Response to "Review of SE-manuscript Anderlini et al. 2020"

We are grateful for the constructive comments provided by the anonymous referee. He/she raises two main concerns, i.e. the seismic catalogs used and the fault geometry definition, which are addressed in detail below. Furthermore, specific comments to individual points of the manuscript are provided. In the following, we will repeat the referee's statements and our reply to it (in bold font).

The study regards a topic of importance and interest and the manuscript in general is well structured and well written. The study area along the southern boundary of the Italian Eastern Southern Alps (ESA) is known for a high seismic hazard in combination with a low-to-moderate plate tectonic strain that is difficult to locally pinpoint to specific faults. The paper reviews the various publications that recently documented different parts of the geodetically determined (mainly horizontal) strain and the seismic deformation rates that exhibit significant discrepancies across the ESA thrust fault system. As the title of the manuscript implies, new insight is gained by combining the existing tectonic, seismic and geodetic information with additional vertical geodetic velocity data integrated from GPS, InSAR and leveling measurements.

I congratulate the authors to a good study and manuscript. I do have only a few general points for consideration by the authors and a number of smaller issues with some of the figures that I list below. Overall, I believe the entity of these issues would require just MODERATE REVISION.

In the first of their main chapters –chapter 3 Geodetic Observations- the authors provide not only a careful evaluation and combination of InSAR and GPS observations but also a discussion of the different geodetic measurement techniques and their resulting data. This certainly denotes a very useful commented summary review of geodetic techniques also for the non-specialist readership. In the subsequent second main chapter of the paper, the authors develop a 2D fault model to represent the ESA front fault system and to interpret the horizontal and vertical strain gradients measured across the fault system. Again the modelling is characterized by careful evaluation and balanced weighting of the different geodetic data. While the model seems to generally „reproduce the observed velocity gradients from all (geodetic) data sets", „geological and geomorphological data appears not to be fully consistent" and I would also list the seismologic information as not being fully consistent. I fully agree with the conclusions by the authors, but I do believe that in correspondence with the careful evaluation and interpretations of the fit and the discrepancies (1) between different geodetic data sets and (2) between the best-fit 2D fault model and the geodetic data, an additional conclusion would not only be appropriate but necessary: we need better seismicity and better geological subsurface structure information. The former obviously does not understandably correspond with the geodetic data assembly and the latter is very poorly constrained or entirely speculative (see also comment below).

We agree with the reviewer, additional new geological (surface and subsurface) and seismological data are certainly required to better interpret the geodetic signal and we add this sentence:

“However, important constraints from new seismicity data and new surface and subsurface geological observations will be required, together with denser GNSS data, to better constrain the tectonic rates and seismogenic potential in this area.”

The seismicity shown in Figures 1 and 3 suffers from several questions regarding the consistency. (1) the time periods are not the same but do overlap between 2012 and 2017. Are you showing events from both data sets for this overlap time? (2) How to the events of the two data sets compare during this overlap time?

We thank the reviewer for this comment that gives us the opportunity to fix an issue that can generate confusion in the readers. We agree that the use of two seismic catalogues, with spatial and temporal overlaps is misleading. We intended to use the two catalogues for illustration purposes, but we acknowledge that this is causing confusion.
Yes, there is overlap between the two catalogues, and the events present in both catalogues (for the overlapping period, 2012-2017) are plotted twice, in slightly different locations (and different colors). The intent of the figure was to acknowledge the presence of this high-resolution seismic catalogue, presented in a recent work (Romano et al., 2019) that has been used to provide a “Portrait of the Montello Thrust”. We do not aim at any quantitative comparisons between seismic and geodetic strain rates. We have modified figures 1 and 3, plotting only the OGS bulletin data in Fig. 1, to give a regional overview of the spatial distribution of instrumental seismicity from 2000, and the high-resolution catalogue from Romano et al. (2019), in the 2012-2017 time-interval, in Fig. 3, for comparison with subsurface structures, focal mechanisms and model geometry.

