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Interactive comment on "Relocation of earthquakes in the Southern and Eastern Alps (Austria, Italy) recorded by the dense, temporary SWATH–D network using a Markov chain Monte Carlo inversion" *by* Azam Jozi Najafabadi et al.

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Review of SolidEarth-manuscript Najafabadi et al. Dec2020

The manuscript regards the compilation of a local earthquakes catalog of 16 months period with the application of a few modifications and improvements to standard location procedure using the dense AlpArray and SWATH-D temprary station network. The study comprises different topics -procedural steps and results- each of potential interest to a wide range of readership.

Interesting enough the first such topic addressed in the abstract is the description and attempted correlation of the seismicity with the regional geology and tectonics. While certainly precisely relocated, the 344 local earthquakes of a 16 months period by no means could be taken as representative for the seismicity in the region and it should not come as a surprise -and not be seen as regional "characteristics"- that it appears in clusters. For a seismotectonic interpretation linking such observed clusters with tectonic faults to conclude, f.e., that "the general pattern of seismicity reflects head-on convergence of the Adriatic indenter with the Alpine orogenic crust." one would have hoped the authors to take advantage of the great data set with on average 36 P observation per event to complement the hypocenter locations with focal mechanisms at least for the larger magnitude events. Furthermore, a comparison and thourough discussion of the relation of the presented high-precision short-period seismicity with the long-term seismicity pattern reveiled by the official catalogs over the past 30 years is not only possible but necessary.

The main topic and work of the study regards the successful application of a Markov chain Monte Carlo inversion of the 12,534 P and 7,258 S observations from 344 local earthquakes to obtain a 1D velocity model and station delays for the region that allows high-precision hypocenter locations. The derivation of the model is well explained and complemented with the description of a synthetic test to provide a statistical estimate of the location uncertainties. In addition, a "ground truth" location experiment with quarry blasts is presented and discussed. This part of the manuscript is very clearly presented and contains a lot of technical details that allow the interested specialist to follow most steps. Considering the readership that might be interested in the seismicity and their tectonic interpretation though, I suggest to most of the chapter 5 could be moved to the supplementary material. What is missing, however, is a critical discussion of the results and, in particular, their relevance and meaning for the seismic catalog of 344 events presented. Considering that with the Markov chain MC inversion the authors address the coupled hypocenter-velocity model problem for the complet (very high-quality in terms of number of observations per event) 344 event data set, I do not understand why there is no mentioning about the internal consistency of the hypocenter solution or about the great potential of these results as initial data (hypocenters and model alike) for 3D seismic tomography. Rather, the list of relocated earthquakes is presented simply as a higher-precision-"than INGV/ZAMG“ catalog for the region.

Finally, the study also contains a section interesting for seismologists (observatory tasks and seismic tomography) about the semi-automated picking of such a large data set. Much of the description of this work part is already allocated in the supplementary material and it should remain there. It would be logic though if the reference to this work and the presentation and discussion of the results would be appearing before and not after the Markov chain MC inversion of the data set (see Figure 7). Furthermore, some details important for the specialist are missing (see individual points 10 to 15 below).

The chapters 6 (Results in discussion) and 7 (conclusions) read like they were written by different people with totally different interests and perspectives and the only connection between the two parts are the 344 precisely located hypocenters. There is no geologic-lithologic interpretation or at least comment about correlation presented between the other results (notabene of great importance for the claimed reliability and accuracy of the hypocenters) of the coupled problem, the velocity model and station delays. The tectonic interpretation of the seismicity presented in chapter 6.3 (pages 21 to 25) is missing taking explicitly into account (and explaining to the reader how and why this is used as arguments for the interpretation) the great advantage of this study (having an event data set of high internal consistency and high precision hypocenter location of quantitatively known uncertainties) and the significant limitations (pre-selected events of unspecified magnitude of completeness and only 16 months of observation period).

In consideration of the above general remarks on quality and deficiancies , I suggest moderate revision of the manuscript before publication.

