Reviewer 2:

General answers:

1) Quality:

We thank the anonymous reviewer for his constructive comments. As stated in the answer made to reviewer 1, reliable focal mechanisms could be computed for small magnitude earthquakes thanks to the dense regional Sismalp network and thanks to the good constraints provided by the relocation of the events in a 3D model and by the corresponding take-off angles. We acknowledge that, while we could not check visually each of the 2215 focal solutions selected for this study, not all of them are equally well constrained. However we would like to stress once more that the aim of our study is not only to derive classic seismotectonic fields over the Alpine belt as was done in previous studies with published focal solutions in the 3-5 Ml range (e.g. Delacou et al., 2004; Sue et al., 2007). Indeed only a few new earthquakes of this size occurred in the western Alps since then. Our goal is on the contrary to increase the spatial resolution of existing seismic deformation fields, by assessing which features appear robust based on a huge, although possibly noisy, set of small to moderate magnitude focal solutions. This goal is motivated by the increasing number of available seismic records in low deforming areas thanks to the enhancement of seismic networks in the recent years. In order to make the best use of these emerging datasets, we here selected 2215 focal solutions over many more that could be computed. We detail in the following why this unconventional focal mechanisms dataset, combined with state-of-the-art analysis tools, allows us to establish unsuspected spatial variations in the seismic deformation pattern in the Western Alps.

2) discussion:

We again thank reviewer 2 for his suggestions which helped us to improve the geodynamic discussion of our manuscript. As detailed below, we now address in more detail the issue of where our results stand regarding the ongoing debate on the state of the European slab beneath the Western Alps as well as the robustness and interpretation of compression.

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.

Concerning the quality of the dataset, first of all, as stated by the reviewer, we were only able to derive well-constrained fault plane solutions for small magnitude events (Ml < 2) thanks to the high quality of the seismic records. The records indeed result from a region of extremely dense seismic station coverage as mentioned by the reviewer. The relocation of the events in a 3D crustal velocity model provided tight constraints on the depth of the seismic events and thus on the take-off angles (Potin, 2016) used to compute focal mechanisms. Moreover the polarity dataset used in the present study was manually revised each year by Sismalp network seismologists. We would also like to stress here again that the quality flag is not a standard deviation provided on focal mechanisms but a summary of several uncertainty indicators. It thus only suggests that fault plane solutions may not be as constrained as A-C quality solutions, and by no means that these solutions are necessarily unreliable. To assess which features are robust within this qualitatively heterogenous fault plane solutions, we thus exploit the redundancy provided by the corresponding huge and spatially dense dataset by implementing a Bayesian surface reconstruction inversion scheme. As explained in section 2.4, we do weight focal mechanisms differently according to their a priori quality, and decipher, given the level of noise necessary to fit the data, how much of these uncertainties are needed to explain the input dataset.

Concerning the 1-sigma uncertainties provided by the Bayesian inversion which may appear low in comparison to the quality of the input data, this legitime question was clarified by adding the following paragraph in the revised version : "**As in any inverse problem, the level of**

uncertainty associated to the solution can only be interpreted relative to the level of resolution. Here, it is important to keep in mind that the solution model is smooth and represents an average over a given wavelength. The model uncertainties shown in Figure 8 are associated with this spatial average, and can therefore be much smaller than the data variability or data uncertainties (see Choblet et al, 2014 for more details)."

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.

In the Kostrov summation which is illustrated in Figure 4, the computed strain rates indeed depend mostly on the higher magnitude events of each area, as mentioned in the main text (l.296). We acknowledge that this graphical information could be valuable to the reader and we plotted the corresponding features in Figure 4 as suggested. Regarding the low amount of moment release in the specific area NE of our network, we confirm that this represents an artefact due to a border effect, based on Figure 1 (less dense station coverage and lack of stations east of the area resulting in less events recorded than further west), and based on Figure S2, which shows the raw seismic moment prior to smoothing. We thus apply an additional mask on Figure 7 based on the density of seismic events.

