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Solid Earth Discussions

Interactive comment on "Mid-crustal shear zone development under retrograde conditions: pressure–temperature–fluid constraints from the Kuckaus Mylonite Zone, Namibia" *by* Johann F. A. Diener et al.

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The paper reports on analyses from three amphibolite samples that cover a lateral strain gradient into the shear zone and therefore exhibit different degrees of mylonitic deformation. A brief petrographic description introduces thermocalc pseudosection models that establish the synkinematic mineral paragenesis and the likely water content of the rocks during deformation. These data complement an earlier study by Rennie et al (2013, JSG) on the macroscopic strain distribution in the Kuckaus Mylonite Zone. The current manuscript discusses the feedbacks between fluid infiltration and

weakening, potential fluid sources, and implications for the interpretation of geophysical data from modern strike slip shear zones.

By addressing a set of research questions that relate to synkinematic transport properties of shear zones the article is very timely. I particularly found the sections that tried to relate their findings to modern analogues very stimulating. As the second reviewer, I support Luca Menegon's views. I find that the paper is generally very well written and an interesting read, and certainly a very relevant contribution. However, I also feel that the paper "as is" falls short of its potentially significant impact. In my opinion, the paper could elegantly bridge the unfortunate but frequent disconnect between metamorphic and microstructural studies and also provide a much less speculative base to its own discussion section, if a) the authors had spent more efforts on characterising the microstructural evolution of their rocks and b) had characterised the source and role of the fluid better. I am aware that these recommendations likely require additional analyses on the samples, but I think these would be worth it.

a) The link between deformation and synkinematic reactions, highlighted by the authors, is rather poorly constrained. Usually, backscatter electron imaging and electron backscatter diffraction are necessary to characterise the mechanisms controlling deformation in fine-grained (tens of um in the present case?) ultramylonites.

[Reply] This is the main shortcoming pointed out by both reviewers, and we have addressed it with the addition of 10 new photomicrographs and backscatter images. These images support our interpretations and also illustrate the features that are pointed out by the reviewer below.

In the particular case, these techniques would have also helped to elucidate mechanisms of phase mixing between the samples, highlight the mica layers that the authors mention and determine the grain-scale effects of dissolution and reprecipitation.

[Reply] This suggestion proved very helpful, and we now provide a much better supported discussion of these aspects.

Secondary electron images have furthermore proven very useful in imaging grain boundary cavities. Such data would have helped the authors to reconcile their findings more firmly with recent studies on strain localisation in mid-crustal ultramylonites. In that regard, some important references are missing in the manuscript, including, e.g., Killian et al. (2011), Billia et al. (2013), Fukuda & Okudaira (2013), Spruzeniece & Piazolo (2015), Rahl & Skemer (2016).

[Reply] This is another very good suggestion that we address with the addition of Fig 4f and the associated description in sections 5.3 and 5.4.

Furthermore, the paper critically hinges on a space for time approach. Whilst it is an almost classical assumption that strain gradients at shear zone margins can be used in that way, it needs to be established that samples KMZ28 and KMZ29 indeed represent precursors of KMZ30.

[Reply] As also pointed out by the first reviewer, the XRF data for these samples are not identical, but rather defines a broad trend where increases in Si and Na and decreases in Fe, Mg and Ca are coupled to increasing strain. We now explicitly point this out in the mineralogical interpretation, and argue that this trend is likely due to varying degrees of metasomatism from fluid-rock interaction during KMZ shearing. We further support our interpretation that these rocks are from the same protolith with additional petrographic and metamorphic arguments, coupled to the very close proximity of the 3 samples. It should be noted that the samples are not from the shear zone margin, but actually from the margin of a low-strain lozenge that is located near the shear zone core (as stated on page 4, line 15).

b) The paper remains very speculative when it comes to fluid sources and the role of fluids during shearing. Whilst I can follow the author's arguments, these are not actually supported by data. Fluids in mid-crustal shear zones have been characterised using stable isotopes in excellent contributions by, e.g., Clark et al. (2005, 2006) and Konrad-Schmolke et al. (2011). Lately, fluid-rock interaction has been refined by using

Boron (e.g., Konrad-Schmolke & Halama, 2014) and Lithium (John et al., 2012) stable isotopes. I would encourage the authors to adopt some of the ideas presented in these studies.

[Reply] This is a worthwhile suggestion, but as pointed out by the reviewer would involve additional analysis that is not possible within the time frame of the review. The shear zone is also largely devoid of quartz veins that would be the most reliable target material for the suggested analysis. We deliberately keep the discussion of possible fluid sources vague because we do not present evidence to support our petrological arguments for the fluid source. However, the main point that we make regarding the fluid source is that is very unlikely to have been local, and therefore the fluid must have entered these lithologies after shear zone initiation allowed for increased permeability of these rocks. We have removed reference to meteoric water as the most likely fluid source from the abstract, and replaced it with a more generic reference to a remote fluid reservoir.

Stylistically, I would recommend to avoid all repetitions and found that the discussion could have been better structured.

[Reply] This criticism is noted, but the repetition in the discussion is deliberate and limited to a re-statement of the relevant facts when a new aspect of the discussion is introduced. We believe this is appropriate to lead the reader and avoid confusion.

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