization and not by the ray coverage.

    What makes me suspicious is that the zone with the strongest smearing appears to be close to 80 km thick, *i.e.*, close to the 77.5 km thickness of the initial "crustal" model. It is also to note that despite the change in size of the anomalies, the recovered geometry is almost excactly the same above 100 km in both cases. Can the authors comment on this point ?

    Also, the authors explain that the smearing is mostly due to the fact that ray paths are mostly vertical beneath the array. If this is the case I would have expected the smearing in the lithospere to be mostly vertical, which is not the case. Can they add an E/W vertical cross-section to see if this pattern is also present in this direction ?

11. FIG. 13–14 and text: If the resolution test shown in FIG. 10 is indeed strongly affected by smearing above 100 km depth, I think that the slice at 90 km cannot be interpreted and should thus be removed.

    This is particularly critical for the cross-sections in FIG. 14 as the presence the "W" and "D" anomalies are mainly visible above 100 km depth.

12. l. 440: It is, I think, reasonable to interpret high-velocity anomalies as lithospheric slabs on the base of petrophysical and geodynamical arguments but not because of "tradition"!

13. l. 453–463: Low velocity anomalies are often observed around subducting slabs. Faccenda and Capitanio (2013) also propose that such anomalies are caused by strong seismic anisotropy in the vicinity of the slab. The low velocity pattern in the Western Alps do follow the trend that can be seen in Barruol et al. (2011). This can be discussed by the authors.

14. FIG. 15 and l. 493: I am confused by the results shown in this figure. FIGURE 10 shows that in the upper-most part of the model, there is a strong smearing to the South along line R1-R1'. The profile in FIGURE 15 is, I think, line R2 (on FIG. 13) – E1-E1' is not defined in map view–, I.E. about 1° to the east of R1-R1'.

    On FIGURE 15, anomalies in the top 80 km are fully recovered, without smearing and without a significant loss in amplitude. How can the authors explain this contradiction ?

Typos

1. l. 208, 216, 218: Add the years to “Diehl *et al.*” and “Tesauro *et al.*”.
2. FIG. 15: Center and bottom panels are both noted (d), (e) and (f).

References

Barruol, G., M. Bonnin, H. Pedersen, G. H. R. Bokelmann, and C. Tiberi (2011). Belt-parallel mantle flow beneath a halted continental collision: The Western Alps. *Earth planet. Sci. Lett.*, **302** (3–4), pp. 429–438. DOI: 10.1016/j.epsl.2010.12.040.

Diehl, T., S. Husen, E. Kissling, and N. Deichmann (2009). High-resolution 3-D *P*-wave model of the Alpine crust. *Geophys. J. Int.*, **179** (2), pp. 1133–1147. DOI: 10.1111/j.1365-246X.2009.04331.x.

Faccenda, M. and F. A. Capitanio (2013). Seismic anisotropy around subduction zones: Insights from three-dimensional modeling of upper mantle deformation and SKS splitting calculation. *Geochemistry Geophysics Geosystem*, **14** (1), pp. 243–262. DOI: 10.1002/ggge.20055.

Tesauro, M., M. K. Kaban, and S. A. P. L. Cloetingh (2008). EuCRUST-07: A new reference model for the European crust. *Geophys. Res. Lett.s*, **35** (L05313). DOI: 10.1029/2007GL032244.