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?*

We indeed omitted to mention that several tomography models over the western Alps suggest a slab break-off, while the recent study of Zhao et al. (2016) represents a possible evidence for a continuous slab down to a depth of ~ 300 km. We thus added the following paragraph to the discussion l.645 :

"The current state of the European slab beneath the Western Alps remains to this day a matter of debate. Indeed, evidence for both a detached slab (e.g. Lippitsch et al., 2003,

Kissling et al., 2006, Diehl et al., 2009, Kästle et al., 2018) or a continuous slab (e.g. Piromallo and faccenna, 2004; Zhao et al., 2015; 2016) are claimed between different tomography models. The focal mechanisms derived in this study thus only provide seismic deformation styles up to 35 km, when the continuation of the European Moho beneath the Adriatic one, deeper than ~ 60 km, is under ongoing discussions. While our results constrain a depth too shallow to help decipher between these two end-member models, we rely on the literature to suggest that a recently (< 5 My, e.g. Lippitsch et al., 2003) detached slab, with its detached extent nowadays located beneath the eastern margin of the Western Alps, could induce extension as well as uplift, due to lithospheric rebound processes and/or mantle upwelling related to the sinking into the asthenosphere of more dense lithospheric material (e.g. Sternai et al., 2019). Slab dynamics can indeed cause transient dynamic topography (e.g., Faccenna and Becker, 2020), or influence exhumation processes (Baran et al., 2014; Fox et al., 2015)."
As for the role of rollback and delamination, they may indeed introduce additional spatial variations of the vertical and horizontal deformation fields if attested. However, our results cannot bring any new constraints on this specific aspect due to the spatial scale and related depth resolution of our study, and we propose here a first order kinematic model that may be enhanced with more complex features in the future.

- *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?*

The robustness of the various compressive patterns is discussed in section 4.2 (l601-611). As shown in section 3.3, the occurrence of compression in the Po plain is a robust feature. However the probabilistic approach used here allows us to decipher robust deformation modes throughout the area and not to assess the robustness of the corresponding azimuthal directions. It appears from section 3.2 (Figure 5 and table 2) that the azimuthal direction of compression in the Po plain is less robust than for the surrounding areas, and varies from N-S to NE-SW between the different inversions. Despite this uncertainty, our results suggest compression with azimuth ranging from ~10° to 45° CWN and differ from some other studies, which identify either E-W compression (Delacou et al., 2004) or N-S compression (Eva et al., 2020). The depth of the corresponding seismic events also varies significantly in the literature. Thus the interpretation of this localized compressive pattern is still a matter of debate, and the spatial resolution of the present study, which aims at studying deformation at the scale of the Western Alpine arc, does not allow us to settle it.

As for the compressive pattern previously observed along the Belledonne fault (e.g. Delacou, 2004) it appeared controlled by very few compressive mechanisms, which were not well resolved. Our study adds both strike-slip and compressive mechanisms in this area. However the few compressive solutions are less well constrained than the strike-slip ones, which explains that compression almost disappears in the statistical approach.
Lastly, the compression south-west of the Alpine arc disappears from the inversion results due to the higher number of equally robust strike-slip mechanisms compared to reverse ones (Figure 3).

- *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 strikeslip 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 over-all strike-slip regime. Still the question remains why there is little spatial correlation between "extensional patches" and surface uplift. Also, you should compare your re-sults 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.*

