R2

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.

Thank you for your detailed and insightful discussion of our work. Please find out responses and associated changes below.

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.

This is the same comment raised by Reviewer 1 above. We have included three new paragraphs in section 5.2 describing pre-seismic (higher temperature) deformation structures which created the wavy, layered structure of the shear zone which developed before regional exhumation brought the rocks of our outcrop into the seismogenic zone, and highlighted explicitly the local low-angle crosscutting relations that also result from the pre-existing wavy structures. We emphasize that every surface in the Pofadder Shear Zone core is wavy, and that the post-seismic deformation of pseudotachylyte ranges from minor (pseudotachylyte microstructures preserved on ~100 micron scale; Fig 6) to nearly complete (pseudotachylytes dynamically recrystallized to ultramylonites and original structures destroyed; Table 1). The strength heterogeneity in the shear zone lithologies, and associated heterogeneous stress distribution, existed in the fully ductile regime and became accentuated during conditionally-brittle deformation. The alternative would be that the shear zone contained only planar parallel structures prior to activation in the seismogenic zone, which conflicts with regional observations and the lack of any older planar structures mapped in or near the shear zone core (Melosh and others 2014, 2016, 2018, Lambert 2013, Groenwald unpublished mapping).

- 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.

We have addressed stress heterogeneity throughout the revised discussion, including in sections 5.2.1: Interface cohesion; 5.2.2 wall rock strength; 5.2.3 Interface geometry, and most thoroughly in a new section 5.4 Contributions of shear zone structure to rupture propagation which explicitly outlines the contributions of viscous flow to creating heterogeneous geometry and the outcome is heterogeneous pre-stress which affects propagation of earthquake rupture.

- 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).

We apologize for leaving out a discrete definition for "like wall rock" pseudotachylytes included in Table 2. This has been added in section 3.5 and the caption for table 2. We make no inferences about the presence or absence of a pre-existing structural guide for pseudotachylytes which formed between similar wallrocks. Thus, there are no "pseudotachylyte-absent" contacts with similar wall rocks in our dataset. Instead we compare the length of pseudotachylyte within a single mylonite type to the total along strike length of that lithology within the shear zone.

- 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?

Our mapping was conducted on basemaps where it was possible to resolve cm-scale features, allowing us to trace the complete map of contacts for layers down to about 1-2 cm in thickness. However, due to the process of creating the basemap orthoimage from a structure-from-motion model, the spatial resolution of the basemap is non-uniform and this introduces some uncertainty to contact position/roughness at scales of centimeters. We have stated this more explicitly throughout the manuscript and do not rely on any data on this scale for our interpretations. For better representation of the pseudotachylyte geometries, we have now subdivided composite "black ultramylonite" layers which were previously shown as thick layers although they contain thin internal selvedges of wall rock. We hope this clears up the questions about pseudotachylyte thickness. Individual pseudotachylytes are actually on the thicker side of average (centimeters) even after ductile overprinting.

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.

We have added this citation.

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.

While no breccias are found in our detailed map area, those reported in melosh 2014 are along strike to the southwest. We now explicitly state this at line 73:

"...is abundant, in the mylonitic fault core"

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!

We have corrected conflicting values throughout.

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.

Sec 3.2 and Fig 2 have been modified to include more detail on the composite (multi-event) layers of pseudotachylyte which comprise these thicker patches of black ultramylonite. Individual layers are roughly tabular, but discontinuous, producing the pinch-swell geometry of the composite layer.

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?

Changed throughout to quartz-rich mylonite.

Line 148

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

Fixed.

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.

We have annotated figures 2 and 6 to show examples of cross-cutting.

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.

We have added examples of the SEM backscatter imagery and eds mapping used to quantify the matrix grain sizes of mylonites to the supplementary.

Line 220

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

The two criteria refer to different scales – we have modified the wording for more clarity (Section 3.4.1)

Lines 243-244

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

Fixed.

Line 245 “coarsening of pseudotachylytes”

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

Coarsening grain size of the pseudotachylytes. Text changed to:

“... formed through coarsening of pseudotachylyte grain-size.”

Line 278 “< 1-13”

Actually locally they are at least 50 cm thick.

Addressed above (Section 3.2, supplement).

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.

This is now addressed directly in section 3.2 and 3.2.4. We do see some local accumulations of thick black ultramylonites comprised of multiple parallel bands - suggesting reactivation - but these are spatially limited and contain wallrock selvedges separating the bands (see Fig 3b for example).

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.

We have increased figure call density in this part of the text to guide the reader to the appropriate material.

Line 305

Better use “amplitude**/**wavelength” instead of “amplitude**:**wavelength”

Fixed

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

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

Figure was modified, we omit the description of cusps.

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?

Competency of a layer is a function of the viscosity and thickness of the layer. The quartz mylonite layer in Fig 3d is thicker than the surrounding granitic mylonites, and since the viscosity contrast is mild, the thicker layer was the most stiff during boudinage in spite of its lower viscosity. This is now explained in section 4.1.

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?

We were limited in using segments that were either interrupted by quaternary cover, or experienced frequent changes in adjacent lithologies as some mylonites pinched out. We have added a sentence to explicitly state this in section4.2

“ These segments represent the longest mapped contacts where we have continuous control on wallrock lithology and thickness of the immediate bounding mylonite layers..”

Line 338

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

Added: "(PSD; as a measure of amplitude of the signal as a function of frequency)" to text.

Line 338

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

Fixed

Line 363

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

Fixed

Line 403

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

Fixed

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

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

As quartz and plagioclase are both very refractory and we don't see any significant difference between their behavior in the pseudotachylytes, we have generalized this text to refer to them both together. Thanks for catching the error.

Line 429 "high frictional heating (plagioclase)"

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

We have modified this statement to include the evidence for both qtz and plag abundant contacts (and maintain consistency with the corrected information above):

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?

Both! By reference to previous modeling work, we now more clearly describe our argument that the geometric effects and the viscosity contrast both contribute to enhancing stress heterogeneity (and also contribute to enhancing the other effect). This is discussed more directly in the revised discussion.