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Interactive comment on "Present-day geodynamics of the Western Alps: new insights from earthquake mechanisms" *by* Marguerite Mathey et al.

Anonymous Referee #2

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Summary and General Comments:

To improve the understanding of present-day seismotectonic processes in the Western Alpine arc, Mathey et al. computed a new catalogue of first-motion focal mechanisms (FM). This FM catalogue is then used to analyse seismic strain and corresponding orientations and regimes of tectonic stress using various inversion and regression methods. The new set of focal mechanisms is derived from an existing compilation of relocated hypocenters in the Western Alps, with corresponding take-off angles and bulletin first-motion polarities of P-waves. The methods for computing focal mechanisms as well as their analysis (including the computation of strain-rates and stress-inversions)

are largely based on established, commonly used methods and software tools. According to the authors, the number of FMs derived in this study is larger than any existing FM catalogue for the region and thus allowing continuous imaging of deformation regimes in 3-D at unprecedented resolution. Their main findings are: i) although consistent to first-order with previous results, fine-scale variations in the extensional regime ("discontinuous extensive patches") in the core of the Western Alps are imaged. ii) predominantly strike-slip regime in most other parts of the arc. iii) N-S compressional regime at the eastern boundary towards the Po plain and (less pronounced) along the western Alpine front. iv) the extensional direction is oriented oblique with respect to the strike of the Western Alpine arc (instead of perpendicular). v) Pattern is likely explained by a superposition of far-field horizontal stresses due to rotation of Adria and vertical deformation due to buoyancy forces.

The manuscript is well organised and written and analysing methods and presentation of results appear reasonable. However, as summarized in the next section, my main concern relates to the quality-control of the derived FM catalogue and possible impacts on the derived conclusions. In addition, the discussion of the imaged 3-D deformation pattern is general and vague in some places and should be improved and extended as outlined below. In summary, I recommend major revision of the manuscript.

Specific comments:

1) Quality of FM-Quality: I have serious concerns related to the quality-control of the presented FM catalogue. About 1258 solutions (out of the 2215 total FMs) have ML magnitudes <2.0. In my experience, working on FMs in the Alpine region, unique, high-quality FM solutions in this magnitude range constrained from P-polarities alone are extremely rare. In almost all cases the solutions are highly uncertain and ambiguous and -if at all- only possible in regions of extremely dense station coverage. In addition, polarities in standard, routinely picked bulletin data (as used in this study) are full of mistakes/blunders and not very reliable unless reviewed by experienced seismologists with the goal to derive a reliable focal mechanism. This is likely even worse

when data is combined from different bulletins. In combination, I expect severe uncertainties and ambiguities in the derived FMs. In less dense instrumented areas, I would even consider solutions in the ML range 2.0-3.0 (769 FMs) as highly uncertain (at least partly), e.g. if the focal depth is poorly constrained and therefore take-off angles can be highly uncertain. In the provided FM catalogue (supplementary material) 94% of the FM solutions are of lowest quality class "D" (2076 FMs) indicating that the majority of the solutions is highly uncertain. The authors associate "D" qualities with "strike uncertainties" of 45-55 deg (and I assume it's even higher for dip and rake). It seems, however, that the a priori quality of the solutions is not taken into account in any of the applied inversion schemes. The uncertainties derived by the Bayesian inversion of P and T plunges in Fig. 8, on the other hand, are surprisingly small (<10 deg for the majority of the region, values I would associate with high-quality FM solutions). This uncertainty in P and T plunge seems therefore unrealistically small to me given the potential quality of the majority of the used "D" FM solutions and raises the question how much of the "small-scale variations" in the deformation regime is related to noise introduced by the large amount of low-quality FM solutions. The authors should extend their discussion on the potential uncertainties of the low-quality solutions and how it potentially affects their inversion results. E.g. they could use a priori weighting based on the solution qualities in their inversion and compare results against solutions without quality weighting. Also, in some of the maps, the locations of higher-quality solutions could be marked by circles of different colour to identify the regions which are constrained by more reliable solutions. In addition, I also miss a benchmark of the "automatically" FM solutions derived by the authors against existing, manually reviewed, high-quality FM or moment-tensor solutions published by French, Italian and Swiss agencies. This would help to assess the potential uncertainties and reliability of this catalogue.

2) Seismic strain rates: As the authors stated themselves, the seismic moment is dominated by the largest events in a region. I am therefore wondering how much the seismic strain rates in Figure 4 and the seismic "flux" in Fig. 7a are controlled by the largest events in this region. I would therefore suggest to plot the corresponding beachballs

of events with ML>=4.0 in the background of Figure 4 and indicate the corresponding magnitude of these events. This might help the reader to understand if the corresponding strain regime and strain rate in the corresponding zonation is mainly dominated by the largest event within this zone. Is the relatively smaller moment release in the NE corner of Figure 7a real or just an artefact of the completeness of the author's catalogue? In Figure 2c, I see several M3-4 events in this region and I would have assumed to see a corresponding relative increase in moment release in this part as well.

