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Solid Earth Discussions

Interactive comment on "Inverted distribution of ductile deformation in the relatively "dry" middle crust across the Woodroffe Thrust, central Australia" *by* Sebastian Wex et al.

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This manuscript presents a variety of field, chemical, and microstructural observations and data related to the Woodroffe Thrust in central Australia, with the goal to better understand how the mylonite zone evolved, and in particular, why the majority of the deformation is developed in the footwall rather than in the hangingwall. This is an interesting and well-conceived contribution about deep-crustal shear zone deformation, and the topic is of broad interest to an international audience. I enjoyed reading it.

The overall interpretation appears to be sound and based on a broad range of observation types. However, there are two main issues that I would suggest the authors

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Discussion paper

consider carefully:

The first has to do with organization and lack of necessary background information. Much of the necessary information appears to have been published already in Wex et al. (2017, Tectonics), but more than citations to that work need to be presented here. For example, there is a distinct need for some description of the “starting material” that was reworked in this shear zone, or at least some description of representative lithologies, assemblages, and estimated equilibrium conditions for those starting assemblages (presumably Musgravian). This has important implication for the authors’ interpretation that the fluid source was the lower plate and that the shear zone system was essentially “closed.” The lower plate rock is currently described as “upper amphibolite-facies during Musgravian without further details. But were these rocks reworked by the Woodroffe deformation in a retrograde or prograde sense? If prograde, then they could conceivably produce free fluid internally through dehydration reactions. But if in a retrograde sense, then they would have consumed water, if present, rather than produce it. The authors could call upon dehydration-driven fluids from deeper levels in the footwall, but then it is not a “closed system.” The whole calcite and O and C isotope story seems to point to an external source of fluid too. Some clarification is needed, and probably a summarization of what Wex et al. (2017) learned about these rocks in the previous study would help a lot. This suggestion extends to the pseudotachylyte system as well, which is used in the current study but with very little description what their role in the evolution of the shearing history is. The authors mention evidence for two stages of deformation at different conditions in the shear zone and give P-T conditions, but does that really reflect distinctly different time periods? What is the basis for “regional temperature gradient” that is shown in Fig. 8 and 10??? There is no description of where that came from other than a citation to Wex et al. (2017), but it basis is pretty critical this proposed story and interpretations here too.

The second is also primarily an organization thing and has to do with sample localities, the various geographic groupings, and the N-S transect. This is all quite confusing.

I suggest the authors start early in the manuscript with a description of the sampling strategy as essentially on a N-S transect like that shown at the bottom of the current Fig. 8 – that is easy to see and easy to keep referring to, but show it in a simplified form earlier than in the 8th figure. Right now, the fact that sample localities are broken into various northern and southern plus/minus central groups based on different datasets is convoluted. For the bulk Th measurements, "northern" is locations 1 through 6. According to Fig. 8, station 7 is plots more northerly than 6 so you really should include 7 too. But for plagioclase stability, "northern" is stations 1 through 9. And for abundance of hydrous minerals, "northern" is only stations 1 through 5. Stop all of the group attempts and simply show how the various observations and data change along the transect – the trends will be the same and much easier to follow.

The final major point is that the beginning of the discussion on feldspar breakdown reactions and fluid activity in sections 8.1 and 8.2 is vague and not particularly strong. How can reactions 1 and 2 be pitted against one another when they reflect different chemical systems? One has K in it and the other does not. There is another vague reference to Wex et al. (2017) for plagioclase chemical variability during recrystallization (dynamic?) but no details – this information is important to the discussion and should be explained in more detail, either here or earlier in the manuscript in a summary of the earlier study. There is a claim of "coeval development" of a (synkinematic?) dry assemblage and a wet one in the same rock with reference to S5. But the figure in S5 only shows a dry assemblage, and it is a static texture (could that be Musgravian even?). The claim that fluid activity has been "quantified" is not justified; this is a qualitative evaluation, not a quantitative one. However, there is still a convincing case that there was indeed more water in the north than in the south; but the case is currently overstated in terms of what has really been constrained petrologically. And Fig. 9 is not necessary.

Other more specific points: Lines 75-80: What is the significance of these earlier mylonites? Lines 90-95: Two stages of deformation and P-T conditions are given, but only one long set of “stable” mineral assemblages is given. Stable with respect to which stage? Section 5.1: Can an estimated error be quoted for the Th measurements? Line 187: switch 6 ppm and 8 ppm Fig. 4: how is the lower bound of the mylonite zone defined and how well is that constrained? Fig. 5: Airborne Th maps – how sensitive is this measurement to depth? What is the thickness of the Kelly Hills klippe? Could that be a contributor to increased Th signal? Line 207: Why assume all PST is in the hangingwall? Section 6.2: indicate whether these measurements are in interpreted hangingwall or footwall Section 7: why do this only on the PST? Why not directly on the host rocks? Fig. 9 – not needed Line 320: Na is not considered either. Iron could also come from Bt or garnet. Fig. 8 and 10 – where does the regional temperature gradient come from and what is its interpreted meaning? Line 360 – did not quantify water activity Section 8.3.2 – this all supports an external fluid source; so how to reconcile? Line 422-23: distinguish “water weakening” from simply a rheologically weaker assemblage (e.g., higher mica abundance?) Line 437: this also argues against a “closed system” Line 450-451: clarify why increasing fluid-rock interaction would result in volume loss?

-K.H.Mahan

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