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 P -wave traveltime tomography using the data acquired from the temporary network of AlpArray and permanent broadband stations in the study area. The manuscript is well written and well organized. The network and the analysis of the data set are presented on a related paper in sufficient detail. I think the manuscript has the quality to be published in Solid Earth but at least a moderate revision would be necessary.

Below are the details of my comments.

The number of stations used are quite large providing spatial coherence among the stations over the network. On the other hand, the number of events are relatively low (331) and some are lower magnitude events. What is the criteria for using Mw5.5 as threshold magnitude ? How many stations recorded the Mw5.5 events ? There is no information on the S/N of the low magnitude events. If the low magnitude events are recorded by fewer stations, would that create any bias as the average of traveltimes of each event is removed traveltime residuals?

The methodology is explained well and the parameters for the inversion are chosen appropriately. The misfit in Figure 12 drops fast during the first 3-4 iterations while stay almost flat for the rest. But the authors prefer to use the results after 12 iterations for very small improvements on the misfit. It would be valuable to present the results after fast drop in misfit (3 iterations) and after 12 iterations. Is there any overfitting the data by increasing the number of iterations ?

The checkerboard tests are done but requires more, to present vertical and horizontal smearing in more detail. The authors state that the checkerboard anomalies are smeared at least 20 km at shallow depths below the crust. But they do not provide any information over rest of the domain. The initial checkerboard depth models should be provided together with the recovered patterns (Figure 10). Spike tests would be valuable to monitor smearing over the solution space.

The incorporation of the crustal model and upper mantle (<100km) into the inversion is a nice idea although more tests are needed to understand the influence of strong constraints on the velocity perturbations. The results after inversion should be compared to the initial model for depths < 100 km. Is there any bias on the transition from strongly constrained upper part (>100km) to unconstrained lower part ?

The authors compares their results to the previous teleseismic tomographic works. It appears that Zhao et al 2016 used lesser amount of data and attained similar resolutions. It would be nice if the authors display vertical cross sections of few profiles from the tomographic images of the previous works (e.g, Koulavov et al, 2009; Zhao et al 2016 ..) crossing the same structures.

The presentation of the 3D model in Figure 16 is the least satisfactory part of the paper. The Figure 16 does not make any impression neither as geology nor a velocity model. Depending on the level of velocity contours a different image with different size of low velocity holes and slab thicknesses would appear. It appears that some of the research questions the authors posed in the introduction such as “how thick and how long are the descending slabs ? ” remained unanswered. A geology based velocity model derived from the tomography can be used as synthetic test to better constrain the slab thicknesses, the extent of the low velocity zones.