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Interactive comment on "Pseudotachylyte as field evidence for lower crustal earthquakes during the intracontinental Petermann Orogeny (Musgrave Block, Central Australia)" *by* Friedrich Hawemann et al.

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RC: You have not shown that they are cyclic, to me the changes from brittle to ductile and back again seem random (not cyclic). AR: With the word "cyclic" we wanted to express that deformation is changing from brittle to ductile and back. We try to demonstrate this by pointing out, that pseudotachylytes are emplaced pre-, syn- and post shearing. AC: We integrated a new Fig 6 to show that sheared pseudotachylytes can be found as clasts in a new generation of pseudotachylyte, demonstrating the switch from brittle to ductile to brittle again, which may actually repeat several times. We now restrict the use of the term “cycles” to the discussion and specifically with reference to earthquake cycles.

RC: How these repeated ‘strong variations’ in stresses are formed, and the causal link between stress variations and the formation of shear zones vs. co-seismic faulting is not discussed or explained in detail in the manuscript. AR: In the discussion, we test our observations against the common proposed models for brittle-ductile interplay in the lower crust. With the data presented here, we cannot establish the cause of the stress variations. This problem will be specifically addressed in a different manuscript, which is currently in preparation. The aim of the current manuscript is different and quite specific – to establish that repeated cycles from brittle to ductile to brittle, involving large volumes of pseudotachylyte, are occurring under water deficient conditions of ca. 650 $^{\circ}$C and 1.2 GPa, i.e. lower crustal conditions

RC: Self-localised thermal runaway (SLTR) following John et al (2009) is plainly rejected as a weakening mechanism in this manuscript because the authors have not found ductile precursors to any of the studied faults. I wonder if they have looked well enough? because there is no detailed description or illustration of fault veins in their figures included here. The deep crustal PST examples I have detailed knowledge from (in Corsica and Norway) we have spent a long time looking and dedicated sampled fault veins (not the nice big injection veins) to observe what happens with wall-rocks during co-seismic faults. Particularly the smallest fault veins (see Andersen et al 2008, Deseta et al. 2014) provide the best examples of ductile wall-rock damage zones. The evidence for crystal-plastic and ductile deformation is not easily found because the high heat tends to melt and destroy the evidence for the ductile wall-rock precursor as well as most of the inclusions of the wall rocks. Therefore, only a few examples provide macroscopic evidence for pre-fault (PST) ductile fabrics, one is from the Kråkeneset gabbro described in John et al. (2009) and I enclose a field photo of this for your inspection, where shear fabrics are preserved along a small fault next to a PST where they are mostly melted away on the same fault. Evidence from minor fault in thin section are more common.Therefore, if you still find no evidence of shearing after new inspection of wall-rock damage zones in you fault veins, you are at least be able to say with confidence that evidence of SLTR is not found after careful inspection! Otherwise, perhaps you should keep an open mind to SLTR as an option until you can document that there is no crystal plasticity or ductility anywhere in the wall-rock damage zones along your fault veins. [. . .] And I want image(s) fault-vein contacts with wall-rocks. AR: We tried to have an unbiased view with regard to the formation mechanism of the pseudotachylytes in the Musgrave Block. The observations we made are in conflict with the idea of SLTR. In the new Fig. 6, we present a pseudotachylyte fault vein including the host rock, where no ultramylonitic precursor is visible. A more thorough discussion is presented in the short comment (SC1) in the discussion. We did not add a more thorough discussion to the current manuscript, as it is beyond the scope and aim of this study.

RC: In the PST in Holsnøy described by Austrheim and co-workers, mineral inclusions in for example garnet is very commonly associated with the shock-type deformation (partial pulverisation of wall-rocks) of minerals during the co-seismic faulting, and should therefore perhaps be included? AR: We do report fractured garnets, but they show discrete and rather planar fractures and are not pulverized. AC: The connection between fractured garnet and seismic stresses is now added to the text, together with relevant references.

