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Comment on egusphere-2022-446

Simone Papa (Referee)

Referee comment on "Shear zone evolution and the path of earthquake rupture" by Erik M. Young et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-446-RC2>, 2022

Review of "Shear zone evolution and the path of earthquake rupture" by Erik M. Young et al.

In this manuscript, Young et al. integrate field and microstructural mapping of a seismogenic fault core exhumed from the base of the seismogenic zone. They focus on the distribution of earthquake slip surfaces to understand what dictates the path of the earthquake rupture within a mylonitic shear zone. They conclude that stress heterogeneity at the scale of the lithologic layering is the most important factor controlling the path followed by the earthquake rupture.

The central question of the manuscript, i.e. what controls the path of earthquake rupture, is certainly an important one in earthquake mechanics and the methods used to address it are appropriate and with regard to the quantitative mapping of contact shapes (paragraph 4.2) are also novel. The manuscript is very well written and organised, the data presented are of good quality and substantially in agreement with the conclusion drawn by the authors, although I suggest more caution should be exercised in the interpretation and certain limitations should be discussed further. The new data and field observation, the quantitative analyses and related discussions certainly provide advances within the field and therefore I consider the manuscript fit for publication, provided that the following issues are addressed by the authors.

General comments

- The highlight of the paper is the detailed field mapping of an exceptional exposure and related analysis of which surfaces are preferentially exploited by pseudotachylytes (i.e. seismic ruptures). The authors show persuasive evidence that pseudotachylytes occur preferentially along pre-existent interfaces and in particular along interfaces with the highest viscosity rocks and viscosity contrast. They also show how pseudotachylyte-bearing interfaces are geometrically different from pseudotachylyte-absent interfaces. This last finding however is of difficult interpretation, since the present-day interface geometry is certainly not the same as it was at the time of pseudotachylyte emplacement. In chapter 5.2.3 the authors discuss this uncertainty and consider “the possibility that the observed pattern may be the result, rather than the cause, of seismic slip”. Furthermore, at line 485 and following, they also consider the possibility that “the enhanced long-wavelength roughness is caused by the pseudotachylyte itself ... that interferes with the progression of boudinage along an interface”. In saying so, the authors acknowledge that boudinage may actually follow pseudotachylyte formation and not predate it. In the light of this, I do not see why the authors in their conclusion suggest that stress concentrations that dictate the path of seismic rupture are caused by pinch-and-swell geometries, although there is no clear evidence that pinch-and-swell geometries were present at the time of pseudotachylyte formation. In chapter 5.2.2 the authors argue that the patterns of pinch-and-swell layering imply an heterogeneous stress distribution, not that they cause it. The authors should clarify that the stress heterogeneities dictating rupture path are the result of the viscosity contrast across the interfaces and should be more cautious when interpreting pinch-and-swell geometries as the cause of stress heterogeneity. Or at least they should discuss this matter further, as they have done in chapter 5.2.3 for interface geometry.

- Several times throughout the paper, the authors refer to interfaces that juxtapose similar wall rocks (e.g. in Table 2 and at line 319). It is never explained what these contacts are and what they look like since they are not shown in any figure. The second part of table 2 seems to imply that pseudotachylyte segments with similar wall rocks are actually exploiting some kind of interfaces within these lithologies (e.g. foliation planes, c' surfaces, fractures, joints ???). Otherwise I do not understand what the “total length of contact type” for similar wall rocks (sw) stands for and why you calculate what percentage of these contacts is decorated by pseudotachylytes. This is an important point to clarify because at present it is not clear if segments of pseudotachylytes with similar wall rocks are crosscutting intact rock (as stated at line 483) or are exploiting some pre-existent interface (as implied by table 2).

- The authors repeatedly state that the map is highly detailed and accurate to 1 cm. However, most of the layers in the map are much thicker than a few cm and not many centimetric layers are mapped. For example, are centimetric layers like those shown in Figs. 3b and 3e actually mapped?

Specific comments

Line 38 (e.g. Swanson, 1998 ...)

Campbell et al. (Scottish Journal of Geology (2019) 55 (2): 75–92.) could be an appropriate paper to cite here as a field study that deals with relationship of seismic rupture with foliation and lithology in an amphibolitic shear zone.

