We would like to thank Prof. Edi Kissling and two other anonymous reviewers for their insightful comments on our manuscript. The constructive comments helped us to further improve the manuscript. We edited the manuscript carefully and addressed all comments of reviewers. Please find below the individual comments of the reviewers as black text and our answers as **blue text**. In the appended manuscript, the **color red** refers to old texts that are removed or replaced, whereas new expressions and changes are denoted by **color blue**.

On behalf of the authors,
Azam Jozi Najafabadi

********Answers to Referee #1 ********

In this paper, the authors relocate 344 earthquakes in the southern and eastern Alps by exploiting arrival time data from the temporary SWATH-D network, supplemented by Alp-Array stations. Overall, the paper is well written, the methods well explained and tested, and the results carefully obtained and discussed. I suppose one could argue that the work they carried out would normally be integrated into a local earthquake tomography study, in which preliminary location of events is undertaken and a robust 1-D reference model is generated prior to the tomography. However, given that this is a new and large dataset from a region of significant interest, that cutting edge methods were used for the hypocenter locations and 1-D velocity structure determination, and that the final earthquake distribution does provide insight into active faults in the region, I would be happy to see the paper published following minor revisions.

(1) Line 9: I would be tempted to replace “precise” with “robust”.
Done

(2) Line 43: I’m not sure I would say “...they depend not only on proper choices of initial values for hypocenter coordinates...” - what is meant by “proper” in this context – that they are close enough to the “true” location to make the inverse problem locally linear?
Yes, by proper we mean close enough to the true location. This explanation is now added in brackets in the updated manuscript.

(3) Line 44: I don’t think I would describe damping as a “technical parameter”. It is better described as regularisation.
Agree – changed to “regularization”

(4) Lines 115-119: In a way it’s a pity that automatic event detection was skipped, since presumably the combined array used in this study provides a much denser data coverage of the region compared to what national seismological agencies have access to. Consequently, there is probably quite a lot of seismicity that has been overlooked, and if the purpose of this paper is to examine earthquake distribution and its relationship to active faulting in the region, then that is somewhat unfortunate. However, on reading the next lines, it appears that an automated picking algorithm was applied, but presumably only to data windowed by the pre-existing catalogue? It would also be interesting to know why the combined national seismological agencies were able to detect and presumably locate 2,639 local events, yet only 384 were deemed good enough for the current study. I understand the issue of seismic gaps and noise, but are the national networks more dense than the network used in this study in some regions?

Yes, you are right, the automatic picking is applied only to the data windowed by the pre-existing catalog events. We totally agree that running a detection routine on the data of this dense and large network will probably yield a much more complete set of events (down to smaller magnitudes). However, our aim was to concentrate on locating the events with high precision rather than obtaining a comprehensive catalog. To locate the events with high precision they have to have a minimum magnitude and – in turn – a relatively large number of observations. We are confident that these (larger) earthquakes are contained in the permanent networks' catalogs. Additionally, one reason for this selection – besides the high-precision – is that we plan to use this dataset also for local earthquake tomography. We mention this now more frequently in the text.

Considering that no information on the precision of national catalog events is available and some of their events are poorly constrained (due to smaller number of stations and larger inter-station distances), we do an event selection based on our own location information after automatic picking:

We used the origin-time of 2,639 local events from national catalogs (now in appendix A of the manuscript) to start the automatic picking with (the automatic picking was not skipped). After the automatic procedure we saw that lots of events are either on the periphery of the network or too weak or too noisy to be detected by more than 5 stations. Moreover, automatic procedure ignored some of the clear picks in the nearest stations and had some suspected picks in the farthest stations. Therefore, we decided to manually/visually check the picks (semi-automated picking). For this purpose, we selected the events with gap<200 and RMS <1s (not too conservative) for further consideration. We explain later in the text that from these 384 events, some are blasts, and some are unclear to us, and some with very small numbers of picks.

(5) Line 140: Should be "..and thus is easily mispicked."

Done

(6) Line 175: It is not clear to me how station terms can account for 3-D variations in velocity structure that are not considered in the inversion for 1-D velocity structure.