(3) How comes much fewer events yellow-red are seen within broken line box in Fig. 1 than visible in cross section in Fig. 3? (4) Where does the obvious cluster seen in Fig. 3 at 10km depth between 45km and 65km profile distance locate in Fig1 map view?

The seismicity overlaps in Fig. 1 and in Fig. 3, but the symbols (scaled with magnitude) in Fig. 1 are much smaller than in Fig. 3. The two figures are now re-drawn considering this comment and the previous one.

(5) Why do you project the seismicity across a band 50km wide onto a profile where you project the geodetic data only across a band 20km wide?

The GPS velocities are projected along a 35 km wide profile (see Section 4), not 20, and the same is done for seismicity, for which we plotted the earthquakes in the same swath profile. We have re-drawn this figure considering only Romano et al. (2019) earthquakes. The dashed boxes in Fig. 2 and Fig. 3 are the same, and used to project GPS velocities and plot instrumental seismicity. We removed the dashed box in Fig. 1 that could generate confusion.

How do the fault plane solutions compare with each other, with the geologic fault geometries (Fig. 3) and with your final model (Fig. 7)?

In general we see some correlation between thrust faulting mechanisms and geological structures. The focal mechanisms plotted in this figure come from two local and temporary experiments (Anselmi et al. 2011 and Danesi et al. 2015). We acknowledge, and better state in the Discussion section of the manuscript, that the hypocentral depths suffer for the use of different seismic velocity models. In particular, low-dipping thrust events on the Montello flat (from Danesi et al., 2015) are considered reliable, and used to constraint the depth of the decollement in the dislocation model.

In Figure 3 you show the subsurface geometries of the ESA frontal fault system and as reference you refer in Figure caption and in text (lines 255 and 267) to Castellarin et al. 2006 („The gray continuous and dashed lines represent major and secondary faults digitized from the TRANSALP profile interpretation (modified from Castellarin et al., 2006). MT = Montello thrust, MBT = Montello backthrust, BV = Bassano–Valdobbiadene thrust, BL = Belluno thrust, VS = Valsugana thrust. “). In Castellarin et al. 2006 Fig. 7 is showing a geologic interpretation of vibroseismic image (down to 5km) and in Fig.8 a geologic interpretation of seismic time section again down to 5km and these figures document a profile that runs across the Montello Hill within the study region of this paper. These high-resolution seismic images and their geologic interpretations significantly differ in even the most prominent geometries with the interpretation shown in Fig. 3 of this study.

In figure 7 and 8 of Castellarin et al., 2006 the vertical scale is in seconds (5 s) probably TWT but it is not specified, therefore we suppose the sections presented are not depth migrated. In this case it is difficult to retrieve from these figures the exact geometry of the drawn fault lines, since the rocks involved are quite different from metamorphic to carbonatic and sandstone units characterized by a large variability in seismic waves velocities (ca 3-8 km/s). Differently, Figure 11 of Castellarin et al., 2006 presents a simplified general interpretation of the TRANSALP profile (TRANSALP Working Group, 2002), it is depth migrated, and thus it provides a real interpreted geometry for the tectonic structures presented even if with less details.

Rather, in Castellarin et al. 2006 Fig. 11 (see included figure below) entitled „simplified general interpretation of the TRANSALP profile there is shown a major detachment fault system separating and translating the imbricated upper crust at about 10km-12km depth from the middle and lower continental crust of Adria. It is the geometry of these fault