Specific points:

(1) Line 12. Replace "accuracy“ with "precision estimate“. Note that in line 13 you correctly assess the "accuracy“ with the blast location test. (2) Line 15. Delete the rest of the sentence after:“ ..1.7 km in depth.“ (3) Line 27. Replace "accuracy“ with "precision“ (4) Line 48. "... has the advantage of being“ largely "independent ...“ (5) Line 51. "best model(s).“ a note on ambiguity would be useful (6) Line 64. Please outline Adriatic microplate in one of the Figures. (7) Lines 67-75. Needs a figure to show the strain if you keep the introduction as is and the chapter 6.3. (8) Figure 1. See point 6 above. Red box in bottom figure does not correspond with bounds of upper figure. This figure is not providing all necessary tectonic information mentioned in the manuscript. You should note that the seismic catalog presented by ISC is by far not complete down to magnitude 2. If you want to show the big picture use either EMSC catalog likely complete to M3 or ISC likely complete to M3.5. Otherwise you could use a composite of the various national catalogs that probably are complete to M2.5. (9) Line 85. Actually there are earlier catalogs that were compiled: European Geotraverse Blundell et al. 1992, Solarino et al. 1997 (10) Lines 115 to 119. You need to elaborate in detail (this can be done in supplementary material but it is absolutely necessary to have this information) how you identified the "common“ events and how in the end you established the event list of the 2619 events. (11) Lines 120 - 126. The discussion of the results of this semi-automated picking (that is well described in suppl.) needs to be more extensive and detailed. On what basis did you define the selection criteria (gap<200 , why not <180’) (RMS <1s), why no mentioning of number of P obs? Did you check all 12534 P obs manually? (12) Line 134-6 and Figure 3. The Wadati diagram shows significant numbers of observations with +/-3s residual relative to constant Vp/Vs ratio. Note the the Vp/Vs ratio varies within the crust and at Moho. You may see this in the Figure as the straight line is systematically shifted onto the side of the highest point density after about 25s P travel time. What residual range do you define as corresponding to the sum of 3D, lithologies and regular Gaussian observation uncertainty effects and what value denotes an outlier? (13) Line 144 and Figure 4. "Checking the phase-type is extremly important.“ I fully agree and record

section display is a good zero-order approach. However, I do not think your figure 4 is of help for doing this. How realistic is the ak135 global model for P phase identification considering the Moho topography by Spada et al. 2013 (your Fig.5c) or the 3D LET model by Diehl et al. 2009 but most important the literally more than a dozen refraction seismic lines that have been published (for a review see Kissling et al. 2006). (14) Line 152. Please provide clear evidence and explain in detail strategy to identify PmP phase by using a totally inadequat 1D model. (15) Lines 152/3. "the number of outliers, ..., is not significant to the total number of picks.“ What value do you define as being an outlier? (analog question to point 12) Note there are dozens of observations +/-2s from the main intensity of data points (that by the way is totally off your Pg line) and that individual hypocenter location precision (and even more important for accuracy) is in truth measured by the fit of just those observations that refer to the specific event. (16) Line 218. "... does not depend on initial hypocenters, ...“ I seriously doubt this (does not depend) and suggest to phrase it differently. Consider how you would identify an outlier with Figure 4 if you do not have a rather good idea the initial hypocenter! Furthermore, consider that you were using a priori information from existing catalogs for your semi-automated picking and that even with all this information you apparently found mispicks and had to select the 384 best events. (17) Line 226. This does not provide an "accuracy“ estimate! May be internal consistency, precision. And you need to provide reasons why this should be expected to be of relevance for the real individual event locations. By the way, your blast test shows otherwise! (18) Line 230. This statement about Moho velocities is simply wrong. No 7km/s velocity has been reported in the Alps. Please check the literature. (19) Line 232. The 19km are just along the flank of the Ivrea body and not relevant for the Moho topography beneath the Po plain. (20) Line 233. -5km is much to high and you are doing something wrong if you need to avoid rays through air by such model top elevation. Note that the increase in pressure within the earth causes a velocity increase with depth and the seismic waves to show a downward curvature You should use the average station elevation for ray tracing. (21) Line 245. Choosing a constant Vp/Vs ratio of SQR(3) is very