We think our formulation was misleading. Therefore, we rephrased it as following:

"Moreover, the model **m** comprises station–corrections for P and S waves (τ_P and τ_S), which account for travel-time effects (delayed or earlier arrivals) due to deviations of the 1–D model from the real 3–D velocity structure in the shallow subsurface beneath the stations."

(7) Line 188: "...for a very large..." - should be "...a very large...".

Done

(8) Lines 188-193: Perhaps I misunderstand something, but the velocity model is defined by a series of horizontal layers, each with constant Vp and Vp/Vs? So why does a 3-D Voronoi mesh with 1 km spacing vertically and horizontally come into it? While it may be technically correct to use this terminology, isn't it less confusing to describe this as a regular mesh in 3-D with 1 km spacing? Also, is there any need to correct for Earth's sphericity, since I believe that the Podvin and Lecomte method is in Cartesian coordinates?

For efficiency, we use a fast 2-D Eikonal solver to calculate the travel times. Therefore, we do not use a 3-D mesh but indeed a 2-D mesh with 1x1 km grid node spacing (vertically and horizontally). The irregular 1-D velocity model (defined by the set of Vp_i, Vp/Vs_i) is converted into this fine 2-D mesh by assigning the velocity value of the nearest model node (Vp_i or Vp/Vs_i,

respectively) to the fine grid nodes (which is in fact some kind of Voronoi cell). Nevertheless, in order to avoid confusion, we modified the sentence to:
"Therefore, the irregular velocity model is converted to a fine and uniform mesh by setting the velocity at each mesh point to the value of the nearest point from the irregular model (Vp_i or Vp/Vs_i, respectively). The fine mesh used by the Eikonal solver has a cell spacing of 1 km vertically and horizontally. "
Based on similar earlier studies, the dimension of our network seems to be small enough to neglect the sphericity, and similar inversion codes for local earthquakes (Velest; Kissling et al., 1994) or simul2000 (Thurber 1977) make this simplification as well (and numerous studies with similar-sized networks)
The actual model is a one-dimensional one, i.e., a set of n layers with constant P-velocities and Vp/Vs ratios. The model is described as a system of equivalent one-dimensional Voronoi cells (=layers). These Voronoi cells are degenerated: typically, Voronoi cells are 2- or 3-dimensional objects.

(9) Lines 193-195: This is perhaps slightly confusing, because the first part seems to indicate an L1 measure of misfit, but with a Gaussian likelihood function, the actual misfit would be L2.
For the Markov Chain method, we used the L2 norm and corrected this in the manuscript accordingly:
"a misfit function, particularly for each model, is defined as the summed squared differences between the observed (d) and calculated travel-times."

(10) Line 215: It would be interesting to have some numbers on how many iterations constitute the burn-in phase, and how many subsequent iterations were used to build the posterior PDF.
In this section (Method), we only explain the methodology and how the inversion works. We provide these numbers later in the text. As the burn-in phase and also the number of iterations is data-driven, these statistics are shown in figures 8 and 9a for the real data and also in the corresponding text. In particular, we used ~15,000 of the final models (every 1000th of all models after the burn-in phase) for the calculation of the posterior PDF; we describe this in Section 6.

(11) Line 263: Should be "earthquake", not "earthquakes".
Agree - changed

(11) Line 273: I find it interesting that the Vp uncertainty is almost zero in the 0-20 km depth range, which the authors put down to dense ray coverage. What values do these uncertainty estimates take, and are they comparable with, say, the standard deviation of the lateral heterogeneity of the synthetic model input at that depth?
The velocity uncertainty can depend on both the data (ray coverage and pick errors) and the lateral heterogeneity. A comparison between the lateral heterogeneity of the synthetic model and velocity uncertainty shows that they follow almost a similar pattern, although, not exactly equal. The lateral heterogeneity of the synthetic model is ~0.05 km/s between 2 and 20 km depth. It varies between 0.1 and 0.5 km/s below 20 km. These values are now added to the text.

(12) Line 284: Should be "Results and discussion".
Done

(13) Line 288: What happens if all model unknowns are allowed to vary in the initial tranche of iterations?

