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 #1
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Dear Referee,

We would like to thank you for your insightful comments on the manuscript. We believe that your comments are well justified and thanks for your positive and encouraging review. In the following, we provide our responses to the remarks raised by you as blue text.

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 locatable (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).

On behalf of the authors,
Azam Jozi Najafabadi