# 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.

Anonymous Referee #3
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Dear Referee,

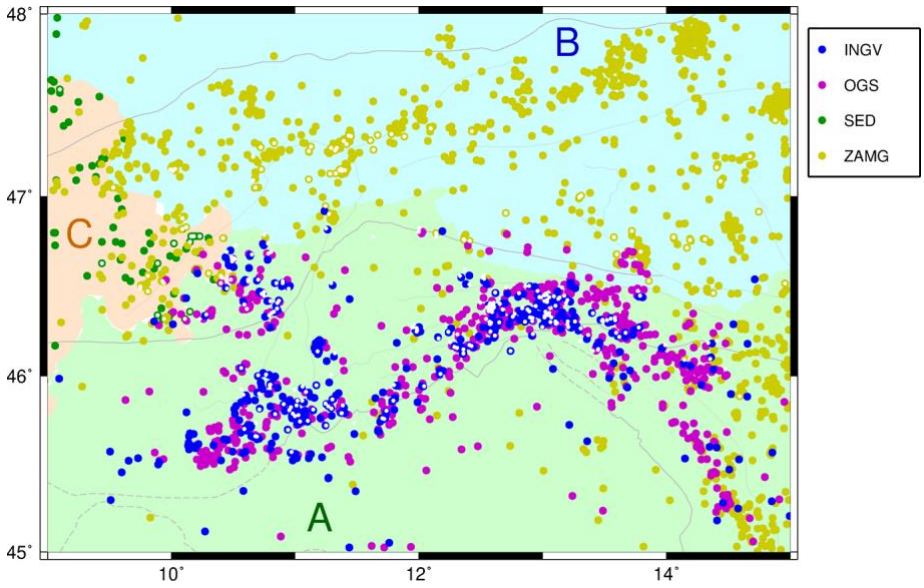
We would like to thank you for your thoughtful comments on the manuscript. Your valuable feedback will help us to improve the quality of the manuscript. Following, please find our response to your comments as blue texts.

The present manuscript by Jozi Najafabadi et al. presents the compilation of a seismicity dataset that will, I presume, eventually be used for a local earthquake tomography study of the Eastern Alps. Using recordings from the dense SWATH-D deployment, they present a careful procedure of obtaining and verifying arrival time picks, derivation of optimal hypocenter locations and a best-fit 1D velocity model. The Bayesian approach for the inversion of hypocenters, velocity model and station corrections is something new, and the present manuscript provides a nice case study for its application. Lastly, the obtained hypocentral locations are compared to mapped faults, from which the apparent activity or non-activity of a number of structures in the Eastern Alps is inferred. This last part is where I see some potential problems that will make some changes to the manuscript necessary. Overall, the paper is well written and definitely of interest to the readership of Solid Earth and the special volume "New insights on the tectonic evolution of the Alps and the adjacent orogens". I recommend moderate revisions before publication.

General comment:

In section 3.2, it is briefly mentioned that the events for which arrival times on SWATH-D stations were obtained and that were then relocated, used for deriving the 1D velocity model etc. were selected from a synthesized catalog that was based on the bulletins of national agencies. For the tectonic interpretation to be viable, this part needs to be made much more transparent. While it is fine to choose a subset of events based on network criteria when working towards a tomography study, it is a completely different thing when the activity or (more crucial) non-activity of faults is inferred from such a subset. In clearer words: the authors need to convincingly show that their chosen subset of events is representative, and does not systematically miss events from certain regions. I suggest to provide a map with all 2639 events from the different national catalogs, in which the chosen 384 are marked. It would likely be even better if the station distribution from the different national networks could be shown as well. I also suggest to better describe the reasoning behind this approach of choosing a subset of events from national bulletins. Are the national bulletins complete enough that one can exclude that the dense SWATH-D network contains signals from small, previously undetected events? Or was the focus on the larger events that would generate arrival times at a larger number of stations?

