Report #1
I have read the revised version of Mathey et al's manuscript as well as their response to the comments raised by the reviewers related to the previous version. All references to lines and pages refer to the version including the track changes.

A) The authors responded at length to the main concern related to the quality of the computed focal mechanisms (FM) raised by reviewer #1 and myself. I can follow their arguments partly, however, I am still not entirely convinced of the value and the reliability of the FMs calculated for magnitudes <2.5. This is mainly because it largely contradicts my daily experience in earthquake analysis in the Alps, in which reliable mechanisms for magnitudes <=2.5, even with comparable dense (or even denser) networks, are rather exceptional. If possible at all, it requires extremely careful manual review during picking as well as the FM computation.
I am quite surprised to read that, according to the author's response, only four of their entire 2200 FMs (about 50% have Ml <2.0) have more than two possible solutions. Again, this completely contradicts my own experience, maybe this is a misunderstanding of what I had in mind. In my own HASH implementation, I use the following parameters, which proved to result in very realistic uncertainties:

dang = 2.0 ! minimum grid spacing (degrees)
maxout = 500 ! max number of acceptable mechanisms output
cangle = 30.0 ! mechanisms are "close" if less than this angle apart (degrees)
prob_max = 0.1 ! probability threshold (cut-off) for multiples (e.g., 0.1)

Maybe the authors use less strict parameters (especially for cangle) or allow for a less broad solution space? Again, in my experience the majority of mechanism result in multiple-solution families for M<2.0 events. The authors also argue that calculating FMs for such small events is mainly possible because of the extremely dense network. However, the network used in their study shown in Fig. 1 is not "extremely" dense on average in my opinion. Unfortunately, no km scale is provided in this map (as in most others), but in many regions the average spacing looks more like 30-50 km to me (which I would not call extremely dense nowadays). In addition, some stations (I guess the lines) are from temporary experiments. One more aspect: Since the authors report the average of all acceptable solutions of HASH, I am wondering if this averaging might lead to a systematic bias towards strike-slip mechanisms in case of extremely poorly constrained solutions. I apologize if this appears extremely pedantic, but my point here is to avoid that readers less experienced with FM calculations get overly confident when using this FM-catalog in future studies. The quality classification currently used seems not sufficient to me to really distinguish between reliable and not reliable solutions and I would have preferred to simply use e.g. the variation in the "acceptable" solutions returned by HASH as a quality measure rather than the predefined classes based on gap etc. Depending on the type of mechanism, the source depth and the distribution of polarities on the stereonet, such parameters can be completely underestimating the true uncertainty of the FM. Using HASH parameters as listed above, in comparison with some manual revised reference-solutions, should have provided better and more robust quality classifications as the one currently used.

Nevertheless, I admit that the overall results look reasonable and consistent with previous studies and maybe the FM qualities impact the overall results less than expected. Therefore, as practical solutions to make the quality classification of the presented FMs more transparent for readers I suggest to:

1) Provide a table in the supplement with all the HASH inversion parameters used by the authors to make the results reproducible.

2) As nowadays commonly required in many journals, provide the full dataset used to calculate the mechanisms e.g. in a data repository. The authors disapproved that suggestion according to their response because data seem to be used for another unpublished study. But providing only FM-basic information like Stationcode, polarity, polarity-quality, azimuth-angle, take-off-angle and basic hypocenter information (OriginTime, LAT, LON, Depth, Mag) should be sufficient (no information on phase picks is required if that is still used for something else). With this basic FM-data provided, everybody could use her/his own tools to assess the corresponding FM quality. I strongly encourage the authors to make that basic FM-data available alongside with their publication.

3) With HASH parameters similar to the ones listed above generate a figure which shows: a) number of "accepted" FM solutions (all strike-dip-rake combinations which fit the data within their uncertainties, which means allowing for at least 1-2 outliers, HASH parameter) vs. magnitude for each event. b) similarly, plot number of solution-families (make sure cangle, prob_max are reasonable) vs. magnitude for each event. I would expect that the number of solutions & solution-families reduce for larger magnitudes. Such figure would provide additional information on the confidence of the FM solutions of low magnitude events.