problematic as we know it is wrong because it varies and likely the average is different. (22) Line 250/1. Not only refer to table but provide correct value here. (23) Figure 5. Your model extent in Figure 5a does not correspond with your map extent in Figure 5c AND either of these extents differ from your study region shown in Figure 1 AND all of these are different than your Figure 2. Make certain you everywhere show the same study region extent, if you want to show more area around, then mark the study region. (24) Figure 5b. The Moho topography is wrong. There is a Moho offset across the plate boundary but you show a vertical Moho interface! (25) Lines 259/60. "average uncertainty of 240m in longitude, 270m in latitude ....“ How did you determine that? In such way this information is not usefull. With what probability do we have what location uncertainty for any single hypocenter? (26) Figure 7. Move to supplementary material. (27) Figure 8. Again a DIFFERENT STUDY REGION SHOWN!. What about the stations to the West of the Tauern window (as example, there are other regions with no visible symbols)? Do they have all zero station delay values or did you not obtain any values for them? Please explain in more detail what the velocity-depth function shows. In may view, it documents the data set is not capable to resolve the velocity structure below 30km depth and certainly not the Moho. This does not come as a surprise as it is well known that you loose vertical resolution below your deepest hypocenters. (28) Lines 297 to 304. This model discussion is inadequate with regards to the previously published information about the crustal structure. If your model does not allow to resolve it, then say so and it is OK. But do not claim it is in agreement with prior independent knowledge if it is obviously not. (29) Line 320. "it proved to be useful for accurately localizing earthquakes ..“. I am missing the prove. Please explain how this was proved. (30) Figure 10. Figure 10b I would again derive the conclusion from this figure that you are lacking resolution power below 30km for Vp and below 20km for Vp/Vs ratio. Figure 10c. Now this looks like the study region. Why not always marking this extent where you do have data from? How do you interprete the distribution of the station delays? Note that there is a single station delay strongly different from all others within its vicinity located near 11.7E/47N. If I obtained such result I would check if it is real or caused by bad data of some sort. Note that otherwise your largest station delays are all within the periphery of the study region as this is well known from minimum 1Dmodel applications. (31) Lines 325 to 335 and Figure 11. What hypocenter depth did you test these blast locations with? How do you define the mis-location vector? Relative to the center of the quarries or do you know the precise location within the quarry for each blast? This accuracy test shows that your previous precision estimates were a bit to optimistic but the accuracy is still very good. (32) Line 331. Please explain in theory why you suggest that including S observations improve the hypocenter location solution? (33) Lines 340/1. Your absolute depth uncertainty has been documented by your ground truth accuracy test (Fig.11) to be a few km and the epicenter location uncertainty is about 1km. Please correct your numbers. (34) Lines 343/4. These differences are indeed significant. However, as it just regards a selected best event data set with on average 36 P observations this is comparing apples and grapes. Your data set is excellent for seismic tomography but absolutely not representative for a seismicity catalog (magnitude of completeness? Just 16 months). On the other hand, the national seismic catalogs contain many poorly locatable –or you could also say difficult to locate- events that need extra processing time to obtain a complete catalog and there is the significant difference in number of stations. I believe it would be useful to discuss thourougly these difference in addition to presenting just the numbers. (35) Figure 12. Figure 12b is not needed, just provide the uncertainty estimates. Note that for cluster interpretation relative hypocenter location uncertainty estimates (and that is what you obtain with your Markov chain MC inversion of the coupled problem that includes a joint hypocenter determination approach) are most important while for absolute location obviously the accuracy is key. (36) Figure 13. For seismotectonic interpretation of clusters along a fault system, you should definitely employ focal mechanisms.

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