Author's Response for se-2020-146

Major Changes

Our manuscript has undergone substantial restructuring and rewriting in accordance with the detailed revisions we obtained from the two reviewers. Here is a summary of the major changes that were done:

- We have now included an explanation in the Introduction of how our new manuscript expands on the work done by Petley-Ragan et al. (2018).
- The Methods section now comes before the Results section.
- The Methods sections has significantly expanded to include SEM, EMP, EBSD, the fit to the grain size distributions and the thermal model.
- We have edited Figure 2 to include the original EBSD phase maps, CPOs and SPOs.
- We have included data from Aupart et al. (2018) on the grain size distribution in the microfractures in a new Figure 4.
- We have included a steady-state thermal model illustrating the thermal evolution of each microfracture in a new Figure 8.
- We have rewritten large parts of the Discussion to include a more accurate interpretation of the lamellae formation and coseismic fluid infiltration. A new section on the role of fluids has also been added to discuss "the bigger picture".
- The Abstract and Conclusion have been modified to incorporate the new changes and interpretations in the manuscript.

Reviewers' comments are in black.
Author's response is in blue.
Author's changes in the tracked changes file.

Point-by-point responses to Reviewer #1

Line 62 – Change "high density" to "high spatial density".
Accepted.
Line 75 – Changed to "high spatial density".

Line 65 – Delete "across the island of Holsnøy".
Accepted.
Lines 139-142 - Sentences were removed and partly rewritten at Lines 129-132.

Line 65-66 – "adjacent to both types of pseudotachylytes" Comment: Until this point, no definition of different types pseudotachylyte vein has been introduced in the manuscript. I guess the authors refer to injection and fault pseudotachylyte veins. Please explain.
We introduced eclogite and amphibolite facies pseudotachylytes in the Geological Setting section.
Line 130 - We have edited the text here to clarify that the microfractures are adjacent to an eclogite facies pseudotachylyte 1.3 mm thick.

Lines 68-69 – "The grains within the microfractures have a crystallographic preferred orientation (CPO) that is controlled by the host plagioclase on either side of the microfracture (Fig. 2)" Comment: honestly, I cannot see how Fig. 2 can support this statement and probably the authors refer to the documentation in Petley-Ragan et al. (2018). I suggest to add 2 pole figures in Fig. 2 and to expand the figure cation. Checking the original photograph of Petley-Ragan et al. it is also apparent that the current Fig. 2 report a colour-coded cumulative map including both plagioclase and K-Feldspar (not stated in the figure caption) and this information is not given.
We have now revised Figure 2 to include pole figures to clearly show the CPO in the plagioclase and K-feldspar grains.
Line 143 – The new version now references a new Figure 2.

Lines 76-77: “The mineralogy of the microfractures and their associated reaction products varies locally. Some contain .. quartz and K-feldspar.” Suggested editing: “The microfractures locally consist of quartz and kyanite, or intergrown clinozoisite, quartz and K-feldspar”.
Accepted.
Line 154-154 – Rewritten as suggested.

Lines 78-81 – “Microfracture mineralogy is found to depend on the X CO2 of the infiltrating fluid (Okudaira et al., 2016) and the orientation of the microfracture relative to the principle stress (Moore et al., 2019). The detailed evolution of the microfractures is thus dependent on a multitude of factors. Comment: this part should not stay here in the data section: should be moved to the discussion.
This has been removed from the results section.
Lines 336-361 – A new section on the role of fluids has been included in the discussion.

Lines 82-91. “Two microfractures of dominantly... MF2 experienced more shear deformation than MF1 (Petley-Ragan et al., 2018).” Comment: this part is a little confused as includes parts that should be moved to the Method section and parts that should be moved in the introductory part to illustrate the aim of the manuscript. The acronyms MF1 and MF2 are introduced here but are present in Fig. 2 which is cited before in the text at lines 69 and 71. I suggest to move the whole description of microstructures of section 3 after the Method section.
The manuscript has been restructured to take this into account.
Lines 79-127 now summarize the (3) Methods used while Lines 128-226 is now the (4) Results.

Line 86 – “(Aupart et al., 2018)”. Comment: This citation is not reported in the reference list.
The reference is added to the list.
Line 401 – Aupart et al. (2018) is included in the reference list.