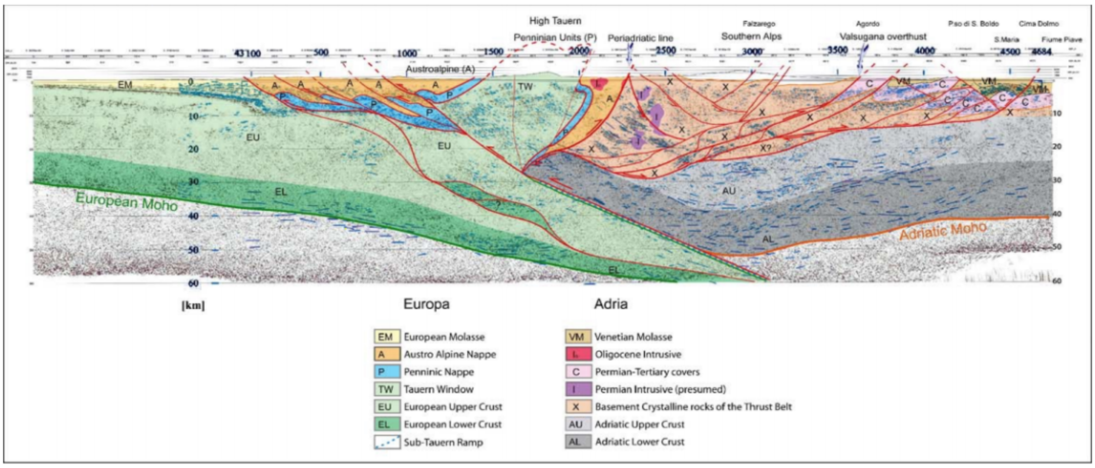
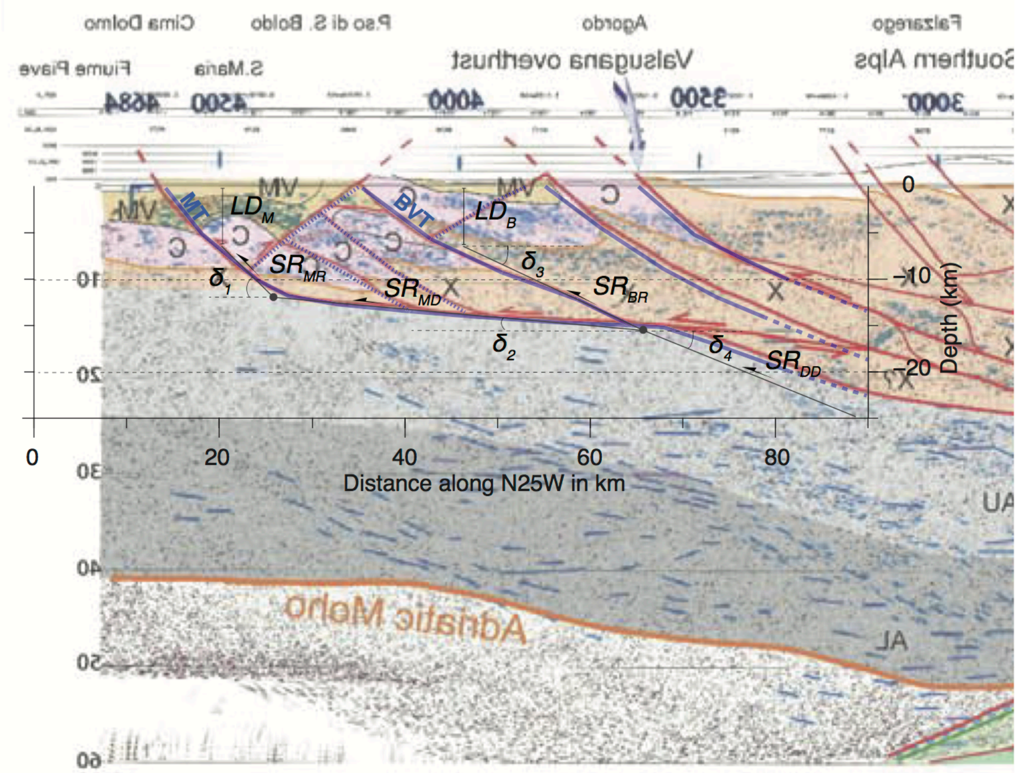


