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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.

Anonymous Referee #1

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This is a concise manuscript (may be a little bit too much concise) on the deformation micro-nanostructures associated with effects of thermo-mechanical processes during seismic faulting in lower crustal rocks (anorthosite) from the classic area of the Bergen Arcs. The manuscript focuses on the plagioclase "fabric" of along micro-fractures adjacent to pseudotachylytes that support the seismic origin of fracturing and fracture healing. The TEM documentation of the microstructures is superb and itself deserves publication. The recorded microstructures are amazingly similar to the quartz microstructures documented in felsic rocks at much shallower levels in pseudotachylyte-bearing faults as reported by Bestmann et al. (2011, 2012, 2016). Actually, Bestmann et al. (2012) also report local annealed microstructureof plagioclase along coseismic mi-

crofractures as well as a change in plagioclase composition (see their section 3.4: Plagioclase deformation microstructure in the Adamello tonalite); in these shallow faults (faulting estimated to occur at the base of the upper brittle crust: i.e. 8-10 km depth) and fluid-rich environment, the annealed aggregate of plagioclase (and quartz) contain abundant fluid inclusions along the grain boundaries which is not apparently a characteristic in the Lindås shattered anorthosites. As recalled in Bestmann et al. (2012), it is of note that this type of annealed fabric (in quartz) has been reported for other shallow faults (Bestmann et al., 2011) and deep-seated pseudotachylyte-bearing faults within lower-crustal rocks (e.g. Musgrave Ranges, central Australia; Ivrea Zone, NE Italy: Wenk (1978)). These observations attest the rather general occurrence of this microstructure in paleoseismic faults under different structural levels. I believe that this characteristic occurrence of annealed aggregates should be emphasized in the manuscript to give a more general significance to the study.

The manuscript is well written, but the manuscript organization could (should) be improved. There is some mixture between discussion, interpretation and data. The method section should be anticipated before the microstructural description in section 3 (which contains reference to the methodological part or parts reported in the "Methods") as better explained below in the review.

There is a clear overlap with the published work of Petley-Ragan et al. (2018). Some statements presented in the current manuscript find support and a background in the published paper. Actually I find that some descriptions/figures reported in the manuscript are not as efficient as the original text/figures in Petley-Ragan et al. (2018) and it should be avoided that the reader has to continuously go back to the published paper to get the correct information. Figure 2 is an example of such a problem: the figure is cited as evidence for the crystallographic control of the host crystals on the CPO of annealed grains in the micro-fractures, but this is hardly inferred from the reported color-coded (inverse pole figure colouring) maps (instead the original images in Petley-Ragan et al., 2018, contain pole figures where this CPO control is well documented).

Some part of discussion, that are relevant to the conclusions, are quite qualitative and there is a need to provide some more rigorous constrains to the analysis. I mainly refer to the “estimates” of the duration of the high-temperature transient (lines 205-212). It should be quite easy to calculate roughly how long the host rock of pseudotachylyte remained at high temperature performing at least a simple thermal diffusion model (not including parameters such as latent heat of crystallization, clast population etc. in the pseudotachylyte melt). To do this calculation, a missing parameter is the thickness of the pseudotachylyte vein which should be reported in the manuscript. The authors cite Bestmann et al. (2012) to constrain the speed of cooling in the host rock (Lines 206-207: “rapidly cooling back to ambient conditions at rates on the order of a few °C/s (Bestmann et al., 2012)”), but it should be considered that the calculation of Bestmann et al. is valid for the case of a host rock ambient temperature of ca. 300 °C (instead than 650-750 °C of the Lindås Nappe anorthosites) and for the specific pseudotachylyte vein thickness of 1.5 cm.

The manuscript would also benefit from a much thorough discussion of some topics that emerge from the presented data. A main conclusion of the manuscript is that: (i) fluid-enhanced diffusion during the coseismic high-temperature transient is responsible for the observed annealed microstructures within the micro-fractures; and (ii) the fluids should be already present during the thermal anomaly. I feel that this issue is not highlighted and discussed as deserved. Since the host rocks (anorthosites) are nominally dry (which explains their brittle/seismic behaviour at amphibolite-eclogite facies high-temperature conditions) fluids should propagate into the rocks coeval with the earthquake rupture and/or during seismic slip. While the role of earthquakes in producing the permeability pathways in the dry, lower crustal rocks of the Bergen Arc has been extensively discussed, the coseismic time of fluid infiltration is by far a much novel, provocative idea and an original part of the manuscript. This idea has been anticipated by previous works by Petley-Ragan and co-authors, but this manuscript provides new evidence (in the authors’ interpretation) of the process. This mechanism should be more thoroughly and clearly discussed and compared with similar or contrasting

