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Interactive comment on "Nano-scale earthquake records preserved in plagioclase microfractures from the lower continental crust" *by* Arianne J. Petley-Ragan et al.

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Responses to Reviewer #2

The comments made by Reviewer #2 were critical to the brevity of our manuscript and thought it lacked detailed evidence to support our arguments. However, they believe it is thought-provoking and would be an interesting addition to the scientific community. We have addressed the reviewer's comments and revised the manuscript as best we can. The main comments by Reviewer #2 were (1) the formation of exsolution lamellae in the plagioclase grains, (2) the overlap with Petley-Ragan et al. (2018), (3) the possibility of grain boundary migration post seismicity, (4) the formation of the Ca-zoning

and (5) expansion of our discussion to include some interesting points brought up by the reviewer. The following goes through these and a few other comments brought up in Reviewer #2's comments.

Both reviewers find our manuscript a bit 'too concise'. We have now expanded the manuscript, both with respect to details and figures regarding Methods and Results, and also with respect to a more comprehensive Discussion section. We have also included a thermal diffusion model as suggested by Reviewer #1 and believe this has greatly enriched the manuscript (see figure attachment).

Reviewer #2 brings up an important point about plagioclase exsolution. They argue that the exsolution lamellae in the plagioclase grains may have formed post-seismic and as a result of the Ca-enrichment in the grains within the miscibility gap (below 800°C). We thank the reviewer for having pointed out a flaw in our logic here. In our revision we follow the argument of the reviewer and propose that the observed exsolution structures probably formed at ambient temperature and over a time scale much longer than the seismic event (1000 years). This is however not a key point of the paper, and we do not feel that this reinterpretation of the exsolution structures is a problem for the main conclusion of the paper.

Reviewer #2 questions the reference of Aupart et al. (2018). We have now included a figure that shows the grain size distribution of plagioclase in the microfractures to avoid confusion with the Aupart et al. paper where some of these data were given in a diagram dominated by data from olivine and was hard to find (see figure attachment). We have also – as mentioned in the comments to Reviewer #1 – included more figures from the EBSD study (including CPO data) to avoid the dependence on results published by Petley-Ragan et al. (2018) (see figure attachment).

Reviewer #2 requested a more detailed description of the rocks, namely whether they are eclogite or amphibolite facies pseudotachylytes. The studied psudotachylytes are 'eclogite facies'. This is now clearly stated in the text. Data collection methods for

EBSD and microprobe for the mass balance are included in an expanded Methods section.

Figure 2 was an issue for both reviewers. Figure 2 is now revised to fully present the EBSD results (see figure attachment). This includes full uncropped phase maps of the microfractures and CPO diagrams to illustrate the strong host-controlled orientation of the plagioclase grains. SPO rose diagrams were also included.

Reviewer #2 additionally questions the importance of our mass balance calculation. The mass balance section is necessary to demonstrate that these microstructures form in an open system, where both H2O, but also K2O and CaO were introduced. This is important because the microstructural information shows that the open system behavior exists immediately following the dynamic rupturing, and perhaps even before the seismic slip. In other words, fluids are following very close behind the rupture tip. This is now discussed in the Discussion section.

The presence of K-feldspar and white mica was noted by Reviewer #2 who argues their significance should be discussed. It is quite clear from the petrographic observations and the phase maps (Figure 2 will now include this) of the plagioclase aggregates that the distribution of K-felspar is focused along the contact to the wall rocks of these microfractures. Presumably, this is where the fluid introduction was most intense and transport from external sources most effective. It is also clear from the CPO (revised Figure 2) that the shear strain in these microfractures has been very limited and very different for typical large strain mylonite zones. The transition from dislocation to diffusion creep with associated phase mixing requires substantial shear strain, and hence is less relevant in this case.