Line 89 – “Both microfractures are associated with clinozoisite, quartz and kyanite growth, and only MF2 contains dolomite.” Comment: Actually, Fig. 4 show the additional presence of garnet, ankerite and titanite within the MF1 microfracture-filling aggregate.
The sentence was deleted. Since some of the mineral phases were only observed under TEM and not with EBSD, we have listed these minerals under TEM Results.
Line 155 states the microfracture mineralogy as observed with EBSD. Lines 191 states the mineralogy of MF1 while Line 210 states the mineralogy of MF2 from TEM.

Line 90 – “The lower J-index, greater misorientations and the presence of secondary fractures indicate that MF2 experienced more shear deformation than MF1 (Petley- Ragan et al., 2018).” Comment: 1) The J-index is not introduced in the manuscript and I really doubt most of the readers are familiar with this parameter. 2) The manuscript extensively refers to the previous work of (Petley-Ragan et al., 2018) that described the microstructures. I think the authors should summarize more properly the previous work without forcing a reader to continuously go back to the published paper.
We have revised Figure 2, removed the J-index comment, and added in text about the relevance of the work done by Petley-Ragan et al. (2018).
Lines 59-62, and Lines 77-78 were included to clarify the overlap with Petley-Ragan et al. (2018).

Lines 93-101 – “Methods” Comment: this section does not contain some necessary information: e.g. the method used for the analysis the bulk composition of microfracture filling and the bulk rock composition. The sentence “The mass balance was calculated in MATLAB” does not provide a great information.” Please integrate this chapter with more informative details.
The Methods section has been extensively revised, and placed before the Results section.
Lines 80-86 now describe the SEM and EMP method.
Lines 88-95 describe the EBSD method.
Lines 96-101 describes grain size distribution method (also a new Figure 4 was added)
Lines 106-112 describe the TEM method.
Lines 114-126 describe the thermal diffusion model.

Line 106: "Few grains contain single dislocation walls within their centre." Comment: Is this visible in the images in Fig. 4? If yes, please indicate the substructure with an arrow.
Dislocations are now labelled in Figure 5 (used to be Fig. 4).
Line 189 – changed to "Few grains contain dislocations."
Figure 5 now includes labels to dislocations and the intergrowth.

Line 107-108 – "that have formed a subgrain wall made up of closely spaced dislocations". Suggested editing: "that are locally arranged to form a subgrain wall". Comment: Is there a difference between a "single dislocation wall" (line 106) and a "subgrain wall"?
Suggested edit accepted. A subgrain wall is an array of dislocations that form a boundary or wall – it is made up of single dislocations. We have edited the text to help clarify this.
Line 190 – changed to "…the host plagioclase contains a high density of dislocations that are locally arranged to form a subgrain wall."

Line 121 – "The intergrowth" Comment: which intergrowth?
Edited to the Bøggild-Huttenlocher intergrowth.
Line 205-206 – Clarifies the Bøggild-Huttenlocher intergrowth.

Lines 139-140 – "The inheritance of the crystallographic orientation of the host plagioclase and its twins within the grains," Comment: as commented above, this is not really documented in the current manuscript but is probably referred to the documentation in Petley-Ragan et al., (2018). This information is largely used in the discussion and a proper documentation should be included in the manuscript.
Figure 2 was revised to show the strong CPO.
Lines 88-95 describe the EBSD method.
Lines 59-62, and Lines 77-78 were included to clarify the overlap with Petley-Ragan et al. (2018).

Lines 141-142 – "An equilibrium fabric with crystallographic inheritance is generally created by dislocation creep and grain boundary migration". Comment: I am a little confused by this sentence. The annealed microstructure of the micro-fracture is a process of grain boundary migration to minimize the strain and surface area energy. Not sure why the authors invoke dislocation creep. The inheritance of the host grain CPO could be well explained by annealing of an in-situ shattered microstructure within the microfracture (as actually suggested later in the manuscript).
We agree that the sentence is confusing and have removed the sentence.
This part of the discussion has been extensively revised.

