Solid Earth Discussions

Solid Earth Discuss., referee comment RC3
https://doi.org/10.5194/se-2021-13-RC3, 2021

Comment on se-2021-13

Anonymous Referee #3

Referee comment on "Regional centroid moment tensor inversion of small to moderate earthquakes in the Alps using the dense AlpArray seismic network: challenges and seismotectonic insights" by Gesa Maria Petersen et al., Solid Earth Discuss., https://doi.org/10.5194/se-2021-13-RC3, 2021

General comments :

In this paper, the authors provide MT inversions homogeneously performed on the whole European Alpine belt thanks to the Alp Array initiative and corresponding dense seismic networks (AASN). They rely on the seismicity recorded during 4 years at ~600 stations to derive 75 moment tensors of associated local magnitude ranging between 3.1 and 4.8. In the first part of their manuscript, they present a set of methodological tests in order to define the best input datatypes and the best parameters to constrain their moment tensor inversions. These methodological tests moreover allow them to establish moment tensor inversion guidelines for areas of similar tectonic context and of small to moderate seismicity. The second part of their manuscript very briefly describes the 75 well constrained moment tensors retrieved from applying the aforementioned guidelines to the Alpine/Apenninic areas over the 2016-2019 AA seismic data. The last part of the manuscript deals with the representativity of the patterns they identify in the seismicity (focal depths, seismic clusters) and in the tectonic regimes of the Alpine belt and surrounding areas with regards to seismotectonic, geodetic and historical contexts.

The paper is well written, and mostly well organized, which makes it fluent and pleasant to read, even if the different parts could be better balanced (see below and following specific comments). I highly appreciated the methodological testing section, which delivers several conclusions which will certainly be of interest to a broad community of both seismologists and tectonicians (minimum station coverage required depending on geological clues, min and max epicentral distances, contribution of tensile faulting). They indeed apply a very thorough and rigorous analysis of the potential biases associated to either the dataset, the network or the inversion parameters. These tests allow the authors to estimate the % of non-DC components in their MTs, as well as their possible meaning, and to assess the resolution of the DC components. The very first part of this section could be condensed

though (see specific comments below). Section 3 presents some very interesting results, that the reader may wish to see deepened and better highlighted. The discussion part (section 4) however mixes up some aspects which in my view represent new computations and result descriptions (families identification through clustering, depth distribution) with analysis of the results and comparisons with other studies, which makes the take-home message difficult to grab. I suggest reorganizing sections 3 and 4 as specified below. Most importantly, the results lack of interpretation and would benefit from being put into their broader tectonic and geodynamic contexts. An attempt to do so is sketched in section 4 but this should be extended (i.e. present-day plate kinematics and limits, crustal units, Moho depths...see specific comments). Similarly the discussion would benefit from a more detailed comparison of the main findings with previous studies, and lacks of references to recent Alpine large scale studies.

Lastly, I recommend rewriting the conclusions to better highlight the major findings of the study and main contributions brought to the seismological and seismotectonic communities, as well as the potential applicability to other tectonic domains/study areas. I leave to the editor to appreciate whether the paper requires minor or moderate revision based on the following specific comments. In any case, I strongly believe that this paper will make a valuable contribution to the Alpine -and possibly broader- community after a few improvements.

Specific comments:

-The introduction is well written and exhaustive. Referring to Figure 1 sooner (§3) would help the reader when describing the various predominant tectonic regimes throughout the belt though.

-I would suggest making subsection 2.1 more synthetic. For example, keep in pages 7 and 8 only the details about the choices you made, especially since you provide detailed comparisons of input data types and various combinations in section 2.3.

-section 2.2 presents some nice methodological conclusions, that should be emphasized as stated l.208 : why not summarize these guidelines at the end of the conclusions of the paper, in order to open the discussion towards applicability to other similar contexts ? For example stating that for the specific Alpine context or for other densely instrumented low seismicity areas, DC component is well resolved whether allowing for a CLVD or isotropic component or not as mentioned l.236., or that a small number of stations/small azimuthal coverage may be sufficient depending on their orientation wrt to the strike of the fault as stated section 2.2.4, or that CLVD and isotropic components cannot be distinguished reliably.