Reviewer #2 questions our interpretation of a limited time-scale for annealing of the plagioclase microfractures. This is a very interesting discussion and we agree with the reviewer that far-field (tectonic) stress is still probably significant after the seismic slip. However, if the SPO is developed slowly and controlled by the far-field stress,

it is interesting that the long axes of the grains are always parallel to the fault. If we assume that the long axis is most likely oriented perpendicular to the largest stress axis (ïĄş1), this observation would imply that ïĄş1 is perpendicular to the slip surface. In this case, it is hard to envisage that the fault developed as a shear fracture driven by tectonic stress (as normally assumed). We discuss this possibility in another paper under review in Nature geoscience, and - to make a long story short – we think that this is a likely scenario in the Bergen Arcs case where the deep earthquakes may have been triggered as aftershocks from large earthquakes in the shallower seismogenic regime (in which case they do not form as shear cracks controlled by the far field stress). So, although a full discussion of this is beyond the scope of the current paper, the reviewer has a good point which we will include in the discussion section.

The Ca-zoning in the plagioclase grains was brought up Reviewer #2 who requests further discussion of this feature. The plagioclase chemistry is now explicitly discussed and compositions of different plagioclase domains are given. We also discuss the open system effects on the Ca content of the 'bulk' aggregate using our mass balance as evidence.

Reviewer #2 asks if we can strengthen the argument that the fluids are derived from the melt. We do not argue that the fluid is derived from the melt. In fact, we will argue – as also suggested by Reviewer #1 – that the fluid arrives just behind the dynamic rupture and is probably present during the seismic slip stage.

Finally, Reviewer #2 asks for further discussion of the final model and the relative importance of different options. We will rewrite the Discussion and make it significantly more comprehensive. A thermal pulse may not be needed for the observed microstructures, but it is no doubt that the thermal pulse actually existed since the fault slip caused frictional melting. The goal of this paper is not to prove the obvious fact that the thermal pulse actually occurred.

The line-by-line comments by Reviewer #2 were addressed. Responses to them are included in a pdf file.

Please also note the supplement to this comment:
https://se.copernicus.org/preprints/se-2020-146/se-2020-146-AC2-supplement.pdf

Interactive comment on Solid Earth Discuss., https://doi.org/10.5194/se-2020-146, 2020.