Lines 146-147 – "Dislocation- and grain boundary migration are too slow to have taken place within this time scale". Comment: this sentence need a reference or an additional support. Actually, Bestmann et al. (2012) described suggested that dynamic recrystallization and annealing did occur in the short-lined transient of thermal anomaly associated with the frictional seismic event.
This part of the discussion is now revised to include both Bestmanns arguments for rapid healing and the possibility, raised by Reviewer #2, that the SPO was developed more slowly under the influence of a far-field stress with a principal stress axis approximately normal to the fault surface.
Lines 243-255 discuss fluid infiltration within the timeframe of the seismic event.
Lines 256-263 discuss the development of the SPO and why we interpret it to also have formed during the seismic event.
Lines 263-265 highlight the similarity of our findings with those from Bestmann et al. (2012, 2016).

Line 148 – ".. more rapid recrystallization process". Comment: more rapid than what?
This part of the discussion is now rewritten to highlight that the fluid-rich recrystallization was more rapid than dry conditions.
Line 248-249 introduces the importance of fluids in the recrystallization process.

Lines 146-152. Comment: This whole part need rewriting to better clarify the authors' thoughts.
The discussion has now been extensively revised.

Lines 243-271 were added to help clarify our thoughts.

Lines 204-205 - It is only until after an earthquake causes wall rock damage that fluids enter the wall rock through coseismic microfractures, and these fluids are likely overheated by the frictional slip (Bestmann et al., 2016). Comment: As discussed in the main comments above, the process of coseismic fluid infiltration is a relevant and intriguing issue that deserves some more estensive discussion.
The role of fluids is now discussed in the Discussion section.
Lines 243-255 discuss fluid infiltration within the timeframe of the seismic event.

Lines 208 – "Assuming that elevated temperatures lasted for up to a minute within 1 mm of the pseudotachylyte (MF1), Comment: I suggest to be more quantitative rather than roughly assuming. The temperature evolution in the host rock adjacent to a pseudotachylyte could be modelled. See my main comment above.
We agree with the author and a thermal model is now included.
Lines 114-126 explain the method of the thermal model, based on Bestmann et al. (2012).
Lines 218-226 describe the results of the our thermal model.
Line 246 explains how MF1 must have entered the miscibility gap 20 sec after frictional heating, based on our thermal model. This means that fluids must have infiltrated the fractures by this time to influence the composition of the plagioclase and trigger the formation of the intergrowth.

Lines 209-211 – "At distances greater than 1 cm from the pseudotachylyte (MF2), the wall rock experiences minor heating to a few 10_C above ambient." Comment: same comment as above.
A thermal model is now included to support this argument.
See edited Lines above.

<u>Point-by-point responses to Reviewer #2</u>

Line 9 - What important implications? Does it increase or decrease permeability and how does this affect metamorphism and rheology.
Edited to clarify that coseismic damage INCREASES wall rock permeability, enhances subsequent fluid-driven reactions and transforms rock rheology.
Lines 9-11 – Edited to "…co-seismic damage increases wall rock permeability, permits fluid infiltration and triggers metamorphic reactions that transform rock rheology."

Line 31 - Need to mention nucleation in here. Menegon et al show that the fracturing drives nucleation of new grains. It's a bit more informative than 'recrystallisation'.
Added.
Line 38 includes 'nucleation'.

Line 34 - Where does annealing fit into the scheme of modification processes? You haven't mentioned annealing before.
Changed "annealing" to "recrystallization".
Line 43 now has 'recrystallization'.

Line 46 - How are they beneficial? I assume this is the reason that you are doing this work so please explain more fully.
They are beneficial in illustrating the short-lived mechanical and thermal stress experienced by the wall rock during a seismic event.
Lines 55-56 was edited to clarify this.

Line 51 - ...to observe and presumably measure the chemical composition?
The EDX detector on the TEM cannot perform *quantitative* chemical measurements of the plagioclase. Our quantitative measurements of the plagioclase compositions comes from EMP analysis.
No edits were done.

Line 58 - Pseudotachylytes are characterised by melt. Can you link the mineralogy within the veins to the conditions of melting as opposed to subesquent recrystallisation at higher metamorphic grade conditions?
Not necessarily. We interpret the mineralogy of the pseudotachylytes to be a result of recrystallization after quenching of the melt. There may be micro- to nanoscale structures in the pseudotachylytes that originate from initial quenching but we have not found any and would be beyond the scope of this paper.
No edits were done.