3) Discussion/Interpretation: In my opinion, the discussion part could be extended and improved. The discussion is a bit vague and general. Some aspects which could be extended:

- Role of slab dynamics in the Western Alps: Different models have been proposed on the slab structure: detached (e.g. Lippitsch et al 2003) vs. attached (e.g. Zhao et al 2016). In addition, if attached, rollback and delamination might play a role (e.g. Sue et al 1999). How do the latest insights into slab-models fit/support your observations? Do the presented results add new constraints to this ongoing discussion?

- I do not understand how robust (and how to interpret) the N-S compression in the western Po plain shown in Figure 6 is. In Figure 5 this compression seems more SW-NE? and others (Delacou et al 2004, Eva et al 2020 (tectonics)) show also more SW-NE or even E-W directed compression. Is this due to very few mechanisms of one cluster? How well are they constrained? Why did the former compressional domains east and west of the Arc (e.g. Delacou et al 2004) "disappear/reduce" in your results? Is it possible that the weight of the compressional events is just suppressed by the larger number of additional small (and likely noisy) low-quality FM solutions of oblique type? Or did the former reverse type mechanisms change to strike-slip in your FM calculation? If so, are the new solutions better constrained then the previous solutions?

- How can the contrasting juxtaposition of compressive and extensional domains at the eastern boundary be explained (e.g. Figure 9c)? Similarly, can the mix of strike slip and normal mechanisms within the core of the Western Alps be simply explained by a general transtensive regime (some faults take up strike-slip, others the normal component)? Are principal axis compatible with transtension (i.e. is the orientation of the T-axis consistent and only B and P flip?). If not, it is difficult to explain with a homogeneous far-field stress. A final sketch figure indicating the overall kinematics would certainly help to summarize and better explain the proposed model of vertical deformation controlling the patches of extension while rotating Adria controls the overall strike-slip regime. Still the question remains why there is little spatial correlation between "extensional patches" and surface uplift. Also, you should compare your results and model in more detail with the recent paper of Eva et al 2020 (Tectonics), who propose a similar 3D seismotectonic model of the same region.

4) The English is reasonable; however, I spotted some smaller issues I have listed in the following list. I would recommend another round of proofreading by the authors.

Detailed Comments:

- Line 33: "lays" -> "locates"

- Line 34 and many other places in the manuscript: "high frequency spatial variations" -> I would replace "high frequency" with e.g. "short wavelength" or "variations over short distances". Frequency relates to time not space.

- Line 43: "alpine belt" -> "Alpine belt"

- Line 48: "with respect to Europe" or "the European plate"

- L. 52: "Western Alpine arc"

- L. 71: "updated catalogues allow to"

- L. 80: Indicate the exact period of the catalogue: from 19XX to 20XX. What is the estimated Mc of this catalogue? All events included from all contributing agencies or only above a certain magnitude?

- Figure 1: Maybe here (or in another map) highlight the location of M>=4 events, e.g. as circles of different colour.

- L. 100: SED operates currently >200 stations. For a more up-to-date reference on the network see e.g. Diehl et al. 2018, Earthquakes in Switzerland and surrounding regions during 2015 and 2016, https://doi.org/10.1007/s00015-017-0295-y

- L. 102: "recorded" -> "installed"

- L. 108: "combined a 3D-velocity inversion"

- L.110: "in the order of a few degrees" – This is quite an optimistic statement in my experience. Especially for shallow sources, the uncertainty in focal depth can result in quite significant uncertainties in take-off angles, which can be more than few degrees... Not sure how this uncertainty is estimated, but I would at least add "for well constrained hypocenters"...

- L. 111: An earthquake is not a blast, I would write: "The complete catalogue includes earthquakes as well as blasts related to quarry and mining activities."

- L. 119: What is the S-phase needed for? Are you using S-polarities as well? As pointed out above, the azimuthal/take-off gap criteria are not sufficient to exclude grossly ambiguous FM solutions. I would have rather considered HASH's number of FM families (use only solutions with 1 family of solutions -> +/- unique solution) and number of possible solutions as quality criteria.

- L. 126: "uncertainties on picking" -> shouldn't it be "in polarities"? Did you use quality weights for polarities (e.g. U/D vs +/-) for the grid search? Did you use the uncertainty in TOA? How is this uncertainty estimated and how is it implemented in your HASH procedure?

- L. 127: Was ML recomputed by Potin or is this basically the original SISMALP ML? Is ML really always measured from S or is it simply the peak amplitude on the horizontal components (regardless of P,PmP, S, SmS, surface wave)?

- L. 143: “pression” -> “pressure”

- L. 144: “The representation of style . . .” Rephrase this sentence.

- L. 180: This is a bit confusing, first the authors summarize different scaling-relationships to convert Ml to Mw, referring to a study proposing a polynomial fit but then it seems the authors use a 1:1 relationship without too much explanation why this is valid. What is the mistake in term of Mo of this simplification for larger events (M>4.0)? Why not using the already established relationship?