We added the following paragraph to the discussion: "**The contrasting juxtaposition of extension and compression occurs in a region of complex geometry and complex processes related to the Alpine orogeny (plate boundaries, Ivrea body). It has sometimes been interpreted in the literature as a border effect of gravitational collapse of the Western Alpine chain (e.g. Delacou, 2005), as well as a marker of indentation resulting from Africa-Eurasia current plate dynamics in other studies (e.g. Eva et al., 2020). In both cases, sharp variations in the stress field are expected. In the case of the gravitational collapse for instance, stresses depend on the spatial derivative of the load, and to some extent varies like the derivative of the crustal thickness and topography: clearly, these variations occur on short spatial scales. While we cannot decipher which processes are at the origin of this very specific and local pattern, we stress that too few compressive patterns are retrieved at the border of the chain to support the gravitational collapse scheme, and that indentation by Adria or Corso-Sardinia blocks does not appear to be the main process controlling crustal deformation nowadays in the Western Alps, since a majority of extension and strike-slip mechanisms are found, drawing a self-consistent transcurrent deformation field over the whole Western Alpine region.**"
As for the occurence of both extension and strike-slip, these features set up the main finding of our paper as stated at the end of the reviewer's general comment. We indeed explain this particular transcurrent pattern as the result of the interaction between (i) far-field forces related to the present-day rotation of Adria wrt stable Eurasia, which results in the homogeneous strike-slip field observed at the regional scale, and (ii) intrinsic forces possibly related to isostatic

processes and slab dynamics, which can result in the small scale spatial variations of extension that are retrieved in the core of the belt.

Last, we do agree that a final sketch will make our point clearer. We added in the revised version of the manuscript a 3D sketch to the discussion section, summarizing our main findings and how they could relate to the main processes possibly involved in crustal deformation in the Western Alps. Regarding the spatial decorrelation which is evidenced in our work as in other previous studies between maximum of deformation (i.e extension, whether geodetic horizontal component or seismic deformation) and vertical uplift measured from geodesy, we suggest that extension, which is imaged both by geodesy and by seismotectonics with a maximum in the South-Western Alps, could be generated by the same processes involving uplift, even if it appears spatially decorrelated from uplift maximum. The aforementioned processes (isostatic adjustments and possible slab detachment) may thus infer crustal deformation which is released both seismically, through extension, and aseismically, through uplift. Seismic deformation would thus locates preferentially in the inner zones, along the two main seismic arcs inherited from the complex orogen history of the Alps, while aseismic deformation locates preferentially on the North-Western part of the Alps, possibly due to additional short wavelength processes such as differential erosion and/or differential ice mass loss.

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:

We took the suggested corrections into account for the revised version and answer below to the various interrogations of the reviewer:

- 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?

We added the exact period of the catalogue which is from 1989 to 2013. The Mc was estimated to 1.15 for the whole Western Alpine region based on a Gutenberg-Richter analysis of the complete dataset corresponding to the map in Figure 1 .

- 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.

The minimum number of one S wave criteria is applied in order to ensure to select well located events, especially in terms of depth constraints. Amongst the computed focal mechanisms, only 4 have multiples with two possible solutions. These events are marked with a star flag (*) in the revised appendix of the manuscript.

- 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?

We indeed use two levels of polarity uncertainty (U/D and +/-) in the focal mechanisms inversion. We did not use any weighting for take-off angles since localisations, take-off angles and azimuths were computed in the same 3D velocity model and are therefore all of the same order.

- 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)?

Ml have been recomputed based on events relocation. However the bias that would have been introduced if it had not been done would be small (average of the location differences is 5 km, see Potin, 2016) compared to the original uncertainties on the magnitudes. The computed Ml is defined by Richter (1936), and is indeed simply the peak amplitude but on all three components, since for some shallow events amplitude is maximum on the vertical component.

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

We rephrased it as follows: "**One can use ternary diagrams to graphically represent intermediate faulting styles between pure strike-slip, pure normal or pure reverse motion.**"

- 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?

The recent work by Laurrendeau et al. (2019) has been demonstrating that already established relationships (Cara et al., 2015) are estimated at the national scale and do not take into account regional variations in crustal attenuation characteristics. This work also demonstrates that events in south-eastern France, based on events available in both Ml and Mw, are closer to a 1-1 relationship than to previously established relationships (Cara et al., 2015) which tend in this region to underestimate Mw of events with Ml > 3.0.

-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?

We choose not to apply any weighting for fault planes, following previous studies in this region (e.g. Delacou et al., 2004) since we work at the regional scale with small magnitude earthquakes for which faults are often not identified.

-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?