You are correctly pointing to the main aim of our work which is local earthquake tomography, and this is the reason to focus on creating a catalog with the most consistent earthquake hypocenters (and corresponding travel time picks). Our hypocenters have the highest precision in the region (at this time) and our clustered seismicity agrees very well with seismotectonic characterizations based on long-term records. Therefore, our hypocenters can shed light on active faulting and enables us for a convincing tectonic interpretation.
We totally agree that the dense SWATH-D network will make it possible to detect very small earthquakes not contained in the national catalogs. However, for the purpose of locating the earthquake with high accuracy - and in turn using the dataset (hypocenters, travel-time picks) for local earthquake tomography - we were mainly interested in earthquakes of some magnitude so that they are observed by a number of stations (see section 3.2 of the updated manuscript). We assume that these earthquakes are to a very large extent listed in the national catalogs (example: minimum magnitude -0.8 by ZAMG for the Austrian part of the study region).




Here is a map containing all 2639 events from the national catalogs and the chosen 384 events are indicated by white dots. Unfortunately, we have no information regarding the stations for the national catalogs. The number of figures in our article is already rather large, thus we would like to not include this figure in the manuscript. The events by national catalogs are publicly available.

I agree with a previous reviewer that fault plane solutions would be nice to have for a more detailed tectonic interpretation. However, I can see that the main aim of the manuscript is the description of the dataset that will be used for tomography, and speculate that the tectonics part was added mainly for the sake of the Special Volume topic. I believe the careful derivation of the hypocenters and velocity model, using a rather novel approach and performing many quality checks, is in itself enough material, so that a deeper-going tectonic interpretation employing focal mechanisms is not strictly necessary here,
Thank you very much for this comment. In the meanwhile, a manuscript dealing with the focal mechanisms was submitted to Solid Earth (Petersen et al., 2021). We mention this now in our manuscript and added the reference.

Specific comments:

**l.25:** Why were only data from 2017/2018 used when the stations ran into 2019? Should be mentioned with a word or two.
At the time of doing this analysis, the data from 2019 was not available. We will include an updated dataset in the LET study.
Also, mentioning the total number of SWATHD stations here could be useful, especially since the number of AlpArray stations that were also used is brought up in the next paragraph.
Done

**l.33/34:** "to identify the status of the seismically active volume...". This is a strange formulation, and should be changed.
Modified to:
"… to identify the general pattern of seismicity on the surface and at depth"

**l.36:** remove the
Done

**l.55ff:** I would recommend to use fewer abbreviations, this is making the manuscript unnecessarily hard to read. Best limit abbreviation to a handful of terms that really show up a lot throughout the manuscript, and write out the rest (this is maybe also my personal taste...).
We actually use these abbreviations several times in the manuscript, especially in the section "Results and discussion", and also in the figures. We think that using the whole word makes the sentence long (in some cases) and also reading the figures alongside the text would be more difficult!

**l.82:** this bracket is not closing again
Agree - corrected

**l.91:** stuck should be struck; corrected
about what time interval are we talking for the ML>6 earthquakes? Last decades, centuries, millennia?
"in the last several centuries (Slejko, 2018)" – This is added to the text

**l.96/97:** This statement is problematic, because while the present study is using a denser seismic network, the chosen approach of using a subset of events from agency bulletins (see General Comments) makes it impossible that previously missed events (if they exist) will be detected. Thus, the present study can do nothing to address the problem that is hinted at here (inactive region maybe because not well instrumented).
Agree – The improved coverage of the region with seismic stations was one of the main aims of the deployment of the dense SWATH-D network, however, the issue towards a more complete catalog is not directly covered in our study. Other studies dealing with this issue are underway, but results are not yet available. Accordingly, we think that this statement is not necessary here and thus we deleted the last two sentences.

**l.105:** The "however" doesn't fit here
Agree - corrected

**l.111:** remove "stations"
Done

**ll.115-119:** For me, this paragraph is the main problem of the manuscript as is. At least for the tectonic interpretation part, the authors need to convince the reader that no selection bias of earthquakes exists, i.e. that regions interpreted as aseismic based on the chosen 344 events are also aseismic if one looks at the entire >2600 events in the original database (see recommendatiosn in General comments). Also, the statement here seems to indicate that the national bulletins were deemed complete, which stands in contrast to l.96/97.
Our statement in lines 96-97 (which is eliminated in the updated manuscript) was related to individual seismological studies in the region (lines 85-86 of the original manuscript), and not national bulletins. However, if we consider the national bulletins with 2,639 events to be complete, it also indicates rare seismicity in the aforementioned region. We are not claiming for a complete earthquake catalog, but for a high-precise catalog of seismicity which agrees very well with previous studies in the region and also national bulletins.