→ We thank the anonymous reviewer for his continued help improving our manuscript. While we stress that the described seismic networks allow us to compute some reliable small magnitude focal mechanisms, we acknowledge that the reader should be well aware that the computed FM dataset is suitable for our large regional scale study, but may not be suitable for any local scale study due to the heterogeneity in the quality of the FM computed in the present study. Thus we followed all the suggestions to help the reader understand our variable quality focal mechanism huge dataset. We indeed define mechanisms "close" together as those having a difference angle < 45°, and choose to ignore multiples with a low probability by setting the probability threshold for multiples to 10%, which explains the low number of multiples even for low magnitude events in our dataset. As for the use of the average of all acceptable solutions, this output is indeed the one used by the software's authors (Martínez-Garzón, 2014), for which they do not mention any potential bias in HASH FM computation for less constrained solutions.

1) We now provide in the supplement a table (Table S1) with the control parameters used for HASH focal mechanisms computation.

2) We now provide in the supplement the dataset ("computedFM_HASHformat.inp") corresponding to the 2215 computed focal mechanisms in HASH input format.

3) We added the suggested plots in the supplement (Figures S7 and S8) in order to help the reader assess the level of constraints on focal mechanisms depending on magnitude range.

B) While reading the manuscript a second time, I realized that section 3 (which seems the result section) extends over 14 pages (of 35 total). It seems to contain already a lot of interpretation and discussion and all the details listed in this section make it difficult to not lose focus. I would suggest to consider shortening this part to the essential findings. In addition, there are still issues with the English, some sentences are rather awkward or unclear. I listed some examples below, but I definitely recommend another iteration of thorough proofreading by the authors.

→ As suggested by the reviewer we removed some detailed description of the results that could be observed on the figures and stick to the major findings of the results section in the main text. We additionally made the English corrections suggested and went through an in-depth proofreading of the manuscript which improved its overall fluency.

C) Final comment: Isn't the fact that extension in the southern part appears to have no corresponding uplift signal in the geodetic data related to the depth of this extensional zone? In profile 4 extension seems >15 km, isn't this why no geodetic signal is seen at the surface? Therefore, the comparison of geodetic data and (surface-projected) seismotectonic results and derived conclusions in Fig 11 might be of limited value? Maybe a better comparison would have been (as proposed in my previous comments) to make a stress-regime map limited to FMs in the upper crust (e.g. <10 km) and compare that to the geodetic data?

→ As suggested in the previous review, we tested a stress-field map limited to the FM in the upper crust (Figure R2). However we did not add it to the manuscript since it appears consistent with the one derived using all FM with their complete depth range (Figure 6). Concerning the comparison of seismic and geodetic deformation, we stress that geodetic results also have their own uncertainties. The southern part of the study area, which shows limited uplift signal in the geodetic map from Sternai et al. (2019), is subject to more uncertainties than other parts since it relies on a limited number of young GNSS stations from the Italpos network. This area indeed appears affected by null to moderate uplift depending on the GNSS solution considered (e.g. Kreemer et al., 2020[1]).