-L. 200: In classic stress-inversion the slip is assumed to be on the active plane (not on the auxiliary one). Others (e.g. Kastrup et al 2004) therefore down-weighted solutions for which the active plane is unknown (e.g. no information from relative relocation available). How did the authors address this problem in their stress-inversion strategy?

-L. 243: The methods “enables us to assess whether formal uncertainties on fault planes . . . are over or underdetermined”. What is the result of this assessment then for your data? What are the formal errors of your focal mechanisms?

- L. 267: “in each” -> “for each”?

- L. 286: “are presented as surface projections in Fig. . .”

- L. 289: -> “Therefore, including historical events . . . in the summation could modify. . .”

- Figure 5: I see some differences in P and T between the two inversion methods which seem larger than the 1sigma estimated by the Bayesian inversion. Any explanation for these differences?

- Line 344: You mean both methods give similar results for the orientation of sigma3? Consider rephrasing this sentence.

- L. 351: -> “zoning realised” -> “zonation defined . . . contains seismicity of different tectonic regimes”

- L. 354: -> “Overall, the set. . .”

- L. 381: Not sure I understand this sentence. What are “longitudinal directions/faults”? Isn’t the mentioned fault striking NE-SW?

- Figure 7b) why not add a colour bar (similar to Delacou et al 2004) rather than describing the meaning of the colours in the caption?

- L. 455 and elsewhere in this section: Instead of “153 events” I would write “153 focal mechanisms”.

- L. 459: This extensional zone at 10 km: How well is the depth constrained for the associated events? This zone north of the Valais is more complicated than strike-slip. It contains all kinds of mechanisms: Strike-slip, oblique normal, oblique reverse, most likely it consists of an array of strike slip faults connected by releasing and restraining bends/step-overs. See e.g. Diehl et al. 2018, Earthquakes in Switzerland and surrounding regions during 2015 and 2016, https://doi.org/10.1007/s00015-017-0295-y. A bit north of the major strike-slip zone, towards the Alpine front, there is indeed a zone of extensional events (e.g. M4.3 Chateau-D’Oex earthquake of 2017, Jaun M3.8 event of 1999), which is maybe what the authors image in their cross-section 1 (needs to be checked). It is described and discussed in a recent publication which is currently in press and should be published online soon (Diehl et al. 2018, Earthquakes in Switzerland and surrounding regions during 2017 and 2018, https://doi.org/10.1186/s00015-020-00382-2). However, this extensional domain is much shallower than in the author’s cross-section (uppermost crystalline basement or Mesozoic sediments, likely <5 km).

- L. 470: -> “as shown in Figure 7b”

- L. 480: -> “. . . . Figure 7b shows . . .”

- Figure 9: It would be helpful to add additional geological reference information from geological profiles, like position Alpine front, Ivrea body, etc. What are the tiny black dots (difficult to see)? Projected earthquakes (everything) or just earthquakes with corresponding focal mechanisms used in the inversion/regression? I would make the symbols corresponding to FMs bigger (maybe as circles), try colour code the quality of the mechanisms. This would help to distinguish parts well constrained by data from areas with inter- or extrapolated values.

- L. 502: -> "… runs from Lake Geneva … "

- L. 507: remove "Indeed"

- Figure 7b/text around line 517: Why not show Figure 7b for different depth intervals (similar to tomographic results) rather than projecting everything to one layer? This would lead to a "patchier" distribution with more white-spaces, but would avoid some of the misunderstanding due to vertical projection?

- L. 540: "…follow the structure of the European crust…" Not sure in terms of what? You mean in terms of dip? Or lithology? Should be more specific.

- L. 543: "former slab" What do the authors mean here? Does this "former slab" relate to the possibly detached slab? As mentioned above, this discussion needs to be extended.

- Figure 10: Are the beachballs shown on the profile in b) lower hemisphere projections (as in map view) or cut along the profile (projections)? Since they all plot on each other it's difficult to see anything… In caption of 10b, why not simply say: Dashed lines represent the European and Adriatic Moho after (???). Moho in 10b is from Spada as well?

- L 585: "… which is independent of any a priori tectonic zonation"

- L 586: "… movement along the longitudinal Alpine strike" Not sure what the authors mean here.

- L. 588: "rough scheme" -> "first-order distribution"?

- L. 650: "low noise transcurrent motion" Low-noise in terms of what?

- L. 655: "but may also add another component" -> What is this other component?

- L. 655: "While a purely plate-related geodynamic model seems discarded by now… our observations may revive the role of plate motion…" This sentence doesn't make much sense to me. Is it discarded or not? What other process should explain seismicity and deformation?

- L. 678: "seismic imagery" -> "seismic imaging methods"

- L. 687: "The high spatial resolution of seismotectonic regimes sheds light…"

- L 695: Why not add used polarities and take-off/azimuth angles to the supplementary material to allow others to assess the quality of mechanisms.

- L 698: What about all the data added from other networks? Nowadays most networks have DOIs and should be cited with their corresponding network code and DOIs.

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