**l.135/136:** how were outliers defined, and where can I see outliers in Figure 3 (can they be marked?)
All the observations that have $t_s$-$t_p$ >0.72 $t_p$ ± 4s were considered as "outlier" and removed from the data which formed only 0.3% of whole observations. After this removal, only 2% of the whole observations have ts – tp > 0.72 tp±3 s.
Based on experiences with similar datasets and according to own synthetic tests (using the source and receiver geometry of this study and including 1- a Moho topography based on a simplified Moho from Spada et al., 2013 (see section 5), 2- assumed (moderate) differences in average Vp/Vs ratio in the crust and upper mantle, 3-crustal Vp/Vs ratio anomalies which we see (in respect to spatial dimension and amplitude) both in our first tomographic results and in other regions of the world, and 4- picking uncertainties) we expect a scatter of no more than ± 2 to 3s around the linear trend of the points in the Wadati diagram.
More detailed information is provided in response to point (12) of Reviewer 2.

**l.138:** If a part of the goal audience are people mainly interested in the activity of structures in the (south)eastern Alps, the three phases and the triplication distance should be briefly explained, e.g. in a brief sketch that could be added to Figure 4. Also, giving an estimate of the overtaking distance, e.g. with the crustal thickness and velocity given in ll.147/148, would be beneficial.
Motivated by the critical questions and comments regarding Figure 4 by reviewer 2, we modified the text dealing with the phase-type identification considerably. By doing this, we also deleted Figure 4 which seemed not helpful in this context. Please see more details on this issue in the response to reviewer 2.

**l.140:** be (remove ing)
Done

**l.158:** is indicated the right word here?
Agree – the sentence is modified to:
"… a simultaneous inversion for hypocenters and velocity structure (and/or station corrections) is needed."

**l.160:** Well, a Bayesian-type approach has been used for all these geophysical studies. As it is written, it sounds like this was always the exact same approach (which it wasn't)
We agree. We modified the text slightly:

“Different to the conventional approach of damped least squares, we use a Bayesian approach (Bayes, 1763). Bayesian approaches have been applied in a number of geophysical studies (Tarantola et al., 1982; Duijndam, 1988a,b; Mosegaard and Tarantola, 1995; Gallagher et al., 2009; Bodin et al., 2012a,b; Ryberg and Haberland, 2018). Ryberg and Haberland (2019) recently implemented a hierarchical, transdimensional Markov chain Monte Carlo approach for the joint inversion of hypocenters, 1–D velocity structure and station–corrections for the local earthquake case.”

**l.176:** structure (-s)
Done

**l.212:** reformulate that first sentence Section 5: I am not completely sure I understand the reasoning behind this test. The authors construct a first-order 3D velocity model of the Alps based on published data and perform a retrieval check using the real data (hypocenters, stations) as input. Thus any misfit in the output should stem from 3D structure and/or general uncertainty, but only with the assumption of this specific 3D model...since the true 3D structure of the Alps will almost certainly differ from the utilized model (presumably only to secondorder differences?), do we have any idea if the retrieved 3D effects are similar for a (subtly, substantially) different reality? I think the paragraph needs a more detailed description about the purpose of the test, what it is supposed to show and what it can not show. Nevertheless, I appreciate the effort that went into performing it!
With this test, we want to study how (well) the synthetic hypocenters (and the velocity) model are recovered by our inversion routine. We think this is interesting because we use a simplified framework in the inversion (i.e., 1-D model, station corrections) obviously not capable to capture the full 3-D conditions which we expect for the Earth. For this, we designed a 3-D model (based on existing information) which resembles some first-order features in our study area such as Moho topography or shallow low-velocity sedimentary basins. Furthermore, we can study how the (output) 1-D model looks in comparison to the (input) 3-D model, how large the derived noise is in relation to the synthetic input noise and how the pattern of station-corrections corresponds to the shallow velocity anomalies. In inversions for (3-D) subsurface structure (i.e., tomography) people regularly employ synthetic recovery tests.
In order to make this clearer we modified the text. Please see also our comment regarding the issue (17) of Reviewer 2.