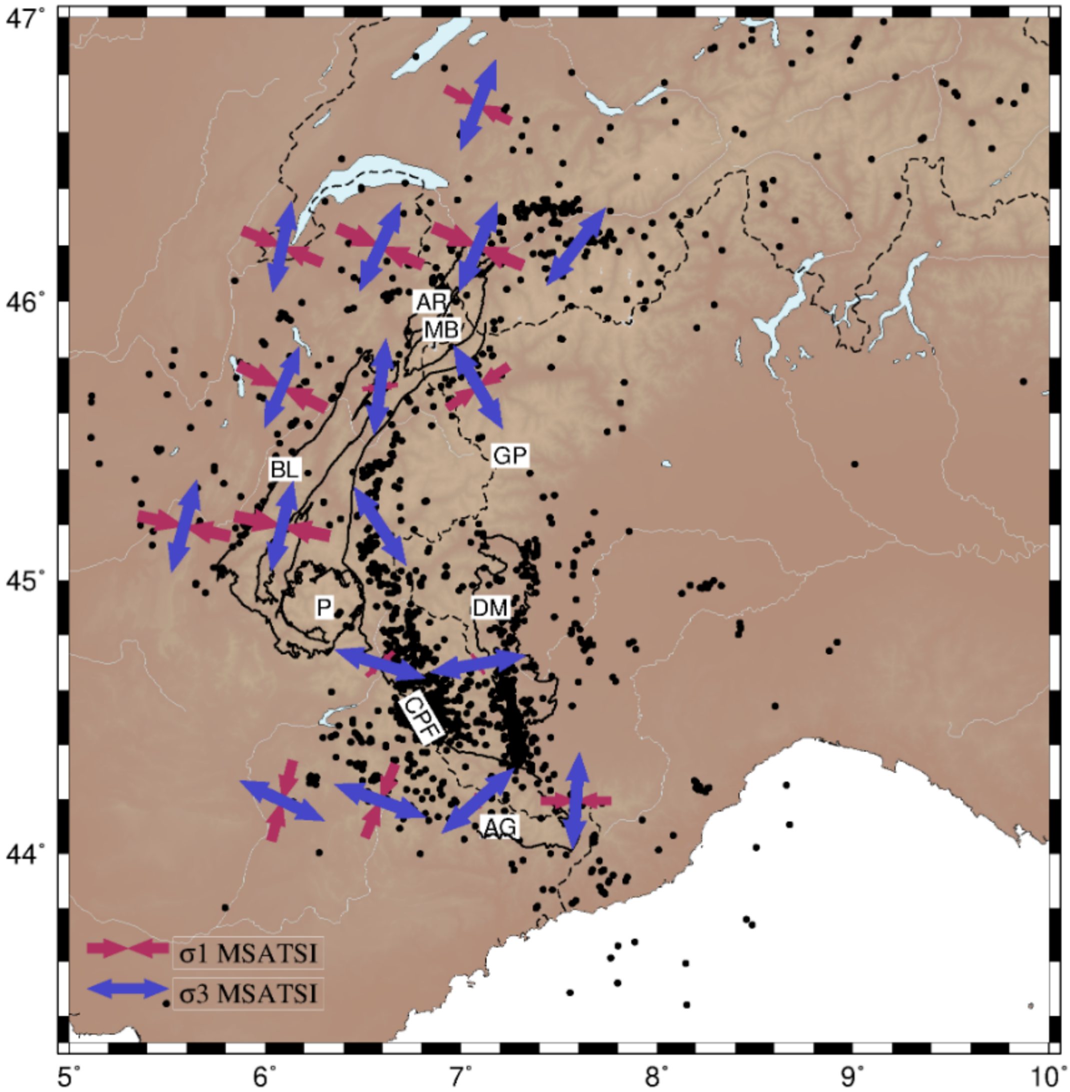

Figure R2. Stress-field derived on a 0.5°x0.5° grid using focal mechanisms in the 0-10 km depth range only. The inversion is performed with MSATSI if at least ten focal mechanisms fall within each cell. The orientations of the deviatoric stress tensors appear compatible with the one retrieved using the complete depth range of focal mechanisms (Figure 6).

In addition to these general comments, I have specific ones listed below.

Detail comments:

- Line 19: “down to low magnitudes” -> rephrase. Give exact numbers instead

→ We rephrased it to “records in the 0-5 magnitude range” as stated l.84.

- Line 24: “since 1989” -> indicate entire period of this study: 1989-2013

→ ok.

- Line 33: “Compression is robustly…” -> Maybe: “Robust indications for compression are only observed at the boundary between the Adriatic and…”

→ ok.

- In many places: be sure it's correct: "short-wavelength" vs "short wavelength"

→ we corrected it to "short-wavelength" at lines 34, 40, 730 and 764

- L. 79 (caption Fig 1): -> "… can be clearly identified in the seismicity"

→ ok

- L. 88: "stress oriented inversion" -> "stress inversion"

→ ok

- L. 130: Why not simpler: "The preferred solution corresponds to the average solution of all possible acceptable solutions…"

→ ok

- L. 131: "The HASH code…"

→ ok

- L. 134: The statement on the Ml computation does not make sense in this location. Move it somewhere to line 185 where you talk about magnitudes. In addition, the statement still seems wrong, it's not the maximum S-wave, it's the maximum of P or S wave, right?

