EGUsphere, referee comment RC1
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Comment on egusphere-2022-446

Friedrich Hawemann (Referee)

Referee comment on "Shear zone evolution and the path of earthquake rupture" by Erik M. Young et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-446-RC1, 2022

Young and coauthors present a study of a well exposed core of a shear zone. The main hypothesis of the manuscript is that preexisting lithological heterogeneities on a small scale guides the rupture geometry of earthquakes. The manuscript is well written and well structured and proper observations are made and appropriate methods to underpin the interpretations are used. The systematic approach and methods used for the quantification of the geometry are novel in this field of study. The topic is definitely very suitable regarding the scope of the Solid Earth journal. Before publication, I would like to request revisions on the raised concerns regarding the timing of deformation and associated geometries as well as the careful remake of some figures.

Friedrich Hawemann

General comments

The topic of where seismic rupture takes place is of high importance, and the idea that pseudotachylyte can often be found at rheological boundaries is valid, and has been observed before, also as bounding boudins (Toy et al 2011). In my opinion, this is not the best field site to establish this relationship, as the ductile deformation following pseudotachylyte emplacement is significant and alters the original geometries. The ultramylonite bands wrap around the pinch and swell structures and boudins, and therefore the pseudotachylyte generation predates these features. However, the authors argue that the stress concentrations necessary to form the pseudotachylyte, are generated by the pinch and swell geometries (line 540).

Maybe it is possible to explain the observed geometries and distribution of pst like this:

Pseudotachylylte is generated along a lithological boundary. Pst are weaker than the host rock(s) during subsequent ductile shear, effectively lubricating the boundary and facilitate the formation of pinch and swell structures.

Furthermore, the thickness of the former pseudotachylytes is quite astonishing, considering a formation by a local stress variation. Also, pseudotachylytes generated by local stress variations are more likely to crosscut the mylonitic fabric, and this should be still visible even after further ductile overprint (Toy et al 2011, Hawemann et al 2019, Campbell et al 2020).

Also, the analysis of the interfaces of pst-bearing and non-bearing contacts is hard to validate, as present-day geometries are not the geometries at the time of emplacement of the pseudotachylytes.

I would therefore recommend either a thorough and robust explanation of the timing of the structures or a significant reinterpretation of the observed geometries before publication.

Minor comments.

The introduction has to be extended towards the literature existing on the topics handled by the manuscript.

It is also surprising to never see new pseudotachylyte form next to a previous generation of pseudotachylyte, as they also should offer a high competence contrast.

Please also note the annotated pdf with comments!

The descriptions and interpretations in this manuscript are very well and carefully executed. But they have to be supported by higher quality figures. Here are some comments on how to improve the figures, some pictures may have to be redrawn or retaken.

Figure 2:

The legend for the stereonet is bigger and placed more prominently than the stereonet itself. Change 000 to “N”. Try to place the scalebar more intuitively, for example just use a 1 m scale on the map itself.

The map itself has a bit unpleasant colours. The background drone image is not advantageous as a background here, as it does not add any information. Maybe try to give some transparency to the geology-layer, also makes the colours more digestible. The contacts are all jagged lines, sadly. A lot of orientation data is presented, but does not add all too much, as this information is nicely shown in the stereoplot already. Maybe you could trace some foliations, would offer a more intuitive way to look at it. Also, some layers are offset by the late fractures, these should therefore be included into the map.

As the map is really a crucial part of this manuscript and is presented as “highly detailed”, it would be great to see it being improved and more carefully prepared.

Figure 3: “All photos oriented with dominant foliation (NW) across the photo.” That still leaves two possible orientations. Otherwise the photos are okay, even though the various tools for scale fill up a significant part of the image for no good reason – something to consider in the next field season.

Figure 4: What is the orientation of these images?

Really low contrast in SEM Images, very dull, out of focus polarization microscope images.

Figure 6 a: out of focus and the PPL image shows some ghost topography- looks like the Analyzer was not retracted completely. Remove the dashed line along the mylonite-ultramylonite boundary. A white number 8 got lost in the upper part of a).

Figure 6a:

I think it is a very good idea to make this list and show the observations in the samples and I don't doubt a pst origin for these ultramylonites. However, the quality of the images and their scale often make it impossible to really support your interpretation of the microstructure.

(2) Flow banding is not easy to argue for here, I think, as it is parallel to the foliation.

(7) It is not very obvious that this is a polycrystalline clast. It looks to me like a feldspar with inclusions.

(3) To me the boundary looks parallel to the host rock foliation.

(5) I cannot see that on this scale.

Remove the dashed lines in e and b.

References used in comments

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Please also note the supplement to this comment:
https://egusphere.copernicus.org/preprints/2022/egusphere-2022-446/egusphere-2022-446-RC1-supplement.pdf