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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.

**Anonymous Referee #1**

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The paper, " Seismicity and seismotectonics of the Albstadt Shear Zone in the northern Alpine foreland" by Mader et al., presents a study of the seismic activity in an intraplate region affected by regular low and moderate seismicity in Germany. They take advantage of the recent densification of the regional seismic network, in the framework of both the AlpArray European project and a local project specifically focusing on this area. The paper is well written and both the methodology and the results are well introduced. I suggest some corrections to improve some parts of the manuscript, and I make below some comments that the authors should discuss in the text. I believe that this study is important to get a better perspective of the origin of the seismic activity in such regions which remains poorly understood.

Regarding the region under study, I would recommend to the authors to give more



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informations on the initial catalog. First, some more details on the quality of the locations, especially at depth (fixed or not), and the magnitudes. Second, it is important to mention if the catalog is discriminated with natural and anthropic events, and how. This is a crucial point. The catalogue used in this study corresponds to the initial catalog of the LED, complemented by additional pickings. I understand that this would represent another significant work, but I found a bit regrettable to not used the dense temporary network to decrease the completeness magnitude and enlarge the catalogue to get a better spatial distribution of the seismicity with a lower magnitude. Using a simple detection approach and the semi-automatic manual picking would help to improve the dataset. In general, the discussion regarding the magnitudes is lacking (also in figure see Figs. 7) In Fig. 1, I am surprised to see so many stations with very few picks. In Figure 5 as well, the low number of picks (P+S) per event is striking taking into account the density of the network. I suggest to add the focal mechanisms with the polarities in the Suppl. Mat. Within the text (section 4.4), I would recommend to modify a bit around L. 311, because we first think that the authors are doing composite mechanisms. The authors should mention in the text if the phase amplitudes are also taken into account in the method they used to determine the nodal planes. In section 4.5, all the arguments to chose a strike-slip regime should be included here. I am not very convinced by the method followed to determined ShMax, which is mainly appropriated for large events. For micro-seismicity, it seems more relevant to consider the stress field in the area under study homogeneous and conduct a inversion of the whole set of focal mechanisms (e.g., Rivera and Cisternas, 1990; Julien and Cornet, 1989, Michael, 1984, 1984; HardebeckÂăand Michael, 2006). I am not sure that the spatial variations of the Shmax direction can be relevant here, as mentioned by the authors. The use of this large diversity of focal mechanisms probably helps to constrain a stress regime and stress direction. The part of the discussion dealing with the fault plane solution is unclear. Looking at the Fig 12, it seems that reverse slips occur mainly on the most dipping planes, which can be surprising, and the authors should explain what his the main point of this figure. I found that the last section of the discussion could have been





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strengthened. After reading it, we do not see what is the contribution of this work to better understand the stress field of this region.

Figures: Figure 1: This figure should be improved by increasing the size of the maps and by adding the corresponding geological background (transparency on shaded topography) since it is quite developed within the texte (section 2). The epicenters are either marked by circles or gray dots, why? I would recommend as well to change the topographic background by adding an illumination on the slope gradients. Add a rectangle on (a) corresponding to the zone of (b). Add Lake Constance. The magnitude scale is not appropriate since the large events are indicated by stars. More structural and tectonic details on (b), it remains unclear. Figure 2: the limits are confusing. I would recommend to keep the same frames than Fig. 1a. Also, the same colors used in Fig. 1a for the stations are also confusing. Use also a shaded topography. Indicate the station MSS (the white triangle is not visible in Fig 6). Figure 7: the magnitude scale is required Figure 8: indicate the area of study and indicate differently your data. Figure 10 and 11 should be combined into only one.

Minor comments: L. 14: tone down this sentence by removing ÂńÂăonlyÂăÂż, some geophysical imaging and paleo-seismological studies could help as well. L. 45: major damage: what kind? L.47: its ÂńÂăgeometryÂăÂż. Unclear, if you mean geometry of the seismicity, this is not correct. I would change into ÂńÂăto derive the geometry, the segmentation of the faulting patternÂăÂż. L. 49: you mean ÂńÂăprior large earthquakesÂăÂż? L. 51: add 'temporary' L. 58: remove minimum here, this is confusing for the Introduction L. 59: change geometry of the ASZ into ÂńÂăthe geometry of the fault pattern at depth in the ASZÂăÂż L. 60: remove ÂńÂăpermanentÂăÂż L. 84: It is not clear if it is the current or past stress field ? L. 94: The direction of the current principal stresses are constant, the amplitudes are not constrained. L. 104: ÂńÂăen echelon features compensating the displacements of the ASZÂăÂż: I am not sure to understand. L.112: ÂńÂămoderate displacementsÂăÂż : recent, Quaternary? L. 115: Add ÂńÂă(Fig. 1a)ÂăÂż after Lake Constance. L. 116: change ÂńÂălimitÂăÂż into

ÂńÂălimitedÂăÂż L. 116 to 119: these lines should be reformulated, they are unclear. L. 127, 173, 435 + caption_Fig12: remove ÂńÂăthÂăÂż and ÂńÂăndÂăÂż to the dates. L. 128: add ÂńÂărespectivelyÂăÂż. L. 128-129: ÂńÂăThe average. . .ÂăÂż. This is co-seismic slip rates. Mention from which data they are deduced. The same for the return period, mention briefly how this is estimated. L. 132: change ÂńÂăin a depthÂăÂż into ÂńÂăat a depthÂăÂż L. 134: the term ÂńÂăextensionÂăÂż is confusing here (lateral extend. . .) L. 142: ÂńÂăat the beginning of the summerÂăÂż. L. 158-159: Clarify ÂńÂăthe error boundaries are checkedÂăÂż. L. 180-181: give more explanations. L. 206 : ÂńÂăa few. . .ÂăÂż L. 222: ÂńÂălayersÂăÂż L. 226: ÂńÂăno eventÂăÂż L. 239: . . . the Stress Transfer stations from 2018ÂăÂż L. 367, 368: writhe the same number of decimals. L. 429-431: not necessary. And check consistency with the Fig 12, where rake varies between -65 and +65°, why?

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