Actually, there is no difference in the final models (Vp, Vp/Vs, quake locations, etc.) when running the inversion with or without the “first phase” (where we do not change the initial velocity model but only quake locations). When using no “first phase” the run-time (CPU time) is only significantly increased. So, we introduced the first phase for practical reasons to accelerate the computation. Note that we start our Markov chains with completely random velocity models and initial quake locations, i.e., these initial values are potentially “very far away” from the final results, spanning a very wide range. Keeping the initial velocity models fixed during the “first phase” moves the initial quake locations to their approximate epicenters, thus accelerates the inversion.

(14) Line 319: Should be “A detailed interpretation of the pattern of corrections. . ..”
Done

(15) Line 323: Should be “Estimation of hypocenter accuracy. . ..”
Done

(16) Section 6.3: This section is essentially fine, although with a relatively modest database of 344 earthquakes, it is not entirely clear what new insights are brought to the table beyond what might be gleaned from national catalogues that have been accumulated over periods of decades and involve many more earthquakes (albeit not as well located). To some extent this brings us back to the question of trying to use auto-detection methods that take advantage of this large array to find potentially large numbers of small earthquakes missed by the national agencies.
Our database is mainly aimed for high location accuracy of the occurring earthquakes and not focusing on relatively small earthquakes (with only a few observations). Especially the focal depth-estimates are very sensitive to the quantity and quality of the picks and the velocity model. Furthermore, we intend to use this dataset for the calculation of the Local Earthquake Tomography (now this is emphasized more frequently in the manuscript). Therefore, instead of creating a comprehensive catalog, we focused on having a selection of the most consistent and precise hypocenters. We can also add that the microseismicity of this region is being further studied by other groups within the research project.
We think that the earthquake locations that we established in this study have the highest precision in the region (at this time) thus enables us to interpret for example the depths of the earthquakes at the southern Alpine front (section 6.3).

********Answers to Referee #2 – Prof. Edi Kissling ********

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.

We fully agree that for an in-depth seismotectonic study, focal mechanisms are essential. This issue was covered in other projects within the SPP. A manuscript dealing with focal mechanism was submitted to Solid Earth (Petersen et al., 2021), which we now reference in our manuscript. We also added some more information about the mechanisms in some sub-regions. Please see our response below.

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 general pattern of the seismicity agrees with previous studies, e.g., Reiter et al., 2018, which we mentioned in the section “Discussion” of the manuscript. Furthermore, in the discussion section we compare the seismicity distribution of our study with those from previous studies in several sub-regions (based on local networks or national agency data). In the updated manuscript we added some sentences regarding the similarity of the seismicity pattern to long-term seismic catalogs such as the SHARE catalog as well.

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.

We agree with your comment regarding the internal consistency of our hypocenter solution and the ultimate goal of creating this dataset and therefore, we added the following text to the section “introduction” of the updated manuscript:

“The dense, high-quality travel-time picks created in this study potentially lead to constrained hypocenter solutions with high internal consistency. This will enable us to identify the general pattern of seismicity on the surface and at depth throughout the region and contribute to the understanding of active tectonic processes. A further aim of the study is to derive a high-quality dataset suitable to be used in Local Earthquake Tomography (LET).”

A similar text is in the updated section “conclusion”.

We totally agree that the study provides a very well-suited dataset for further studies such as local earthquake tomography. We emphasize this aspect now more clearly throughout the text. In chapter 5 we intended to conduct a synthetic recovery test. With this test, we can study how (well) the synthetic hypocenters are recovered by the inversion routine and how the 1-D velocity (output) looks like the 3-D model (input). Also, how shallow velocity anomalies are “mapped” by the station-corrections and whether the input random noise is recovered by the McMC method. We think that this is an interesting test for showing the performance of the method, especially when keeping in mind that we apply a relatively novel McMC search. We report the outcome of this test (please see also our response to your point number 17). Similar tests are regularly employed in the inversions for (3-D) subsurface structure (i.e., tomography). Therefore, we decided to keep chapter 5 in the main text of the manuscript. Nevertheless, we modified the text in the updated manuscript and hope that the reason behind this test is now better clarified.

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).

