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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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The manuscript describes a TEM investigation of plagioclase microstructures from an ecologite or amphibolite facies deformation zone around a pseudotachylyte vein from the Caledonides of Holsnoy, Norway. The study combined crystallography and mineral chemistry at micro to nano-scales to reconstruct the sequence of processes that affected the damage zone of a deep crustal earthquake. The authors explore potential recrystallisation and annealing phenomena and propose a hypothesis that the feldspar microstructures result from amorphization and subsequent repolymerisation due to a combination of shear heating and advected heat from pseudotachylyte formation.

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The manuscript is written in clear English and the figures are drafted clearly, although need some modification to substantiate some of the authors' inferences (see below). There are several places in the manuscript (highlighted with comments in the attached PDF) where the authors state that "XYZ has implications for ABC" but they do not elaborate on this which makes me doubt that this is true. Overall, the manuscript suffers from its brevity and lacks the detail required to support its arguments robustly. I outline below the major opportunities for clarification that would make this thought-provoking paper an interesting addition to the debate about lower crustal deformation should the authors choose to modify the manuscript. Further minor requests for less substantial revisions are included in the commented PDF.

Exsolution distances in plagioclase The authors' discussion of the diffusion distances in plagioclase is flawed. They state that there is a miscibility gap in calcic plagioclase below 800 °C. They then proceed to calculate the diffusion distances at 900 – 1000 °C to show that the shear heating and/or fluid advected from the pseudotachylyte could heat the rock sufficiently to cause exsolution within a very short timescale commensurate with melt solidification. However, at 900 °C, there plagioclase is stable as a mixture. Therefore, no driving force exists. The authors' suggest that this is the only permissible explanation because there is no exsolution in the protolith that has undergone the same conditions of metamorphism, with the exception of the microfracture which has been heated. However, they fail to take into account the chemical difference between the protolith plagioclase and the calcic parts of the microfracture, although the spend time discussing the variation in exsolution in within the microfracture. The protolith plagioclase (An40) is not within the miscibility gap that causes the exsolution in the calcic parts of the microfracture plagioclase and therefore won't ever show the exsolution textures. Furthermore, the authors state that diffusion on the scale of 25 nm is unreasonable at ambient conditions. Using the equation of Korolyuk and Lepezin (2009) referenced by the authors it is necessary, for An70, to fix the water pressure at ∼1.25 kbar (a value required for the equation but not given by the authors) to recover a diffusion coefficient of ∼10ˆ-15 at 1000 °C. Using the same water pressure at 600°C

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(ambient temperature), the characteristic time for 25 nm diffusion is 1000 years. It is not unreasonable that these rocks spent 1000 years at ambient temperature following psuedotachylyte rupture to form the exsolution lamellae.

Lack of detail concerning data collection and analysis The manuscript builds on the Authors' existing published work, which is heavily referenced, namely Petley-Ragan et al 2018 and another paper by Aupart et al. 2018 that is not referenced. Through A superficial Google search I could only find a paper about olivine by Aupart et al 2018 and so I don't understand what this has to do with these rocks. Salient details of the rocks studied (e.g. are these eclogite or amphibolite pseudotachylytes?) and data collection methods are referenced to these papers making it difficult for the reader to fully understand how some of the key datasets have been collected. EBSD is not even mentioned in the methods section despite it being pivotal to the final hypothesis. The section on mass balance lacks detail including how the calculations were performed, what the microprobe data was used for, how the microprobe data was collected, standards used, under what conditions, and how the fine grain size was accounted for in the microanalysis. While repeated interpretation of the same datasets and re-use of the same samples is acceptable, and brevity is to be commended, more details of the analytical procedures, samples and explanation of the data need to be included.

The EBSD section needs a lot more detail because the presence of a CPO is essential to the authors' hypothesis. I looked at the EBSD data from Petley-Ragan et al 2018, but I strugged to find the datasets that are included in this paper because they are cropped version of the datasets presented in different colour schemes and the authors haven't cross-referenced the figures. There are two more significant issues with the EBSD data that need to be addressed (1) the data are presented in such a way that they do not show what they authors claim. The IPF colouring does not demonstrate a CPO and the pole figures should be included. (2) The rose diagrams presented here are identical to those in Petley-Ragan et al 2018 but the microstructures are cropped version. I find it unlikely that the distributions would be identical with only $\frac{1}{2}$ the grains

(or less in the case of Figure 2b).

