Solid Earth Discuss.,
doi:10.5194/se-2016-66-RC2, 2016

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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Received and published: 15 June 2016

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. 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. 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). 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.

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.

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

With best regards, Florian Fusseis

Interactive comment on Solid Earth Discuss., doi:10.5194/se-2016-66, 2016.