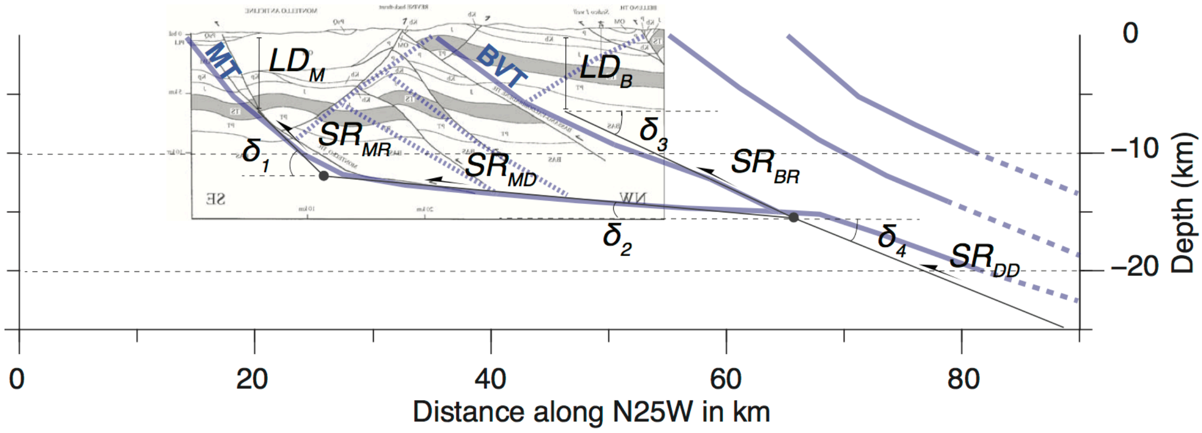
Fig. 11 Castellarin et al. 2006

system shown in red solid lines that seems to have been used for the model shown in Figure 3 of the current study. It seems difficult though to correlate the reflectivity image in fig.11 with the presented fault interpretation since (1) the reflectivity ends at 8km depth, (2) the shallow parts of the fault cut across well-documented continuous seismic signals and (3) no seismic evidence is visible for the detachment fault system at 10km to 12km depth. It is difficult to understand why the local high-resolution images and geologic interpretation would be ignored and a „simplified general interpretation" of regional scale would be used for a local study like the current one. You must provide details of what and why you „modified from Castellarin et al. 2006" and you must refer to the precise Figure that you used from Castellarin et al. 2006 and provide reasons for your specific choice.

As the reviewer rightly intuited, we used Figure 11 of Castellarin et al. 2006 for the fault geometries proposed in Figure 3. We are aware of the limits of its interpretation, but the profile is appropriate for the purposes of the modeling approach. Indeed in the context of the interseismic deformation modeling, the shallow portion of the faults, that are supposed to be locked, are not responsible of the observed deformation that is conversely due to the deep aseismically creeping fault portions (e.g. Vergne et al., 2001; Daout et al., 2006). For this reason our model is not influenced by the specific geometries of shallow fault structures, but on the contrary by the deep detachment fault system that aseismically slips. Moreover the used geometric interpretation is in agreement with the recorded seismicity and the available focal mechanisms (Figure 3) providing important constraints on the fault decollement. For completeness we provide below the comparison of the geometric sketch shown on Figure 6 of the manuscript with the part of Figure 11 of Castellarin et al. (2006), that we used to set up the fault geometry. We are aware that seismic interpretation have wide margins of error but the constraints provided by the instrumental seismicity reinforce the chosen model.



Here we compare the same sketch (Figure 6) with the local high-resolution geological interpretation provided by Galadini et al. (2005) in Figure 11, proving that the proposed geometry is also compatible with this surficial geological cross section.