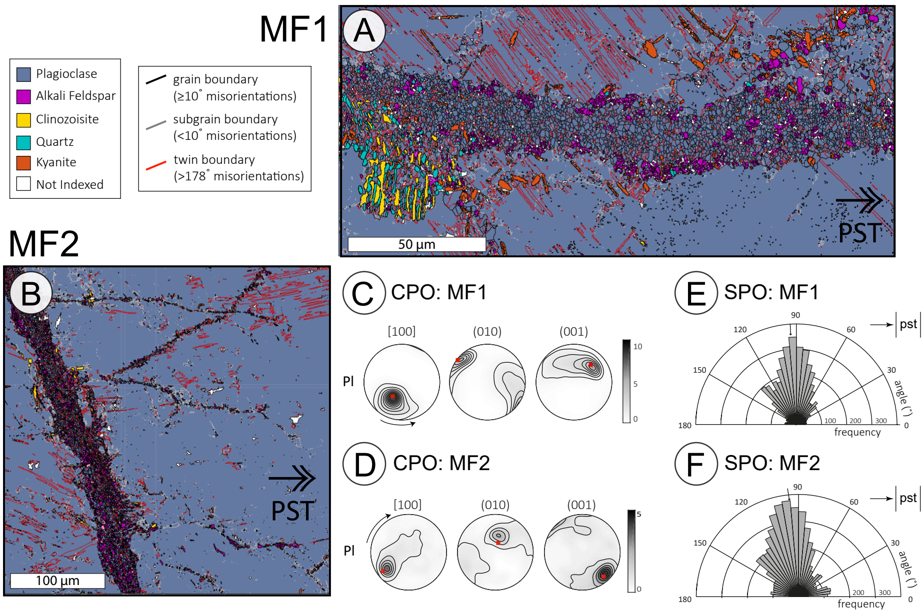
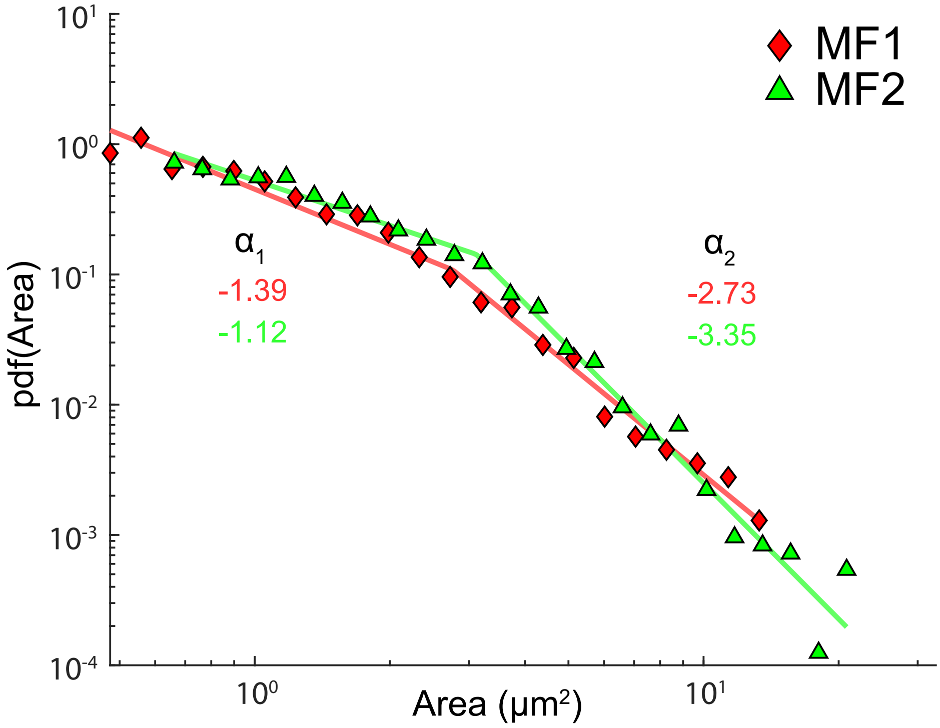
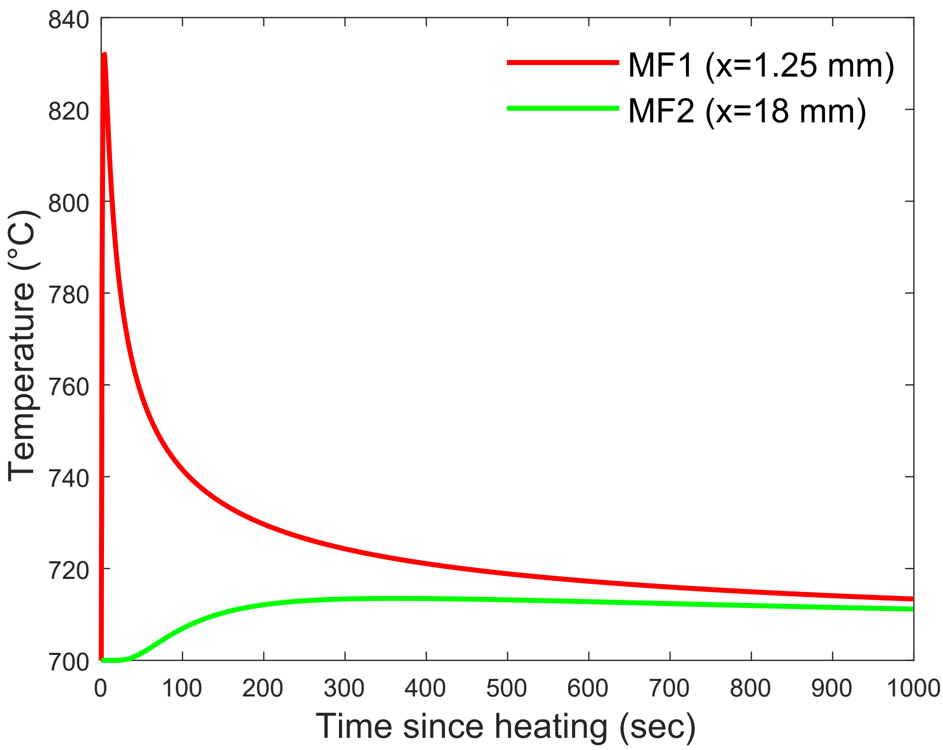


Fig. 1.



MF1
MF2
α_1
-1.39
-1.12
α_2
-2.73
-3.35
pdf(Area)
Area (μm²)

Fig. 2.



MF1 (x=1.25 mm)
MF2 (x=18 mm)
Temperature (°C)
Time since heating (sec)

Fig. 3.