Line 69 - I am not sure on the strict definition of microfracture but they usually occur as discrete fractures within the grains rather than zones with a width of several 10s microns. Consider another term rather than microfracture here.
Unless the Reviewer has a suggestion for another term, we are not sure what else to call them. They are not micro-shear zones as they don't display significant shear strain. They are elongated (most likely planar) zones of crushed and fragmented plagioclase. Microfracture 2 (MF2) displays branching asymmetrical fractures which is characteristic of a brittle rupture. We believe microfracture is the best term for these features.
No edits were done.

Line 70 - Figure 2 does not demonstrate that the grains have a CPO.
Figure 2 has been revised.
Line 143 now correctly references the new Figure 2.

Line 71 - Is the pseudotachylyte wall parallel to the margin of the fine grained feldspars zone? Figure 2a shows the SPO cross cutting the 'microfracture'. You need to relate these features to the pseudotachylye.
Figure 1 clearly shows the relationship between the orientation of the microfractures and that of the pseudotachylyte. The SPO insets of Figure 2 also contain the orientation of the elongation of the grains relative to the pseudotachylyte.
No significant edits were done as we believe the relative orientation of the microfractures to the pseudotachylyte was already clear in the manuscript and Figures 1 and 2.

Line 88 - What is this reference and how do these authors relate to your data?
Aupart et al. (2018) was included in the reference list and a new figure of plagioclase grain size distribution from their work was added (Figure 4).
Line 401 – Aupart et al. (2018) is included in the reference list.

Line 95 - What is the activation volume of the X-rays for this technique given the conditions used? Most microprobe analyses will have a micro-scale resolution which is the scale of most of your grains. What standards were used?
All information concerning microprobe analyses are now included in the revised manuscript. The activation volume of approximately 1-2 μm^3, that is why we give a range of composition values since they are most likely averaged between two or three grains. Standards used were natural wollastonite (Si, Ca), albite (Na) and orthoclase (K), and synthetic Al_2O_3.
Lines 80-86 now describe the SEM and EMP method.

Line 104 - Please provide more details on the mass balance calculations or consider uploading the script used in Matlab. Did you correct for density/molar volume of different phases?
The mass balance methods were expanded. We did not correct for density/molar volume of different phases as we excluded all other phases except for plagioclase and K-feldspar.
Lines 84-87 now clarify the mass balance method.

Line 107 - If the dislocations are in a subgrain wall then they are not 'littered' about. Consider rephrasing.
The term "littered" was removed and the sentence was edited.
Line 190 – changed to "…the host plagioclase contains a high density of dislocations that are locally arranged to form a subgrain wall."

Line 111 - K-feldspar with an homogenous composition.
Edited.
Line 194 – includes '… K-feldspar grains with homogeneous composition…'.

Line 114 - Can you show numerically that the K-feldspar has a statistically significantly greater proportion of grain boundaries compared to the other phases or to what you would expect given the volume analysed?
Our observation that there are more grain boundaries than phase boundaries is purely qualitative based on the TEM scan. It is clear the K-feldspar grains are clustered together. Any statistical analysis would require EBSD which is only in the early stages of development for such small scales. We agree this would be interesting and enrich our results but it is beyond the scope of the current manuscript.
No edits were done.

Line 116 - How robust is that obervation beyond the two domains highlighted in figure 5b? The Ca-rich area on the right hand side of Figure 5b seems to have no lamellae in figure 5a.
It is correct that not all Ca-rich domains are associated with lamella. But all lamella are associated with a Ca-rich domain. This could be a result of the orientation of the FIB foil, with the lamellae only visible in some orientations. We have edited the sentence to include clarify the "local" nature of the association.
Line 198 – includes '…the Ca-rich domains locally overlie areas with submicron lamellae'.

Line 122 - But the host is not within this compositional range (An 40 stated above). So you wouldn't expect exsolution.
Yes, that is right and we have edited our interpretations to match with Reviewer #2's argument of lamella formation as discussed in our response.
Lines 244-256 now clarify the importance of fluid infiltration in bringing Ca and altering the composition of the plagioclase to prefer exsolution.

Line 141 - Figure 3 is not relevant here.
The reference to Fig. 3 was removed.
Line 237 – reference to Fig. 3 removed.

Line 207 - Not referenced.
Aupart et al. (2018) is included in our reference list.
Line 401 – Aupart et al. (2018) is included in the reference list.