**l.268:** Figure (-s)
Corrected

**l.271:** reformulate “rather slight”, add km after 50
The sentence is reformulated to:
“it is modeled by a gradual increase of the velocities at depths from 30 to 50 km”

**l.275:** reformulate “fluctuating”
“fluctuating” is replaced by “irregular”. And by the way, this sentence is moved to section 6.1 where we talk about velocity models of real data.

**l.319:** “contains an overlay site effect” I’m not sure I understand what exactly is meant here.
The sentence is reformulated to:
“contain a superposition of site effects and/or effects from 3–D structural variations”

**l.324:** This is not really a sentence
Agree – There was a mistake in the formulation of the sentence. It's reformulated to:
"To validate the localization procedure, the detected 15 quarry blasts (based on manual/visual inspections; see 3.2) were independently relocated by McMC …"

**l.332/333:** Can hypocenters from the INGV/ZAMG catalogs also be shown in Figure 11, to better illustrate the improvement in hypocenter location?
The large differences in the location of the blasts reported by the agencies to our solutions (and the mislocations) are most likely related to much less data (stations), so it is not surprising that they are different (and show some mislocation). We think that the differences in location are already mentioned in the text. Furthermore, in order not to overload the (old) Figure 11 we prefer not to add the hypocenters to the Figure.

**l.344:** it would be interesting to elaborate a bit more on these differences; is there a trend, eg. with bulletins showing systematically larger or smaller depths?
In response to this comment and also to reviewer 2, we added the following paraphrase to the text:
"Since the national agencies are (probably) using much less data for the location (smaller number of stations used, larger inter-station distances), a significant difference between their hypocenter solutions and those obtained in this study is expected (average of 2.3 km in epicenter and 2.9 km in depth). The earthquake depths calculated by McMC are systematically shallower than those by national agencies (by an average of 1.1 km). The maximum and minimum differences in the epicenters and depths (between McMC and national catalogs) are seen for the earthquakes from the INGV and SED, respectively.
The derived hypocenters in this study do not represent a representative seismicity catalog of the region (the national catalogs contain also many small, poorly constrained events in a much longer period) but form excellent data for further seismological studies e.g., Local Earthquake Tomography (LET). Moreover, this highly precise hypocentral data allows further tectonic inferences. "

**l.345:** This is not surprising, since no search for new events beyond those in national catalogs has been performed
We agree with your point. However, other seismological studies in the region (based on local networks or national agency data) confirm a similar seismicity pattern. Although, considering high-accurate and high-constrained hypocenters throughout the region in our study, a comparison with national catalogs could be meaningful too.

**l.358:** mention the magnitudes of these events
Done

**l.398:** See General Comment: how well can one argue for the absence of seismicity ("seismic gap") based on a catalog that was only a choice of 384 out of 2639 events? A map showing where the remainder of events (those that were not chosen) were located is essential if such a statement is attempted
In the updated manuscript, we emphasize that the seismicity pattern of our study is similar to long-term seismicity by, e.g., Reiter et al., 2018, or seismic catalogs such as the SHARE.
Furthermore, we always (for every sub-region) compared our seismicity with previous long-term seismicity studies. Our seismotectonic interpretation is based on the high-quality hypocenters

that are derived in our study. However, we confirmed the sparse seismicity in part of the region with other studies and thereupon made an interpretation.

**l.405ff:** spelling: Engadin vs. Engadine Fault
Corrected

**l.427ff:** These (at least the first three dotpoints) are not results but the analysis steps that were carried out to retrieve the results. Either only results should be listed, or two separate listings for analysis steps and results are needed.
The conclusion is totally rewritten. Please look at the updated manuscript.
In the name list of AlpArray people, all those containing special characters have formatting issues (LaTeX syntax?)
Corrected

**Figures:**

Figure 1: Typo in Tectonic units legend (forland should be foreland)
corrected

Figure 2: The fault lines in this plot are really hard to see. Choosing a larger linewidth would be helpful. A color scale for the topography would also be nice.
Both done