cases in the literature. It is not always the case that fluids infiltration is so timely associated with earthquake fracturing. A rather contrasting and puzzling scenario is provided by the dry peridotites from the Lanzo Massif (western Alps). In Lanzo, seismic deformation at eclogite facies conditions (600 °C and 2.1 GPa), therefore in a comparable high-grade context as in the Bergen Arc rocks, and produced extensive pseudotachylytes and fractures, without any associated pervasive fluid infiltration that could drive metamorphic reaction and promote ductile (crystal-plastic) flow (Pennacchioni et al., 2020). This case is even more a than a puzzle, considering that the dry peridotite is embedded in serpentinite that underwent de-hydration during the peak eclogite facies conditions (and therefore a fluid source was readily available). A similar case, though likely with only a very minor influx of water fluids, is recorded in the lower crustal rocks of Lofoten (north Norway) (Menegon et al., 2017) also containing deep-seated pseudotachylytes. This prompts to the question of the origin and source of the fluid in the Bergen dry, lower crustal rocks and open a quite exciting issue on the fluid mobility during earthquakes. I warmly recommend the authors to discuss these different scenarios and the implications of their observation of a coseismic fluid percolation. This process has clear major implications on the rheology of the lithosphere (either oceanic and continental) and is therefore a relevant issue for a broad community in Earth Sciences.

Detailed comments pinned to specific parts/lines of the ms

Line 8 – “mechanical stress and thermal energy” Suggested editing: “mechanical and thermal stress”.

Line 17 – “and shape preferred orientation relative” Suggested editing: “and are elongated subparallel”

Lines 26-27 – “During continent-continent collisions, plagioclase-rich granulite- and amphibolite-facies rocks are strong, dry and prone to seismic faulting and subsequent metamorphism (Jamtveit et al., 2016)”. Comment: I do not understand the last point of the current sentence. Dry lower crustal rocks (and dry rocks in general) are strong and commonly survive metastably dramatic changes in metamorphic conditions as stated in several papers from the Oslo group and other authors. These rocks become reactive to metamorphism provided water is introduced in the system (for example along the damage zones created by seismic failure) - otherwise the rocks are not prone to metamorphism even at high temperature. Dry rocks can be so reluctant to metamorphic reaction that even coronitic reaction or devetrification and recrystallization of pseudotachylyte glass can be inhibited (Pennacchioni et al., 2020). So, I suggest to delete "and subsequent metamorphism" unless the message is better clarified

Lines 27-31 – "Plagioclase responds to lower crustal earthquakes by microfracturing and fragmentation followed by fluid- and stress-induced recrystallization (refs.). Grain size reduction by fracturing and subsequent recrystallization localizes strain in the lower crust, defining a transition from brittle to crystal-plastic deformation mechanisms with the potential to develop into shear zones" Comment: I have a similar comment as above. Fluid infiltration after seismic fracturing is not a rule of thumb in high grade rocks and grain-size reduction is not necessarily precursor to ductile (crystal-plastic) deformation by activation of grainsize-sensitive creep.

Line 62 – Change "high density" to "high spatial density"

Line 65 – Delete "across the island of Holsnøy"

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.

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. Check-

ing 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

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".

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.

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.

Line 86 – "(Aupart et al., 2018)". Comment: This citation is not reported 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.

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.

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.

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.

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"?

Line 121 – "The intergrowth" Comment: which 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.

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).

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.

Line 148 – ".. more rapid recrystallization process". Comment: more rapid than what?

Lines 146-152. Comment: This whole part need rewriting to better clarify the authors' 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.

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.

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.

References cited in this review (not cited in the manuscript):

Bestmann, M., Pennacchioni, G., Frank, G., Göken, M., de Wall, H., 2011. Pseudotachylyte in muscovite-bearing quartzite: coseismic friction-induced melting and plastic deformation of quartz. Journal of Structural Geology 33, 169-186.

Menegon, L., Pennacchioni, G., Malaspina, N., Harris, K., Wood, E., 2017. Earthquakes as precursors of ductile shear zones in the dry and strong lower crust. Geochemistry, Geophysics, Geosystems 18, 4356-4374.

Pennacchioni, G., Scambelluri, M., Bestmann, M., Notini, L., Nimis, P., Plümper, O., Faccenda, M., Nestola, F., 2020. Record of intermediate-depth subduction seismicity in a dry slab from an exhumed ophiolite. Earth and Planetary Science Letters 548, 116490.

Wenk, H.R., 1978. Are pseudotachylytes products of fracture or fusion? Geology 6, 507e511.

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