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Interactive comment on “New insights into active tectonics and seismogenic potential of the Italian Southern Alps from vertical geodetic velocities” *by* Letizia Anderlini et al.

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In this article, Anderlini et al propose to apply an approach that has been applied extensively to various tectonically active regions globally but, to my knowledge, not very often to the actively deforming areas in the alps. The authors first derive some velocity fields from GNSS and InSAR data and describe some available leveling measurements. They propose a decomposition of the InSAR velocity maps into vertical and horizontal velocity fields, which are then discussed. They move on to a very classic 2D elastic modeling of the deformation to explore potential stress accumulation when considering the active faults in the region.

In general, the paper is well written and I do not see major issues with it. However, some points need to be discussed and my comments might require a bit of work. Figures are clear (although texts could be emphasized on the maps). I see three main issues in the paper that require being fixed before publication but, after that is done, this paper will be a very interesting contribution to the discussion on how active are these frontal thrusts surrounding the Alps. I hence recommend moderate revisions and I am looking forward to see a revised version of the article. I have set major revisions in the review system because there is no intermediate step between minor and major for this journal.

Main Comments:

- There is very little discussion on how the selection of the data is performed to avoid the effect of subsidence in the plain. The authors propose a strict threshold of -0.5 mm/yr of vertical motion below which any deformation is considered as subsidence and removed from the data fed into the model. In my opinion, this is risky, as some long wavelength subsidence might affect the general pattern of deformation. If subsidence is high near the coast and in the plain, as implied by the data, then there should be a bending effect that will affect the whole dataset. The wavelength of such bending might depend on the processes at stake, but it is unlikely that a strict threshold will allow to bypass this discussion. My point mainly arises from the fact that (and this is an issue) your model does not really fit the InSAR and leveling data you are using. The relatively high rates of uplift measured in the north are not correctly predicted by your model (which underdetermines uplift) while the low rates to the south are over-determined. It seems that there is a constant trend between the geodetic data and the model. Geodetic data agree well with each other, which is great, but the model does not really manage to catch up. This could also be caused by isostatic adjustment adding a long wavelength deformation (i.e. a wavelength longer than profile you have established). One possibility would be to explore the effect of a linear trend (or whatever long-wavelength pattern you can think of) that would represent the long wavelength

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deformation needed on top of what results from dislocations in an elastic half space. This requires exploring the tradeoff between this long wavelength deformation signal and what is predicted in terms of locking depth and slip rates for both faults. It should have an impact and should be accounted for in the inverse problem.

- There is not enough details on how the InSAR data have been processed. Although the SBAS method is now quite known, quantitative information is required to assess the quality of the velocity field. It is not only because it correlates quite well with GPS that everything has been done right. For instance, correcting for tropospheric delays using a phase-topography correlation when trying to unravel a signal that correlates as well with topography is dangerous. One could easily mix deformation with tropospheric delays. Furthermore, since the region has quite strong topographic gradients, unwrapping is probably challenging and there is not a word on that (which method is used for unwrapping? In general, which software is used to compute the interferograms?). Would it be possible to see a baseline plot? Also, is there connectivity issues within the network, considering potential unwrapping issues? What is the RMS of the reconstruction of your time series? How linear is the time series? Is there a time dependent signal? There is much more details provided for the processing of GPS data and the processing of InSAR being much less standardized than GPS these days (especially with the old Envisat data) suggests there is a lot to be added in the manuscript. Finally, a lot of people have developed comparable methods for InSAR downsampling and they deserve some credit (see Lohman & Simons 2005, Jolivet et al 2012, 2015 or Sudhaus & Jonsonn 2009 for instance, but there is many other papers mentioning this).

- The description of the inversion procedure is incomplete. The algorithm used to find the minimum of the cost function should be, at least, named. Furthermore, I suspect there is some regularization of the inverse problem involved (maybe not), but please mention it. In addition, the data covariance is not described. How is it determined? One cannot follow the deal with weights if one cannot reconstruct the covariance matrix. Then, there is a problem in the a posteriori covariance discussion. The authors mention the a posteriori covariance is derived for the linear terms while bootstrap is used for the non-linear terms. In my opinion, the covariance that is derived here is obtained considering the least squares criterion (without regularization? with regularization? Is it just G^T Cd^{-1} G ?) but then, it only corresponds to a “slice” of the model space, that slice corresponding to the best non-linear parameters obtained. If so, the a posteriori covariance is greatly underestimated as it is only representative of a joint marginal of the full a posteriori PDF. Finally, one can see in supplementary material figure S6 that the range of possible models for the locking on the Montello Ramp is bi-modal. Then, if it is not Gaussian, why choosing the mean model? It seems that some models could be more appropriate. Would it be possible to sample for all the possible models using a Monte Carlo approach, which would give all the tradeoffs between the various parameters (and potentially solve the issue raised in my first comment)?

For the minor comments, please refer to the annotated pdf I have sent along with my review. Looking forward to read an improved manuscript, if I am required to do so. I also strongly encourage the authors to add their geodetic data (i.e. the GPS, InSAR and leveling rates presented in the paper) to an online repository so other scientists can have a go at the modeling, once this study is published.

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Please also note the supplement to this comment:
https://www.solid-earth-discuss.net/se-2020-10/se-2020-10-RC2-supplement.pdf

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