In section 3.2 of the updated manuscript, we added a table and histograms (table 1 and figure 4) regarding the results (travel-time dataset) of the semi-automatic picking procedure. Further detail about the event list from national catalogs is now added to the updated manuscript as a new appendix A and it is referenced in section 3.2.

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).

Our intention is to keep the conclusion (and in general the manuscript) more technical than interpretational (in terms of seismotectonic). However, we rephrased the whole conclusion in order to have a reasonable balance between all chapters of the manuscript. In this regard, we rephrased the abstract as well so that our main aim of the manuscript is better conceived.

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

Specific points:

Below are our responses to your 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.

Agree – changed

(2) Line 15. Delete the rest of the sentence after:“ ..1.7 km in depth.“

Done

(3) Line 27. Replace "accuracy“ with "precision“
Agree – changed

(4) Line 48. "... has the advantage of being“ largely "independent …“
Done

(5) Line 51. "best model(s).“ a note on ambiguity would be useful
This part is rephrased to:
“Moreover, the results can be statistically analyzed, and thus errors and ambiguities can be estimated. The method extends the probabilistic relocation approaches (Lomax et al., 2000) by inverting for a set of velocity models well explaining the data. “
However, more details are provided in the method chapter of the manuscript

(6) Line 64. Please outline Adriatic microplate in one of the Figures.
Done

(7) Lines 67-75. Needs a figure to show the strain if you keep the introduction as is and the chapter 6.3.
We think that we have already provided all references to our statements regarding the strain (mainly in section 2). Nevertheless, now the companion paper by Verwater et al. 2021 is submitted (and accessible) in which this topic (including a Figure) is covered in more detail. Therefore, we prefer not to add an additional Figure to our paper. We add a reference to Verwater et al. 2021 to Section 2.

(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.
The figure bounds are corrected.
All the information which is mentioned in the text is now added to figure 1.
The big picture of seismicity (Figure 1) is now taken from EMSC with M3.

(9) Line 85. Actually there are earlier catalogs that were compiled: European Geotraverse Blundell et al. 1992, Solarino et al. 1997
Agree – two references are inserted in the text.

(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.
An appendix A with the information regarding how the event list is formed is added to the updated manuscript.

(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?

We used the origin-time of 2,639 local events from national catalogs (now in appendix A) as initial data to start the automatic picking with. However, we don't have any information about the precision of these events beforehand. Therefore, in the beginning, we decided to select as many events as possible from the national catalogs, apply an automatic picking procedure for these events, and then make a further selection based on our own picks and location information. After the automatic procedure (2,639 events, 68,099 P- and 17,151 S-Picks), we noticed that many of the events are either on the periphery of the network, too weak, or too noisy to be detected by more than 5 stations. Moreover, we believe that the automatic picker missed some of the good picks or introduced some suspected picks. Therefore, we had an early selection criterion (not too conservative) in order to do a manual/visual inspection of the picks. These selection criteria (gap<200 and RMS <1s which gives 384 events with 18,390 P- and 7,762 S-picks) were applied to the automatic picking results. We manually/visually inspected the P and S picks of these 384 events, with careful consideration for picks in the distances of the triplication zone and farther than that (we inspected a very large amount of the picks from these 384 events, maybe 90% of them). We explain later in text that from these 384 events, some are blasts, and some are unclear to us. Later on, this dataset was further selected for simultaneous inversion (301 local earthquakes with gap<180° and minimum 10 P-picks and 5 S-picks).

We added the statistics about the number of picks and their quality classes of 2,639 events after automatic picking to Appendix B (Table B1) of the updated manuscript. Detailed information of the selected dataset for the simultaneous inversion is now given in the main text (section 3.2; table 1 and figure 4) of the updated manuscript.

(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?

We agree that the points are systematically shifted to above the straight line indicating that the Vp/Vs ratio (slightly) increases at larger distances. This could be related to different (average) Vp/Vs ratios in the crust and upper mantle (which is e.g., also suggested by global models such as ak135). We mention this now in the text: "We notice that at P travel-times larger than ~25 s the observations tend to slightly larger S-P travel-time differences, potentially indicating a higher Vp/Vs ratio at larger depth, i.e., in the upper mantle."