→ It is indeed computed as the maximum amplitude of the signal. We moved it to line 186 and rephrased it to "The local magnitude (Ml) of the catalogue is based on the maximum amplitude among all three components of the signal. A double conversion toward Mw has been proposed by Cara et al. (2015), through another national-scale local magnitude (Ml LDG)."

- L. 136: "Over the 2215" -> "From the 2215…"

→ ok

- L. 163: "… on the focal plane solution is lost"

→ ok

- Figure 2: The axes-annotations/labels are not readable in a,b,c. Make them bigger!

→ We made Figure 2 bigger so that labels are now readable.

- L. 220: The MATSI software is based on the method of Hardebeck and Michael 2006, no? In the MATSI paper, Vavrycuk 2014 is not mentioned at all. Please check this…

Figure 3: The axes-annotations/labels of the Kaverina-diagrams are not readable at all. Make them bigger! Also, the dots themselves are too small…

→ MSATSI is indeed based on SATSI from Hardebeck and Michael (2006), which makes use of the same inversion procedure (least-square inversion) as SI (Vavrycuk 2014), which was previously mentioned in the manuscript, as opposed to grid-search inversions such as FMSI (Gephart, 1990). To make it clearer we rephrased it to "We used the MSATSI (Matlab Spatial And Temporal Stress Inversion, Martínez-Garzón et al., 2014) software to perform an inversion for each cell encompassing at least 10 focal mechanisms." We also modified Figure 3 to make it readable.

- L. 309: what do you mean with "time/energy relation"? You mean moment rate?

→ We modified the sentence as follows: "The short time span of the observations (24 years) prevents from thoroughly investigating seismic energy release spatial and temporal variations".

Figure 4: Caption: Explain what symbol delta refers to, I assume its dip?

→ delta indeed stands for the dip. We now refer to table 1 in the caption for symbol explanations.

- L. 357: "Lest" -> "Least"

→ corrected

- L. 381: Here you could also compare the results to others studies with similar results
→ We added the references to the following studies l.381 :
"The orientations of the strike-slip tensors appear in good agreement with previous studies, whether at the regional (e.g. Delacou et al., 2004) or at the local scale (e.g. Bauve et al., 2014; Kastrup et al., 2004). However the orientation of the extensive tensors appear slightly less perpendicular to the belt than previously observed (e.g. Sue et al., 1999; 2007b; see section 4)."
As stated by the reviewer though, the result section is already quite extensive. For this reason a more detailed comparison of orientations with previous studies is found in the discussion section (section 4.2 l.610-645).
- L. 409/10: Isn't it "projected to" the surface?
→ ok
- L. 432: NW or NE Switzerland? Don't you mean NE?
→ corrected
- L. 443: consistent with… or according to… ?
→ ok
- Figure 8: Isn't the higher variation in P expected in a transtensional regime (where P and B are flipping while T remains the same)?
→ Figure 8 represents the uncertainty on P and T mean distributions. Higher uncertainties on P does not necessarily imply higher variations in P plunges. Indeed such variations can be well resolved while more homogeneous areas (i.e. P plunge constant, Fig.7) can be related to higher uncertainties, for example where the data is too scarce.
- Several places: "in the overall" -> "in general"
→ ok
- L. 486: Do you mean "consistent with the surface-projected results"?
→ Yes, corrected.
- L. 508: "narrow band of strike-slip deformation along…"
→ ok
- L. 529: "surrounded by a … regime, especially..."
→ ok
- L. 537: … by a slightly increasing… -> rephrase! By how much?
Rephrased : "It appears that extension is characterized by a depth increasing from ~10 to ~20 km from North to South"
- L. 540: "geological structures" what do you mean? Lithologies? Tectonic units? Specify
→ "[...] while cutting through external geological units both north and south of the profile."
- L. 543: "artefacts" -> caused by what?
→ These artefacts are likely introduced by the meshing of the model.
- L. 558: "of all mechanisms resulting from the projection to the surface"?
→ yes, modified accordingly.
- L. 560: "exemplified" -> "documented"
→ ok
- L. 576: "does not appear to be controlled by the geometry of the former European slab. Both extensive…" COMMENT: I still don't really understand what the authors mean here with the "former European slab"? All I see in this figure is the geometry of the Moho. And why "former"? isn't this still European crust/lithosphere? Rephrase this statement! Same later around line 581.