-section 2.2.1 :paragraphs between l.226-252 would better fit before Figure 3 and its corresponding §, since they give important explanations to understand the tests that are implemented, and since the last § of page 11 is the direct continuation of the first two § of this subsection.

-l.298 : what are the 8 combinations mentioned ? Only 6 are presented in Figure 7. Are the other two envelopes-td and envelopes-cc combinations ? This is confusing. As the latter two are only presented in the supplement I would not mention those here.

-l.306 : « low uncertainties » : how much ? It could help the reader to add the uncertainty color scale to Figure 7 at least.

-l.401 : « draw a more detailed picture of the seismic activity in this study area » : what are these new detailed features compared to the literature in the end? The new characteristics that could be derived thanks to this study should be emphasized in the conclusions, rather than summarized as was already done in the abstract.

- sections 4.1 and 4.2 are merely discussion sections since new results are presented here from applying various clustering algorithms, and especially since no interpretation/analysis is done of the described features concerning tectonic regimes and depth distributions. No explanation is provided for the similarities or discrepancies that are highlighted between the different areas. I don't find any comparison to previous study here either. I would suggest moving these subsections into the result section 3.

-Figure 10 would be clearer if grouping stereograms by family...maybe try plotting a single stereo for each family identified through the clustering algorithm ? You describe a rotation of the P-axes in the SE Alps, is it a new feature or was it already observed by previous studies ? If so, do you provide more constraints/increased resolutions on the corresponding orientations ? How was it interpreted ?

Same question for the transition from thrusting to strike-slip in the SE Alps-Dinarides junction. I guess there is an attempt to do so afterwards l.481-484 but I'm not sure whether it refers to the same feature.

l.434-438 should definitely appear in section 3 rather than here.

-If the authors wish to keep section 4.2 in the discussion, they should give more elements to discuss the observed depth distribution : how are the observed depth variations explained by the various tectonic contexts ? How does the depth distribution of the events correlate with lateral depth variations of the different Mohos ?

-In the seismotectonic context part of the introduction the hypothesis of subduction polarity reversals at the transition with Apenninic and Dinarid slabs is mentioned. Are centroid depths deeper in these places, shallower, or not systematically different from the surroundings ?

-In the caption of Figure 11 is written : « the outlines of spatial clusters of increased seismic activity from Fig12a are indicated for comparison ». Why ? Where is this comparison made in the main text ? Should we expect any correlation between higher seismicity rate clusters and depth variations ? If yes it would be valuable to give a more detailed comparison/explanation. If not, why not rather plot the outlines of the different tectonic regime families identified in Figure 9 and section 4.1 ? And, if possible, it would be helpful to also outline the different plate limits on these maps.

-section 4.3: in my view, the discussion part really starts here, but should be extended. How do the higher seismicity rate areas relate to the tectonic regime families formerly identified ? This is addressed briefly at the end of p.24 but it would be interesting to cover it in more detail. For example Figure 12a could benefit from adding a representative focal mechanism for each cluster. Or maybe overlay the stress tensors/stereos from the supplement. This would help the reader to get a more general picture of the regional seismicity and to put back the results into a broader context. This would support and extend the discussion of the different features made l.481-493. It would also be helpful to refer to the colors of the clusters Fig 12a) when analysing them in the main text (or at least display numbers from Fig1 on Fig12a). It would be easier for the reader as well to structure the discussion by bullet points for each identified family/feature (or by small paragraphs with bold header as was done in the methodology section 2.1).

The discussion would also benefit from analysing the repartition of seismicity and faulting styles in the light of the complex tectonic context mentioned in the introduction. These results could be discussed with regards to current plate kinematics (spatial variations of convergence rates, counterclockwise Adria rotation...). For example indicate current plate limits and kinematics with arrows on the maps of Fig 12.