Figure 4: Please be more specific about what the “various depths” are that were used to obtain the travel-time corridor. Then, I do not see red dashed lines in the plot (as mentioned in the caption). Lastly, I would prefer if the meaning and implications of this nice plot were elaborated a bit more in the text. Is my interpretation of a change from a Pg-like to a Pn-like trend at around 150-200 km correct? How does this fit to theoretical overtaking distances, does this mean that the first arrival is picked everywhere?
We think that the change from Pg to Pn (as the first arrival) is somewhere between 100 and 200 km. This distance depends on both earthquake depth and Moho depth. Since earthquake depths vary between 0 to 20 km and also Moho in this region is very variable, we think that showing the travel-times cumulatively in one graph is indeed not helpful for the manuscript. Therefore, we decided to remove this figure from the manuscript. We also removed some parts of section 3.3 of the original manuscript and moved some parts to section 3.2 (no section 3.3 in the updated manuscript).

Figure 6: missing values in the histograms (Mu and sigma)
The values are by mistake missed in this plot. However, we decided to remove these histograms and showing only the misfits (figure 6b of the updated manuscript) that are more important for synthetic recovery test (also in response to the point (26) of Reviewer 2).

Figure 7: should be 110 m in depth (not km)
corrected

Figure 8: What is the reason for the wide spread of models at very shallow depths? Looks like they did not converge there (same in Fig. 10).
We think that the larger standard deviations of the models shown in Figure 8 (in the original manuscript) are due to the shallow anomalies introduced in our synthetic model (lower

velocities to resemble sedimentary basins, higher velocities for “TW”). Obviously, models with different velocities exist explaining the data more or less equally well.
For the real data (Figure 9 of the updated manuscript), we see actually very small standard deviations down to 25km depth. Maybe the shallow low velocities are also due to shallow lateral heterogeneity (sedimentary basins).
Thicker lines for the plusses and crosses in subfigure b would be helpful.
Done

Figure 9: Typo in label: pahse should be phase;
corrected
also, I guess the green line marks the end of the burn-in phase.
Right - corrected
I don’t really see anything in the right subplot (which also has no axis-labels)
The histogram is shown with the color blue. This is a sharp line centered at 0.36 s. The x-axis is now labeled.

Figure 10: Would it be meaningful to compare the station correction pattern to the one from the synthetic test (Fig. 8)?
To some extend yes. For example, the shallow low-velocity anomalies that we considered for the MoB and PoB in the synthetic model are not too far from the real structure. However, the station-correction pattern related to the TW anomaly in the synthetic test is not seen so clearly in the real data. We modified the text slightly:"
"... Besides, extreme positive corrections are seen in the PoB and MoB as it is expected for sedimentary basins, also consistent with the results of the synthetic test (see above). A pronounced pattern specifically related to the TW as seen in our synthetic test is not observed so clearly in the real data suggesting a different structure in the shallow subsurface. The pattern of stations in the ESA and a few stations in the WSA and CA agree very well with results by Diehl et al. (2009b)."
In the caption, abbreviations CA and EA should be Central and Eastern Alps (not Alpine)
Agree - changed

Figure 11: Caption: remove one of the two “and” in fourth line; see level should be sea level
Both done

Figure 12: Comparing to Figure 2, it seems that the vast majority of events is at the edge or slightly outside of the SWATH-D network. This should be mentioned, and maybe the rectangle shown in Figure 2 can be added here?
We agree, most of the events used in our study are on the edge of the SWATH-D network and some events outside. That’s why we decided to add also data of AlpArray surrounding the SWATH-D network so that all events used in our study are well surrounded by stations (gap<180°). This information is given in our manuscript.
The SWATH-D rectangle is added to this figure.

Figure 13: Caption: “for a better clarity the depth and length scales of cross section A are magnified by a factor of 1.5”; does this mean vertical exaggeration of 1.5? Or only that it was upscaled by a factor 1.5 relative to profiles B and C (without any distortion)?
Both vertical and horizontal axes in cross section A are upscaled (without any distortion) compared to the other two cross sections. This is clarified in the caption as well.

On behalf of the authors,
Azam Jozi Najafabadi