→ we referred to the fact that convergence appears to be over beneath the western Alps from geodesy and related kinematic models. We rephrased it for clarity :
“the distribution of the style of deformation does not appear controlled by the structure of the European Moho [...] thus coincides with the boundary between the European crust and the Adriatic one.”
- Figure 10 caption. In the caption you should describe how the beachballs are projected (they are cut and projected right?) Mention the meaning of the colors of the mechanisms. Since the authors plot all of them, not much detail can be seen.
→ We added the following information: “ a) Cross-section of computed focal mechanisms (vertical sections) projected (see width in c)) along ECORS-CROP seismic reflection profile, modified from Marchant and Stampfli (1997). Green: Strike-slip mechanisms; blue: normal mechanisms; red: reverse mechanisms. [...] b) Focal mechanism cross-section (vertical sections) projected along CIFALPS profile (projection width in c)) of local earthquake tomography, modified from Solarino et al. (2018), color-coded as in a).”
- L. 602: repetition…
→ removed
- L. 639: -> “… extension previously proposed (CITATIONS).”
→ We added the following citations : (Eva et al., 1997; Delacou et al., 2004; Sue et al., 1999; Sue et al., 2007b).
- L. 660: I have to admit that I still don’t really understand how such “gravitational collapse” causes N-S directed compression in the Ivrea mantle. Try to rephrase this statement.
→ We do agree. Our main point is that gravitational collapse cannot explain the asymmetrical compression observed at the periphery of the belt. We rephrased it for clarity : “While we cannot decipher which processes are at the origin of this very specific and local pattern, gravitational collapse schemes fail to explain the localized compressive pattern observed at the border of the chain, and indentation by [...]”
- L. 679: “collocate” -> “correlate”?
→ ok
- L. 681: As the authors mention earlier, the extension in the south is much deeper (profile 4). Isn’t this the reason why the signal in the geodetic data (at surface) is missing? Where the authors see correlation (S-Valais), the seismotectonic extension is shallow…
→ As explained in the answer to comment C), the vertical motion derived from geodesy is poorly resolved in the south-eastern part of the area. If some extensive and uplifting areas appear spatially correlated, we here only stress that maximums of respective deformations appear disconnected.
- L. 699 and elsewhere: Try to avoid the use of “Indeed,”
→ ok
- L. 703: “When the continuation…” This sentence should be rephrased, bit confusing. Maybe what the authors mean: “…, while the debated continuation … is likely deeper than about 60 km.”?
→ yes, modified accordingly.
- L. 715: “can’t” -> “cannot”
→ ok

- L. 720: Still I don't fully understand what the authors mean with "purely plate-related geodynamic model" vs their "role of plate motion". Isn't everything plate-tectonic related? The authors should improve this part of the discussion.
→ By "purely plate-related geodynamic model" we mean without involving interactions with intrinsic surface (erosion and deglaciation) and deep (mantle- and slab-related) processes in order to explain present-day seismicity and surface deformation; while by "the role of plate motion" we mean in comparison with the role of surface and deep processes which have also been proposed as being at the origin of both crustal deformation and seismicity (e.g. Mazzotti et al., 2020; Calais 2016[2]). We added these following information in the discussion:
"While a purely plate-related geodynamic model seems discarded by now (D'Agostino et al., 2008; Devoti et al., 2008) due to the evidence of both extension and uplift in many places all along the Western Alpine arc, which cannot be explained by plate kinematics alone, we stress that our observations revive the role of plate motion in interaction with buoyancy forces in an attempt to explain the current Alpine kinematics and seismicity (Figure 12)."
- Figure 12 caption: -> "… block diagram"
→ Ok
- L. 743: "resolution without the use of a priori … zonation"?
→ ok

References:

[1]Kreemer, C., Blewitt, G., & Davis, P. M. (2020). Geodetic evidence for a buoyant mantle plume beneath the Eifel volcanic area, NW Europe. *Geophysical Journal International*, *222*(2), 1316-1332.

[2]Calais, E., Camelbeeck, T., Stein, S., Liu, M., & Craig, T. J. (2016). A new paradigm for large earthquakes in stable continental plate interiors. *Geophysical Research Letters*, *43*(20), 10-621.