Finally, it seems the major change you imposed regards the introduction of the SRDD (Figs. 6 and 7) that seems to play a major role in your model. However, please consider that the introduction of this and the stipulated other parallel faults dipping down to 20km are purly speculative as no evidence in the TRANSALP seismic data can be found in Castellarin et al. 2006 and in addition such fault contradict the concept and model documented in Fig. 11 with a pronounced and important subhorizontal detachment fault at 10-12km depth reaching far to the North beneath ESA.

As we show in the sketch above, the proposed fault geometry is in overall agreement with the interpretation documented in Fig. 11 of Castellarin et al. (2006), where the considered deep detachment faults follow the geometry indicated by the profile. The Deep Decollement (DD) is derived by the interpretation of TRANSALP profile that highlights at ~15 km depth a possible deepening of the basement rocks, that is however at the border of our model, where we have few data, playing a marginal role in the fault system. Anyway we agree that faults dipping down to 20km from the TRANSALP interpretation are speculative and that these structures might be affected by wide uncertainties.

Specific comments:

Figure 1. Much to busy figure. Reduce opacity of topography grey. Sizes of circles blue-purple and yellow-red reflect magnitude, please show scale. Rotation of Adria relative to Europe – what exactly is used as stable Europe relative point? What portion of Adria is rotating – your inset suggests all of Adria but this is difficult to justify for westernmost Adria. Explain why two different periods 2000-2017 and 2012-2018 are combined and what does this mean for the seismicity to be representative for?

As stated above, we fix the issue removing from this figure the seismicity from Romano et al. (2019), yellow-red symbols, that covers the 2012-2017 period. We followed all the graphical suggestions. As regard the Adria-Eurasia relative rotation, we add in the inset the position of the GPS stations used to determine the Adria rotation pole in Serpelloni

et al., (2016), where as well are reported all the site used to represent the stable Eurasian plate.

Figure 2. increase size of colored circles. If Adria is rotating relative to Europe as shown in Fig. 1, what would be the local motions of the stations within ESA relative to the rotating Adria look like?

Ok, we modified the figure accordingly to the comment. The figure already shows the horizontal velocities of GPS stations with respect to the Adria plate. Indeed the GPS stations located in the Venetian, Friuli and Emilian Po Plains (see Figure 1 for geographical references) show almost zero horizontal velocities being representative of a stable plate (in this specific reference frame, of course).

Figure 3. red dots and their uncertainty estimates: since profile runs oblique to rotation minor circles of Adria, do these uncertainty estimates include the relative differences of rotating Adria?

Yes, the uncertainties are projected along the profile direction.

Note that the seismicity shown along the profile AB extends beyond the dashed box shown in Fig.2, box in Fig. 2 should be as long as AB profile in Fig. 1 and 3. What are the hypocenter location uncertainties?

The box in Fig. 1 was shown only for indicating the study area, but we have now removed from the figure to avoid confusion. In Figure 3 we now plot only the high-resolution earthquake catalogue from Romano et al. (2019), in the 2012-2017 time-interval, selecting all the events for which hypocentral uncertainties are provided, which are of the order of 1-2 km. In particular more than 90% of the events have the horizontal and vertical errors (ERH and ERZ) less than 1.5 km and 3 km, respectively, and the whole dataset has a mean value of ERH = 0.7 km and of ERZ = 1.1 km.

Are the hypocenter parameters of the two earthquake data sets calculated with the same velocity model, with the same magnitude?

No, the velocity models are different, and since the one used by Romano et al. (2019) is more appropriate for the study region, we decide now to show in this figure only these earthquakes, and not those ones from the OGS bulletin.

Please add hypocenter depth color codes as in Fig. 1. Regarding geometry of proposed fault system see critical comment above.

Ok, done.

Figure4. „(after the ramp removal)" please explain or refer to text. bottom panels please refer to red dots in figure caption.

Ok, done

Figure 6. Regarding geometry of subsurface model see critical comment above.

For the proposed geometry see the answer reported for the comment above.

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