We took into account the fault plane uncertainty as input error on our focal mechanisms. **We were able to assess through the Bayesian inversions that these formal errors are overestimated by a factor of ~ 0.6. This is now specified in the revised version of the text.**

- 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?

The stress inversion in the seismotectonic zonations is a different procedure than the Bayesian interpolation of P and T angles. It is therefore difficult to directly compare results and uncertainties associated with the two different approaches.

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

Indeed. We rephrased it to "**The least compressive stress axis (σ3) presents quite similar azimuth values [...] except [...]**".

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

It meant the strike of the belt versus the strike of the fault indeed. We rephrased it to "[..] **with a rotating state of stress compatible with dextral motions on faults parallel to the strike of the belt (such as the Belledonne fault, Thouvenot et al., 2003)** ".

- 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?

As explained section 2.2 (l.153), a focal mechanism cannot be reduced to only 1 dimension. While Delacou et al. (2004) simplified the style of deformation to one dimension, we consider here, in addition to purely compressive or extensive, also purely strike-slip as well as intermediate transtension and transpression motions, thus requiring at least two axes to describe the focal mechanism. We thus refer to the ternary diagram for the meaning of the 2D color code.

- 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).

The depth north of the Swiss Valais is as well constrained as in any other part of the crustal velocity model covering the Western Alps, thanks to a high density of swiss stations and to the high quality of related seismic records. While depth uncertainty is a research topic by itself, we remind here that the locations in the 3D velocity model are constrained by both P and S waves and differ, on average on the whole area, of about 5 km (in 3D) from the locations previously estimated by Sismalp in a 1D velocity model (Potin, 2016). This specific area is however complicated as stated in the above comment and we now refer to Diehl et al., 2018; 2021 in addition to Maurer et al (1997) and Eva et al (1998) to further help readers specifically interested in these local scale features.

- 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.

The dots indeed refer to the focal mechanisms used in the inversion in order to highlight areas well-constrained by data. We made the corresponding symbols bigger. We refer to Figures S3 and S4 for robust versus extrapolated values. The location of geological massifs are indicated by acronyms, while the location of geophysical characteristics are indicated on the tomography models on Figure 10.

- 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?

The inversions were indeed also performed for different depth intervals. The corresponding interpolated deformation fields (mean of the probabilistic distribution) were added to the supplementary material to help the reader decipher which mechanisms, at which depth, most constrain the surface-projected reconstruction of the deformation field shown in Figure 7b.

- 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.

"... follow the structure of the European crust in terms of dip".

- 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.

Yes it does. This point is now enhanced in the discussion as developed in the answer to specific comment 3) above.

- 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?

The focal mechanisms in all cross-sections are represented as cut along the profile. Ok for caption 10b : "Dashed lines represent the European and Adriatic Moho after (Solarino et al., 2018). "

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

replaced by "dextral motion along the strike of the belt."

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

We rephrased it to "robust transcurrent motion"

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

The Adriatic plate rotation is suggested to add a strike-slip component to the observed deformation field. We rephrased it to "The counterclockwise rotation of Adria with respect to stable Europe (e.g. Calais et al., 2002; Serpelloni et al., 2005, 2007) largely counterbalance buoyancy forces (Delacou et al., 2005)".

- 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?

As explained in the discussion section, the processes driving seismicity and deformation in low deforming areas, and especially within the Western Alps, is a long ongoing debate. While some studies (e.g. Calais et al., 2002) suggested that crustal deformation could be related to the sole active plate-tectonic processes, other studies suggested that surface- or slab- related isostatic processes could also explain some crustal deformation and seismicity (e.g. Sternai et al., 2019, Mazzotti et al., 2020, Salimbeni et al., 2018, Eva et al., 2020). The main finding of our study, as summarized in the general comment of this review, is that both active plate tectonics and other neotectonic processes must be involved to explain the observed deformation field. We made this point clearer with the final sketch and the corresponding discussion in the revised version.

- 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.

The relocalized catalog will be the object of a specific publication, to which we will refer in the final version if available.

- 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.

We added the DOIs to the citation of the national networks when available.

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