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 expect for the region (e.g., Vigano et al.,2015) and generally from 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. We would like to emphasize that we only considered values with $t_s - t_p > 0.72\ t_p \pm 4$s as "outliers" and removed them. These outliers were only 0.3% of all observations (which should not make a difference even if they were incorrectly flagged as "outliers"). We state this now more clearly in the text. After this removal, only 2% of the whole observations have ts – tp > 0.72 tp±3 s.

(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).
We agree. The comparison of the cumulated picks (from earthquakes with a variety of different earthquake depths) with the bounds of the synthetic traveltime curves (from a global model for different earthquake depths) was too coarse, too unspecific, and obviously not helpful. As we mentioned in the text (both in section 3.2 and in the appendix), we visually/manually inspected the waveforms of all earthquakes and their automatically determined arrival-times, and added, deleted, or modified picks where applicable. This inspection was done mainly on individual traces and was most helpful for identifying wrongly picked noise burst etc. Arrivals of stronger events, for which we expect XmX or Xn arrivals, were additionally assessed with the help of record sections (epidistance plots of individual earthquakes; based on preliminary locations). So, we are quite confident, that we identified most of the phases correctly. Nevertheless, we acknowledge - and are very much aware of - that even these detailed visual/manual checks cannot prevent some amount of wrongly identified picks in our dataset (a general difficulty for manual picks e.g., already pointed out by Diehl et al., 2009). We state this now more clearly in the text. In this context, we think that Figure 4 of the original manuscript is indeed not helpful for the manuscript and thus we decided to delete it, as we deleted parts of the text of section 3.3 in the original manuscript (some parts were moved to the newly formed section 3.2). In any case, we would like to emphasize that we never used Figure 4 for identifying and removing individual picks (potentially misidentified).

(14) Line 152. Please provide clear evidence and explain in detail strategy to identify PmP phase by using a totally inadequat 1D model.
We agree, based on this plot we are not able to do this. We deleted this statement (together with the whole paragraph) – please see our comment above.

(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.
Yes, we agree on this point. Our discussion of "outliers" was not specific enough. Similar to the phase-identification issue (see our comment above), we are very much aware that our dataset probably still contains some number of "outliers" (howsoever defined) and we stated this now more clearly. However, as we also state above, we did not use Figure 4 or any related (automatic) mechanism for identifying and removing individual picks (potentially misidentified). We have just used it as a – probably too simple – statement regarding the existence or non-existence of "outliers".
As stated above, we deleted the whole corresponding paragraph (and Figure 4) and modified the text.

(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.

We only partly agree. We are still convinced, that our inversion method itself does not depend on the initial hypocenters (as is the case for the traditional inversion routines using a linearized approach, DLSQ inversion, etc.), see Fig. 8 in Ryberg & Haberland, 2019. However, we agree that some dependency might be introduced through the selection of picks (e.g., “outlier removal”), especially in the case of automatic picking (involving e.g., sequential intermediate location steps based on initial velocity models). Nevertheless, we think that even this influence is minimized because we are using a visually inspected pick-dataset.
We modified the part in the following way:
“Therefore, the McMC method only uses the travel times and does not directly depend on initial hypocenters, origin times, velocity models, or even the model parametrization (e.g., grid node spacing). Nevertheless, because in the (semi-automatic) picking procedure the selection of picks is involving the comparison with travel times based on preliminary hypocenters (which depend on initial velocity models), the results of the inversion depend *stricto sensu* to some degree on initial values.”

