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Interactive comment on "Seismicity and seismotectonics of the Albstadt Shear Zone in the northern Alpine foreland" *by* Sarah Mader et al.

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Comments to the paper: "Seismicity and seismotectonics of the Albstadt Shear Zone in the northern Alpine foreland"

by Sarah Mader, Joachim R. R. Ritter, Klaus Reicherter and the AlpArray Working Group

(Submitted to Solid Earth)

Reviewer: Thomas Plenefisch

The paper of Mader et al. is a classical seismicity and seismotectonic study. It focuses on the region of the Albstadt Shear Zone (ASZ), an area in SW Germany situated be-

tween the Black Forest and Rhine Graben in the west, the Alps in the south and the Franconian Jura in the northeast. From the seismicity point of you it is an intraplate area of moderate and permanently ongoing seismic activity. With an earthquake of magnitude close to 6 in 1978 the Albstadt shear zone comprises one of the two strongest earthquakes in Germany and its border regions over the last 50 years. Therefore, the region is also of great interest with regard to the assessment of seismic hazards in southern Germany.

The paper is divided in two parts. The first section is about the relocalization of the seismic events in the Albstadt Shear Zone from the time period 2011 to 2018 and the interpretation of the resulting seismicity pattern. Therefore, Mader et al. use the already existing catalog of phase picks of the state earthquake service of Baden-Württemberg and extend the dataset by phase picks of waveforms from the AlpArray network and from an project of the authors themselves, the so-called StressTransfer seismic network, which both have stations around the ASZ. The entire dataset of phase picks is then used to invert for a new minimum 1D model of P and S velocities as well as for station delay times. The final step is the relocalization of the earthquake dataset with the NonLinLoc program by using the new 5-layered velocity model. In the second part polarities of P phases and in some cases also for SH phases are used to invert for the focal mechanisms. Therefore, Mader et al. apply the FOCMEC-program. Since the magnitudes of some events are relatively small and often the signal-to-noise ratio is low the authors build clusters of events of narrow spaced hypocenters. All picks of one cluster are then used together in the inversion, quasi like a composite fault plane solution. In this way they calculate focal mechanisms for 36 earthquakes. However, do to the cluster approach this procedure results actually in 14 independent solutions.

The two subjects of the paper, firstly the relocation of earthquakes in an relatively small, intraplate area, the Albstadt shear zone in the Swabian Jura, with the aim to better resolve individual parts of the entire fault zone and secondly the focal mechanisms determination and its interpretation in the seismotectonic and geological context are of general interest for seismologist, but also for a broader geoscientific community and thereby an interesting topic for Solid Earth.

The paper is clearly written and well structured. The input data seems to be of good quality and the applied techniques as well as the results sound to be reasonable for me in most parts. The first part of the paper comprising the calculation of the minimum 1D model as well as the relocalization is completely convincing me, I have no idea of any criticism. However, I have not so good feelings with the second part of the paper that deals with the focal mechanism and stress field determination. From my point of view therein there are some points which deserve an improvement or some more clarification respectively:

1. Focal mechanisms and stress field in SW Germany: When talking about stress field in SW Germany (line 83 – 86) it would be appropriate from my point of view to also reference the paper by Plenefisch & Bonjer (1997) and Bonjer (1997) who inferred the stress field of the Southern Rhine graben area by the inversion of 40 focal mechanisms determined by themselves (Bonjer 1997).

2. Presentation of the FOCMEC results: I wonder why the authors do not show any figures with the original output of the FOCMEC program, this means stereographic plots with used polarities and the calculated fault planes that are in accordance with the polarities. This is usual practice and gives the reader an impression about the distribution of the input data and the resulting and suitable pairs of fault planes. I suggest here to show a figure with the results for at least two or three earthquakes with different quality factors (after Table 3). The solutions for the other events or clusters respectively could be given either in the supplements or event better - since altogether these are 'only' 14 solutions - in one comprehensive figure in the paper itself. I think this is a must. I am aware of the fact that figure S5 shows the uncertainties in strike, dip and rake. This is a nice figure, but it does not replace the figure I proposed above.

3. Relative weighting and unity weighting: Please, describe shortly in the text, what is

the difference between these two weighting approaches.

4. SH polarities: In line 306 the authors state that in some cases they could determine SH polarities. After all these are 27 events according to Table 3. Since these SH polarities could be determined I assume that one could also determine amplitude ratios of SH/P and use them as input in the FOCMEC inversion. The use of amplitude ratios is scheduled in FOCMEC. It puts further constraints on the solutions and provides an easy way to determine one single solution by minimizing the differences between measured and calculated ratios. Why didn't you use such ratios, please comment on this topic. Perhaps the use of amplitude ratios could help in case of a small number of polarity observations and could made cluster analysis needless.

5. Missing a real inversion for the stress tensor: P-, T-and B-axis of a focal mechanism represent the strain axes. Only in case of a new fracture the strain axes automatically represent the principal stress axes. However, in case of a preexisting zone of weakness, which is commonly assumed for small size earthquakes, P-, T- and B-axes do not usually represent the principal stress axes. It is only the direction of the slip vector which is constrained by the orientation of the principal stress axes and the relative stress magnitude. An inversion of an ensemble of slip vectors finally allows the determination of the stress field and the strike of minimum or maximum horizontal stress axis respectively. I am not really convinced by the way Mader et al. determine their orientations of SHmax. I wonder why they have not performed a 'real' stress inversion of their focal mechanisms. There are several programs to do this (e.g. Gephart & Forsyth, 1984; Rivera and Cisternas, 1990; Michael, 1984; Hardebeck & Michael, 2006). I think this should be done before digging deeper into the interpretation of the stress field in and around the ASZ, even though I assume that the results will not dramatically change. The diversity of the focal mechanisms of the individual clusters which is obvious from Fig. 7 and Table 3 fulfills one essential requirement of a successful stress tensor inversion.

From my point of view the paper clearly deserves publication in Solid Earth after some minor revisions or comments which I have listed above.

Please also note the supplement to this comment:
https://se.copernicus.org/preprints/se-2020-167/se-2020-167-RC2-supplement.pdf

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