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Interactive comment on "Fracturing and crystal plastic behavior of garnet under seismic stress in the dry lower continental crust (Musgrave Ranges, Central Australia)" *by* Friedrich Hawemann et al.

Friedrich Hawemann et al.

friedrich.hawemann@fu-berlin.de

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We want to thank Matthias Konrad-Schmolke for his critical review and suggestions. Below is a list of all comments from the reviewer (RC), answers from the authors (AC) and manuscript changes (MC).

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Reviewer 1, general comment RC: One crucial argument for crystal plasticity in garnet is the observation of dislocation walls that mark the boundary of one subgrain in the garnet crystals. The authors state that these dislocation walls are the result of dislo-

cation climb in the crystal (lines 252-253) and therefore indicate the activity of viscous deformation mechanisms in garnet. I am not entirely convinced that these dislocation walls are only produced by the migration of dislocations through the crystal, although I am not aware of studies that demonstrate neither pro nor contra arguments. The fact that the authors do not cite any references is also not helpful with this regard. However, there is evidence that such dislocation walls can be generated in undeformed rocks, e.g. during fluid infiltration, such as demonstrated in Konrad-Schmolke et al., 2018. Of course, fluid infiltration does not play a role in the rocks presented here, but other mechanisms for the formation of the dislocation walls must be discussed in this manuscript, as these structures are a fundamental argument for crystal plasticity. Furthermore, the interpretation of the presence of rotated subgrains in terms of subgrain rotation recrystallization is, in my opinion, also questionable. Konrad-Schmolke et al., 2007 demonstrate the presence of subgrains in garnets (and their slight misorientations) in undeformed rocks. In general, I think that the manuscript would very much benefit from a more indepth discussion of these features. The papers cited in this review should only serve as examples and I think that there are many other contributions to the topic that I am not aware of at the moment. However, I think the manuscript is very well suitable for publication after moderate revisions and a more thorough discussion.

AC: As noted by the reviewer, fluid infiltration does not play a role in the rocks presented here, so the mechanism proposed in Konrad-Schmolke et al., 2018 cannot be relevant in this case. The rocks we are considering here are also clearly deformed, with stresses being high enough (at least transiently) to cause fracturing of garnet. Progressive subgrain rotation by migration of dislocations into walls bounding such subgrains is a mechanism that has been very widely proposed both in the material and earth sciences. There is a large body of published work supporting and describing this mechanism – indeed as noted by the reviewer "I think that there are many other contributions to the topic". It is not the aim of the current manuscript to provide an exhaustive review be we have now added the following additional references as a representative selection: Hobbs, B.E.: Recrystallisation of single crystals of quartz. Tectonophysics, 6, 353-401, 1968. Passchier, C.W., Trouw, R.A.J.: Microtectonics (2nd Edition), Springer, Heidelberg, 366 pp., 2005.

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Reviewer 1, specific comments

RC: Line 138: if the fractures a dilatant there must be some material in the cracks. Can that be evaluated?

AC: No, these fractures remain empty, as implied by the word "unfilled" in the original text. These fractures are Mode 1 extensional fractures, which we think open during propagation of the seismic wave and immediately close, preventing any mineral filling.

MC: The word "dilatant" is perhaps better replaced with "extensional", so the text now reads "An apparent late generation of unfilled extensional fractures [. . .]". All other similar references to "dilatant fractures" have also now been changed to "extensional fractures".

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RC: What about the other, fast diffusing elements, such as Mn and Mg? Differences in diffusion lengths would indicate different diffusion velocities and thus support the idea of a diffusional modification.

AC: In Figure 4 d) we present the profiles for Fe, Mg, Mn. Fe and Mg show the same diffusion length as Ca. Mn does not show any measurable modification throughout the crystal.

MC: This observation was missing in the text, therefore we added the following sentence for clarification: "The length-scale for variation in Fe (XAlm) and Mg (XPyp) is identical to that for Ca (XGrs), whereas the Mn content (XSps) does not show any variation (Fig 4d)."

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RC: Lines 196-197: This diffusional modification is likely due to subgrain boundaries that might or might not be associated with subgrain rotations. This has been demonstrated in Konrad-Schmolke et al., 2007 (EJM). This should be discussed or at least mentioned.

AC: Since a subgrain is defined by a relative crystallographic rotation (commonly taken arbitrarily as between ca. 4° and 15°, when it is considered to be a "high-angle boundary" to a "new grain", e.g. Urai et al., 1986), the generally accepted argument is that subgrain boundaries are always associated with subgrain rotations, as new dislocations are continuously added to the subgrain boundaries (e.g. Passchier and Trouw, 2005, p.43). We have added the reference to Konrad-Schmolke et al. (2007), as well as recent publications of Petley-Ragan et al. (2019), Jamtveit et al. (2018a,b, in press), Engi et al. (2018), Giuntoli et al. (2018) and Angiboust et al. (2017) when comparing and contrasting our "dry" results to fracture and diffusion in garnets in deep-seated rocks where fluid infiltration plays an important role.

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