(17) Line 226. This does not provide an "accuracy“ estimate! May be internal consistency, precision.
Agree - It is changed to consistency.
And you need to provide reasons why this should be expected to be of relevance for the real individual
event locations.
The event locations (and the velocity model) derived by the inversion procedure depend on a large number of parameters such as the quantity and spatial distribution of earthquakes and receivers, noise, quality of travel-time readings, the class of the model used (in the inversion) and so forth. Furthermore, the test is not only interesting in respect to the hypocenters but also to the derived velocity model. We think that it is appropriate to test the recovery by synthetic tests as it is standard for the recovery of subsurface structure (e.g., in LET studies). Since the McMC approach for solving the coupled hypocenter-(1-D) velocity problem is quite novel we think that this kind of test is particularly interesting. We already described our reasoning for conducting the test in section 5, however, we added/modified the text slightly. Furthermore, we added some sentences to the discussion of the results of the test.
“Comparing input event locations (synthetic) and the inverted (output) ones allows us to study the recovery of the hypocenters, location consistency, and potential systematic errors related to the use of a 1-D model, which we can generally expect for the derived real hypocenters. For example, it can be studied whether events at the periphery or in certain parts of the model have systematically larger uncertainties (e.g., due to their location and/or spatial distribution of picks). 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. Similar tests are standard in structural studies (i.e., LET) to study the recovery of certain features. “
By the way, your blast test shows otherwise!
We think that the simultaneous/joint hypocenter relocation is a very powerful concept. We agree that our blast test indeed shows very good performance of this test.

(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.
While the reviewer is certainly right regarding the reported seismic velocity values in the Alps, we would like to point out that this is a synthetic test with the main aim of checking whether and to which extent a 3-D velocity variation (i.e., Moho topography and shallow velocity variations)

influences the recovery of the hypocenters (and which 1-D model is derived). We think that the exact values of the velocities are not that important for this test as long as they are in the "usual" range. We used also for example an angular shape of the Tauern Window and the sedimentary basins which is also not close to reality. We never claimed that these are reported values, it's a description of our synthetic model. Therefore, we prefer to leave the synthetic model as it is. Nevertheless (and in response to issues 17 – 21), we modified the whole paragraph describing the synthetic model.

(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.
Agree - changed

(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.
During the McMC inversion many different – also weird and unrealistic - velocity values, i.e., also models with a velocity increase with elevation, are (randomly) tested. This is totally different e.g., compared to velest. We are very much aware of the fact that usually on Earth there is a velocity increase with depth. These unrealistic models obviously have to be suppressed because otherwise, we obtain a somehow "mirrored" (artificial) velocity model above the surface (in our case, only models without low-velocity zones are accepted). Furthermore, the method does not use rays (or raytracing) at all but instead calculates the travel time field with an FD Eikonal solver. More information can be found in Ryberg & Haberland (2019) (exact reference in the bibliography of the manuscript).

(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.
This is a synthetic test, and the goal is mainly to assess the performance of the method to recover the earthquake locations when using a simplified 1-D velocity model in the inversion. We agree that the reality is more complex and maybe our test is not complete and might not cover all aspects which should be checked for. In Ryberg & Haberland (2019) a much simpler recovery test with a 1-D synthetic model was performed, now we wanted to add some more complexity (although not claiming that this is the ultimate test). We modified the section describing the synthetic model (please see also our response to issues 17 – 20 above and 24 below).

(22) Line 250/1. Not only refer to table but provide correct value here.
Done

(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.
Done

(24) Figure 5b. The Moho topography is wrong. There is a Moho offset across the plate boundary but you show a vertical Moho interface!

Yes, the reviewer is right, the Moho shown in the Figure and used in our synthetic test is only a very simplified version of the Spada et al. 2013 Moho (Moho offset, double Moho, etc. are not reproduced). Furthermore, the crustal structure is very much simplified. We state this now more clearly in the text. However, we still think that simplifications for the synthetic test model are acceptable, and a simplified synthetic model is still useful for studying the recovery of some features (hypocenters, 1-D model).
Please see also our comments on similar issues 18 and 21.

(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?
In the previous paragraph, it is explained that the model parameters are defined based on the average and standard deviation of the final models. In the old version of the manuscript, we showed in figure 6b the histograms of the uncertainties (1σ) of all earthquakes. However, for avoiding any misunderstanding, we decided to not mention these uncertainties here and only talk about the misfits which are important for recovery assessment.

(26) Figure 7. Move to supplementary material.
We think figure 7a of the original manuscript is quite important to display in the main text because it is the best information showing the recovery of the synthetic test. Therefore, we removed figure 6b (not as important as Figure 7) and merged figures 6a and 7 into one figure (in the updated manuscript it is figure 6).