Mass balance calculations The section on the mass balance calculations does not really contribute to the final hypothesis and explanation in any way and left me wondering why it was included. Due to the lack of details about how the calculations were carried out it is not possible to reproduce the calculations, however, increasing one component should lead to a reduction in the others unless there is volume gain or loss due to closure in compositional data. The authors either need to remove this section or discuss the significance of these calculations in their model.

Significance of K-feldspar microstructures and white mica Following on from the mass balance calculation, the significance of K-feldspar in the microfractures and minor K-bearing mica is not discussed. The authors assert that the K-feldspar forms aggregates dominated by grain boundaries rather than phase boundaries but do not offer any statistical analysis of their microstructures to support this. Moreover, the significance of this observation is never discussed. Phase mixing is often cited as a mechanisms for forming and maintaining fine grained aggregates during a switch from dislocation to diffusion creep in mylonites so the fact that the fine grained aggregates here are apparently not mixed may be significant.

Interpretation of limited time-scale for annealing The authors infer that, due to the SPO in the fine-grained feldspars, the recrystallisation must have occurred during the pseudotachylyte formation and subsequent cooling because a stress or thermal field generated by the slip was required. Firstly, they base this on their own work which includes the datasets presented here so the arguments are in danger of becoming circular. Even so, they do not quantitatively rule out boundary migration in the residual stress field at elevated temperatures following the initial pseudotachylyte formation. Just because some of the stress has been relieved by the earthquake slip, it does not mean that there is no differential stress applied. What are the relative grain boundary migration distances for co-seismic versus interseismic periods if the rocks are at and elevated ambient temperature?

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Significance of Ca zoning in plagioclase One striking feature of the plagioclase in the microfracture is that it is zoned chemically. This feature is discussed in respect of the presence or absence of exsolution lamellae but not with reference to the annealing and microstructural evolution post rupture. The magnitude of the compositional variations is not documented and the origin of the apparently extra calcium to form the more anorthitic parts are unclear. Are the sodic parts the same composition as the protolith plagioclase or was the chemistry of the whole microfracture altered during formation? The microstructures are similar to those commonly observed during annealing while the rocks are undergoing reaction (e.g. Pearce & Wheeler 2011 j.1525-1314.2010.00872.x; Holness & Watt 2001 j.1468-8123.2001.00015.x; Piazolo et al 2012 10.2138/am.2012.3966) or fluid infiltration. Can the authors account for this losses from the pseudotachylyte and strengthen the argument that the fluids are derived from the melt?

Clarity of the final model The discussion includes many options for how the microfractured plagioclase interacts with the adjacent pseudotachylye including thermal diffusion of frictional heat that causes exsolution as well as fluid advection from the pseudotachylyte. Aside from the above discussion that thermal pulses are not required, the relative importance of these different options is not clear. Similar confusion exists as the authors discuss the repolymerisation of the plagioclase. They suggest that maybe there are fragments that act as nuclei, nucleation on the fracture walls and nucleation in a stress field, but the relative importance of these ideas is not fully explored.

Some minor considerations: Introductory Discussion You mention many of the parameters commonly considered in controlling deformation style but not strain-rate. Since you are discussing the switch from brittle to crystal-plastic processes, to what extent is the strain-rate important?

Reference Frame and 3D significance of SPO The SPO present within the grains is convincing in the rose diagrams however the authors need to justify that it is ‘strong’. Moreover, the grains qualitatively do not appear to have a very strong ellipticity. Is there

a significance to the grains showing a SPO even though any individual grain itself is not particularly elongate? Furthermore, the grains in Figure 6 appear to be elongate across the TEM section. Do they have an SPO and if so is it stronger than in the 2D section analysed by EBSD?

Evidence of amorphous material Is there any evidence of amorphous material from electron diffraction? Konrad-Schmolke et al 2018 show remnant amorphous material to justify their hypothesis but none is shown here.

Please also note the supplement to this comment:
https://se.copernicus.org/preprints/se-2020-146/se-2020-146-RC2-supplement.pdf

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