I really appreciate the effort to compare seismicity with representative measurements of geodetic deformation (i.e. spatial gradient of uplift rate as a proxy for vertical strain). For the map of the maximum shear rate, are the results similar to those which would be obtained using the 2nd invariant of the strain tensor ?

Beachballs on Figures e) and f) are overloading the global picture. As mentioned above, I would rather display a representative mechanism, or stereo, or stress arrow on Figure 12a, and leave the sole seismicity distribution here (or even better, seismicity density, as isolines maybe), especially if the authors wish to focus the discussion on the areas presenting both higher seismic rates and higher geodetic deformation.

-l.505 : I disagree on the relation between vertical geodetic gradients and seismic activity. As stated by the authors themselves in the following paragraph, several areas show a spatial decorrelation between higher vertical gradients and seismicity (SW Alps, E Po plain).

-l.520 : representing the absolute vertical gradient indeed probably biases your comparison between tectonic uplift and seismicity occurrence. Why not instead represent positive vertical gradients only ? On the contrary the correlation between cumulative seismic moment (12d) and shear strain rates is striking and should be emphasized.

This comparison is really interesting but lacks references. How are these correlations supported by other studies (regional e.g. Serpellonni et al., 2016 https://doi.org/10.1016/j.tecto.2016.09.026, or local, e.g. Anderlini et al., 2020 https://doi.org/10.5194/se-11-1681-2020 ?

The conclusions do not present the same quality (substance and form) as the rest of the paper. They should be rewritten to participate in highlighting this fruitful study. The major findings of the paper should be more clearly emphasized, including the ones concerning the seismotectonic insights. For example it is a major finding in my opinion, which will moreover be of interest to a broad community, to note that with 4 years of acquisition of small to moderate earthquakes, the authors are able to derive seismotectonic domains which are representative in faulting style of those derived from longer term seismicity/higher magnitude events ! Maybe state it more clearly ~ l.540. The very last § of the conclusions is not appropriate here. It should appear at the end of the methodological section instead. I suggest an opening focused on the applicability of the methods to other study areas and summarizing the main guidelines for similar tectonic context in order to be able to derive reliable and representative MTs over short time span and dense networks (see suggestions in the above comment related to section 2.2).

Technical issues, English and typos :

- l.30 and in several other places : « both » not « both, »

- if possible uniformize English or American English spelling (characterised vs catalog for example)

- l.81 « data points » : polarities ? Stations ?

- Figure 1 caption : datasets) .

-l.88 and everywhere at the beginning of sentences : « Furthermore » not « Further »

-l.119 « an »

-l.126 « *Grond* »

-l.130 : « firstly » […]. Secondly, […] »

-l.133 « Grond »

-l.134 « bootstrap (BS) chain »

-Figure 2 caption : « are indicated above each column ».

- Figure 2e) : mxx instead mnn and so on ?

-l.183 « sensitive to »

-l.212 representative … in terms of what ? Magnitude range ?

-l.240 « If[…], it is however […]. »

-l.269 and in several other places « in the case of », not « in case of » + « without any station covering »

-l.275 : « one must assess the »

-l.322 : Fig. 7)

-l.367: a limited a azimuthal

-l.369 « any *a priori* »

-Figure 8 caption : « radius r given below each column ». Refer to section 2.1 and Fig 2 for the fuzzy MTs.

-Figure 9 caption : I would have expected more solutions from the 1983-2015 bulletins over the whole area : is it due to the minimum Ml 3 threshold ? Or are some MT solutions missing from example from Geoazur ?

-l.381 : « (Fig.9, Supplement Table X).

-l.393: « section 4.2 »

-l.421-422 : the two sentences are redundant.

-Figure 10 : add a), b), and c) labels on the subfigures.

-l.426 : no stresses here, but regimes or horizontal T-axes

-l.454 : « at shallower depths »

-l.461 and whenever referring to historical seismicity/events : « historical » not « historic »

-l.488 : « mechanisms. In addition »

-l. « merged catalogs » : merged by who ? How ?