(27) Figure 8. Again a DIFFERENT STUDY REGION SHOWN!.
The figure frame is now set correctly.
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?
There are stations that have very small delays and it’s probably hard to see them (they are like a point in the figure). The stations with zero delay are indicated now with a different symbol in the updated manuscript. There are also some stations without any delays, which were considered in the early part of the study but not included in the final inversion (e.g., no high-quality picks, station problems, etc.). However, all the stations to the west of Tauern Window have symbols (comparison between figures 2 and 7b of the updated manuscript).
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.
We totally agree that resolution typically degrades below the (deepest) earthquakes. However, we would like to point out that the synthetic (3-D) velocity model in the depth range between ~30 and ~55 km is characterized by a strong Moho topography, which *per se* cannot be exactly recovered by a 1-D model (used in our inversion). We think that is not (only) an effect of the data (less ray coverage and resolution below the deepest earthquakes) but also of the inability of a 1-D model to capture a 3-D variation. So, the input model is indeed not well resolved in this depth range, but it is most likely an effect of the data and the much simpler model class used in the inversion. Within the model complexity (in each depth level) the input model is recovered in average (with some variability indicated by the standard deviations). Of course, upper mantle velocities are reached at a large depth. We think this is a nice result of our test which also sheds

light on the interpretability of such 1-D models. We modified the text and hope that this issue is somewhat clearer.

(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.
We agree. For example, our model is relatively close to the Diehl et al. (2009) model in the upper part, however, at larger depth it is different. We describe this now better. Furthermore, the resolution of our model is only good down to about 45km depth, below that the standard deviations (1 sigma) are getting significantly larger indicating fading resolution. Also, this aspect is now described in more detail in the updated manuscript.

(29) Line 320. "it proved to be useful for accurately localizing earthquakes ..“. I am missing the prove. Please explain how this was proved.
Our phrasing was obviously misleading. We wanted to add a more general statement that - although it might be difficult to interpret the station corrections in a straightforward (“physical”) way - the whole concept of the simultaneous /joint inversion (including the inversion for these station corrections) is very powerful. We reformulated this part in the text to:
“Nevertheless, the general concept of simultaneously inverting local earthquake datasets for a (simplified) 1-D velocity model, hypocenter positions/origin times, and station corrections proved to be very powerful for accurately localizing earthquakes (see e.g., Kissling,1988).”

(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.
The resolution for Vp is starting to fade below 30km depth, however, standard deviations of around 0.02 in the depth range between 30 and 40 km (yellow ranges in Figure 9 of the updated manuscript) still indicate fair resolution. For Vp/Vs, we see this limit at around 30 km. We write this now in the text.
Figure 10c. Now this looks like the study region. Why not always marking this extent where you do have data from?
We homogenized the lat/long in all the maps.
How do you interprete the distribution of the station delays?
We discussed the pattern of station corrections and possible qualitative relations with geological features in lines 313 – 318 of the original manuscript.
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.
Yes, the reviewer points out a totally valid point, thanks. We checked the data and identified this station as having wrong picks. Since we generally have to expect influences of these wrong picks on all other parameters in our simultaneous inversion (velocity model, other station corrections, and hypocenters), we removed all picks of this station, reran the simultaneous inversion, and located again all events with the updated velocity model. So, Figure 9 (of the updated manuscript) shows now the updated data. Please note that the changes in the hypocenters and station corrections were very marginal, and all inferences in the paper are not changed.

(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.
We appreciate this question. It motivated us to calculate the misfit vectors for the blasts similar to Husen et al. (1999) using the centers of the quarry areas (since we do not have the exact blast locations). We, therefore, added the following text to the manuscript:
"Based on the average mislocations of the blasts (relative to the centers of the quarry areas) and also their location uncertainties, we estimate the absolute location errors in the range of 1 km horizontally and 500 m vertically."