RC: Figures! In this part of my review with general comments I suggest that you improve most of your figures or at least the explanation in the figure text. If you discuss more in text I want you to specify where this can be found in the main text. I also want to see micrograph of fault-veins and I want better (in fact much improved) text to most of the figures. In many cases texts are very short and do not explain well enough what we see particularly in the photo figures. There are also some errors for examples in Fig 8b where the pressure unit is written as GPa but probably given in numbers as kbar? [. . .] In Fig. 3 you have some nice PST images, but again the explanations in the fig-text is very short and inadequate. I miss a much better explanation of what I see in fig 3a and 3c, and a discussion/explanation of how rotation of clasts in 3a occurred, and if there is a PST fault vein along the contact with the amphibolite dolerite and the duplex-like structure in 3b. This can be done better! AC: Figure captions have been improved to aid a better understanding of the images. However, in principle, we consider that figure captions should be concise and limited to description rather than interpretation. The figures are all described and discussed in detail within the main body of the text. We apologize for the error made in Fig 8b, which has been corrected.

RC: A regional geological map (Fig. 2) should normally have a regional cross-section as well. AR: In our opinion, the geophysical maps are more instructive for the purpose than a geological map, as these also see through the cover providing a clearer tectonic interpretation and highlighting the difference between Mulga Park- and Fregon Subdomains, as well as the post-Musgrave Orogeny granites that were not depleted in Th. AC: We included a recent reference (Wex et al. 2017) where a geological map and cross sections can be found.

RC: In figure 4 there is an inset backscatter image of an obliquely foliated injection vein? Explain what we see and why is there a foliation there. Is this flow foliation or some post PST deformation phenomenon? AR: As the foliation is slightly oblique to the margin of the vein, we interpret the foliation to be the result of ductile shearing. AC: This has been clarified in the text.

RC1_supplement: Other comments, if not already addressed above, have been integrated in text and figures.

RC: Silicate melts may have a high fluid, any info on the content of fluids in the pst? AR: As clearly shown in the sample description, biotite is the only water-bearing mineral observed in the studied sample (F68). Furthermore, kyanite rather than clinozoisite/epidote is present in the pseudotachylyte, also indicating that the fluid content of the initial melt was low.

RC: injection vein with later localisation? (comment of Fig. 3b) AR: The geometries are not clear enough, to call this an injection vein. The ductile overprint is later, as stated in the text.

RC: is there any evidence for quenched mineral zoning as evidence for progressive cooling? AR: The grain size of minerals is extremely small, and no zoning is visible.

RC: Do we see two generations of pst or just a transition from foliated to not foliated? Text is not adequate for reader to understand this brittle- to ductile transition. Explain better! I think more illustrations are required, also optical micrograph, to me this could look as a ductile precursor to a static quenched pst! (comment to Fig.5a) AR: The host rock is a quartzo-feldspathic mylonite, with clearly visible quartz ribbons (appear dark). We here want to demonstrate the brittle overprint of the mylonitic foliation. There is no evidence, that the mylonite represents a ductile precursor. Also, in the model of John et al. (2009), the precursor (ultra-) mylonite is expected to be completely melted. AC: The figure captions have been improved to clarify this.

RC: Is there any issue between cooling from max shear heating (friction) temperatures and temperatures derived from mineral equilibria modelling? Could there have been superheating? AR: As the temperatures are derived from equilibria modelling of dynamically recrystallized minerals and not on minerals directly crystallizing from a melt, we are confident that superheating effects are not reflected in the mineral compositions.

RC: P-1.2 GPa and 690C are considerably more narrow than the pseudosection shows, any reason for this? AR: The range of this field is very narrow anyways, so for simplicity the center point is used as input to calculate the mineral composition. We do not claim the method to be this accurate.

RC: Mineral inclusion masked from PT models because they are considered not to be part of a stable assemblage, is this justified, we see crystals f.example garnet as ‘sponges’ of inclusion due to seismic deformation. see Austrheim papers AR: We did not mask mineral inclusions. We masked clasts that were not dynamically recrystallized

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and therefore not part of the stable assemblage.

RC: 'close to these conditions', is there a ref. for this? AR: This is derived from the pseudosection.

RC: insufficient explanation of element maps, mineral names on fig required, what is blue in Fe in C. AR: The initial idea of the element maps was to show the reaction of feldspar clasts and the different iron-oxide phases. We agree, that a larger map with labeled phases can be helpful for the reader. AC: An enlarged version of Fig. 9a with mineral labels will be added to the appendix.

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