Lines 71-72 “Both the pseudotachylyte and dynamic breccias have been shown to be mutually crosscutting with mylonitic foliations”

Is this true also for the outcrop studied in this paper? If so, it has an important bearing in your observations and discussions. Maybe an example could be shown in a figure.

Line 103 "1-50 cm-thick layers"

Throughout the paper inconsistent ranges of thickness for the layers are reported (e.g. 1-50 cm at line 103, 20-180 cm at line 111, 2-60 cm at line 411). Actually, the quartz mylonite layer in the right-hand-side of the map is more than 300 cm thick!

Line 113 "< 20 cm thick bands of nearly constant width"

Although commonly < 20 cm thick, black ultramylonite layers in the map are locally up to 50 cm thick. Also, I do not agree that they have constant width, to a visual approximation they seem to pinch and swell like the others.

Line 126

The term quartz mylonite has been extensively used in the literature to define mylonitic quartz veins or quartzite, mostly made of pure quartz (e.g. Mainprice and Casey, 1990, *Geophys. J. Int.*; Ralser, Hobbs and Ord, 1991, *J. Struct. Geol.*; Grujic, Stipp and Wooden, 2011, *Geochem. Geophys. Geosystems.*). To avoid confusion, I would suggest to use a different name, maybe quartz-rich mylonites?

Line 148

Porphyroclastic mylonite is referred to as "porphyritic" in figure 3b and caption.

Line 169 “only lithology observed cross-cutting foliation planes of other mylonites.”

This is an important observation, it would be nice to have a figure showing this in the paper.

Line 172-173 “... photomicrograph mosaics ...”

I suggest to provide at least an example of the photomicrograph mosaics and the result of image analysis in the supplementary material.

Line 220

Characteristics 2 and 6 seem a bit contradictory (preserved flow banding (2) and absent compositional banding (6)).

Lines 243-244

7.75/**11** and 2.5/**11** instead of 7.75/**10** and 2.5/**10**

Line 245 “coarsening of pseudotachylytes”

Do you mean coarsening of grain size of pseudotachylytes or coarsening of pseudotachylytes themselves?

Line 278 "< 1-13"

Actually locally they are at least 50 cm thick.

Line 280

The thickness of black ultramylonite layers is locally outstanding, making them very unlikely to represent the mylonitic deformation of a single pseudotachylyte. Probably, when a pseudotachylyte decorates an interface, this becomes a preferential rupture pathway for the next one. This is in agreement to your finding that interfaces with high viscosity contrast are preferentially exploited. Maybe you could discuss this further.

Line 303

The relative competence ranking is only qualitative and, although reasonable, seems a bit speculative. To improve the robustness of this chapter I would suggest to select figures showing key contact morphologies that you refer to in the text to justify the ranking.

Line 305

Better use "amplitude/wavelength" instead of "amplitude:wavelength"

Line 325-326 "as it makes cusps...Figure 3A"

To me it is not clear where you see this in figure 3A.

Lines 329-330

Although it is reasonable that the quartz mylonite is the weakest lithology, I wonder if you have an explanation for fig. 3D, that shows the quartz mylonite boudinaged within the granitic mylonite. Should this not mean that the quartz mylonite is more competent than the granitic mylonite?

Line 332

Can you explain why you selected these segments? Was it not possible to use all the interfaces mapped to have more robust results?

Line 338

What the PSD is should be explained somewhere in the text.

Line 338

"Figure 7**c**, e" instead of "Figure 7**d**, e"

Line 363

"Figure 7**b**, **c**" instead of "Figure 7**d**, **e**"

Line 403

"**Fitz** Gerald and Stünitz" instead of "Gerald and Stünitz". Change also in the references.

Line 423-424 "the highest melting point ... occurs in plagioclase"

Actually plagioclase has a lower melting point than quartz. Check Spray (2010).

Line 429 "high frictional heating (plagioclase)"

If by this you mean that plagioclase has the highest melting point, then it should be quartz instead.

Line 540 "pinch-swell geometries, which causes stress concentrations"

Are stress heterogeneities caused specifically by pinch-swell geometries, or are they inherent to the viscosity contrast between different layers?

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