(32) Line 331. Please explain in theory why you suggest that including S observations improve the hypocenter location solution?
According to Gomberg et al. (1990) S-picks provide powerful additional constraints on hypocentre location, especially regarding the focal depth. They demonstrated that the partial derivatives of S-travel-times are larger than for P-waves and that they act as a unique constraint especially in cases of S-picks recorded within 1.4 focal depth's distance. However, precisely measuring the S phases is absolutely necessary and we carefully and conservatively identified the first arriving S-picks on the 3C high-quality recordings. Nevertheless, we think that our statement in Line 331 (of original manuscript) was not really precise and we decided to delete the statement regarding the S-picks. We replaced it by:
"It is expected that the errors of earthquakes (which are potentially deeper than blasts) are smaller than those estimated from this test because they are less affected by the heterogeneous shallow structure which was poorly accounted for by the model (Kissling 1988 & Husen et al., 1999)."

(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.
The location uncertainties that we report here are the standard deviations from the mean value of all the models of McMC. We provided now new estimates for the mislocation (based on quarry blasts test; see point 31) that we can now mention here as well. We rephrased the sentence to:
"Based on statistical analyses of the McMC inversion results, the average epicentral and depth uncertainties (1σ) are ~500 m and ~1.7 km which are compatible with precision estimates by the synthetic test (Sect. 5). However, the absolute location errors estimated by quarry blasts test (Sect. 6.2), are 1 km horizontally and 500 km vertically. "

(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.
Our intention was not to show poor hypocenter solutions from the national agencies. Most of the large differences between location of the blasts reported by the agencies and our solutions

(and the mislocations) are indeed related to the amount of data (stations). 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 2.4 km in epicenter and 3.7 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."

(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.
We agree and delete this part of the figure. We now mention the uncertainty values in the text.

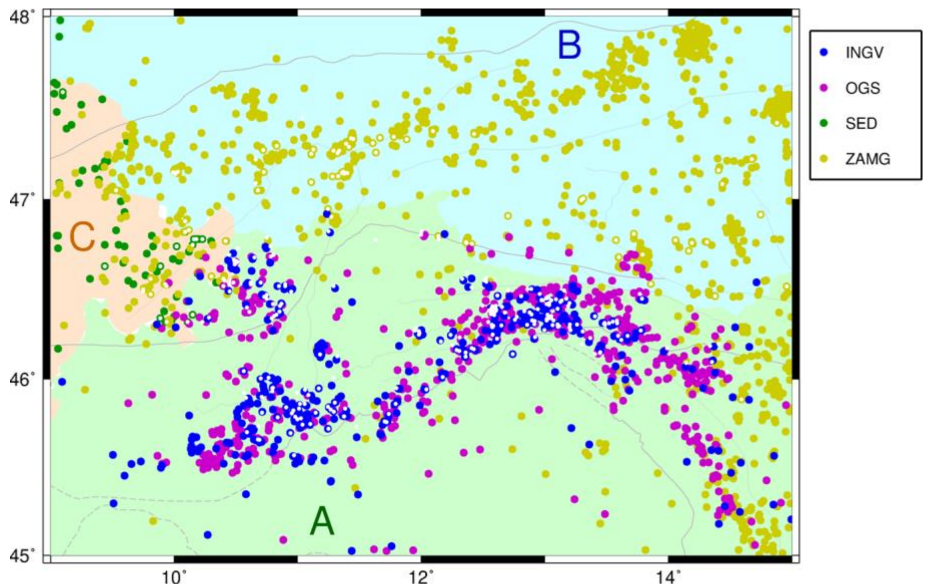
(36) Figure 13. For seismotectonic interpretation of clusters along a fault system, you should definitely
employ focal mechanisms.
We totally agree. In the discussion, we wanted to compare our derived seismicity, which is now captured in a consistent way throughout the study area with a dense network, with the seismicity known from other studies and datasets (e.g., from permanent, though coarse, networks running for many years). In this context we also discussed some occurrences along fault zones, however, we did not go into too much detail here. We are aware that for a conclusive investigation, focal mechanisms as well as a more complete catalog is necessary. Within the research program, more studies are underway focusing on exactly these issues. In the meanwhile, a manuscript (Petersen et al., 2021) was submitted (to SE) showing focal mechanisms from the AlpArray and SWATH-D networks. We refer to this study now in the text, however, results were not available at the time of initial submission of our manuscript.

********Answers to Referee #3 ********

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.