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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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Dear Authors, dear Editor, This manuscript presents an analysis of P-T-fluids conditions recorded in variably overprinted mafic rocks along the dextral strike slip Kuckaus Mylonite Zone in Namibia. Three samples collected along a strain gradient have been examined for their petrography, mineral chemistry, bulk-rock chemical compositions and mineral equilibria. The results are discussed in terms of the effects of fluid infiltration on strain localization under retrograde conditions and on the associated implications for the strength of viscous shear zones. A possible origin of tectonic tremors by reac-

tion weakening under hydrostatic fluid pressure conditions in shear zones is proposed. Discussion and conclusions are generally supported by the results and observations. This specifically applies to the pseudosection analysis of P-T-fluids conditions, which is particularly sound. The discussion on fluid infiltration mechanisms and on feedback mechanism in the shear zone is, however, less well hinged on actual observations, and in my view the microstructural analysis should be expanded to fully support the interpretations/conclusions. Overall, this study represents another very nice example of how important fluid infiltration is for the mechanical behaviour of the middle and lower crust. Processes like reaction softening have been largely overlooked in the rheological modelling of the lithosphere; this study, like many other published in the recent years, highlights the fundamental role played by fluid-rock interaction and reaction behaviour of minerals. The manuscript is concise and very well written, and I have enjoyed reading it. The figures are all informative and clear, but more figures could be included to expand the microstructural analysis.

Main comments

1. Deformation microstructures and mechanisms. Not much is said about the deformation microstructures in the shear zones. The Authors refer to creep cavitation, grain boundary sliding and grain size reduction processes (e.g. in chapters 5.2 and 5.3), but microstructural observations are not presented. All these processes are very plausible in the KMZ, but the paper generally suffers of a lack of SEM-BSE images (and possibly CPO measurements) of deformation microstructures that could be used to infer the dominant deformation mechanisms and, thus, to strengthen the discussion on the actual feedback mechanisms potentially active in retrograde shear zones. For example, inspection of the grain boundary morphology with secondary electron imaging (e.g. Hippertt, 1994; Mancktelow et al., 1998; Menegon et al., 2006; Fusseis et al., 2009) can support the presence of grain boundary fluid enhancing grain boundary sliding (as envisaged in page 11). Is there any evidence of heterogeneous nucleation of reaction products in dilatant sites in the mylonites, which could be consistent with

the creation of dynamic porosity during grain boundary sliding (= creep cavitation)? Are CPO measurements accessible in the timeframe of the revision? If yes, they could help identify the dominant deformation mechanism in support of the current discussion. Also, in the discussion about the competition between grain size reduction and grain growth (page 11), the Authors seem to ignore second phase pinning. Phase mixing in (ultra)mylonites typically inhibits grain growth due to second phase pinning; this maintains the grain size sufficiently small to activate grain-size sensitive creep, thereby stabilizing strain localization in the ultramylonites. There is ample literature on this subject that the Authors can refer to in the discussion.

2. Equilibrium mineral assemblage and fluid content. In part as a follow-up of the previous comment, plagioclase zoning patterns could perhaps be used to better constrain the synkinematic P, T conditions and to infer possible deformation mechanisms. If there is asymmetric zoning (perhaps with albite-rich rims preferentially elongated parallel to the stretching lineation), the Authors have a strong argument to infer the (1) those rims grow synkinematically, and (2) their growth is the result of dissolution-precipitation creep (e.g. see Imon et al., 2002; Menegon et al., 2006). I am also a bit confused by the synkinematic mineral assemblage. On page 4 it is written that chlorite and epidote do not have a preferred orientation, could that mean that they are the product of a late, static overgrowth? Again, documenting the possible nucleation of these phases in dilatant sites/strain shadows in the ultramylonites would clearly confirm that they're synkinematic. I have followed the arguments in favour of a discrete tectonic event in chapter 5.1 and they are convincing – but it would be ideal to support them with a robust microstructural description. As for the fluid content, why was the LOI of the three samples not measured and not used as input data for the pseudosections? It could have given some independent constraints on the amounts of fluids in the three different rocks.

3. Shear zone initiation. I could not follow the argument of shear zone nucleation along an existing well-oriented granulitic fabric. First, I was under the impression that the

KMZ is discordant with respect to the granulite-facies structures and fabrics (page 8). Second, I cannot see a clear, well-oriented fabric in the granulite-facies lens in Fig. 2a (and 3a as well). A provocative interpretation could be that the mylonitic envelopes (KMZ30) represents rock portions that were originally more hydrated than the lens, therefore lending themselves to hydration reactions and strain localization during retrogression. According to Fig. 5c, this could have been possible if e.g. KMZ contained ca. 6 mol% H2O. Actually the XRF compositions of the three samples are markedly different and this raises the question on whether or not they represent the petrological and microstructural modifications of the same material with increasing hydration, strain and retrogression. If we accept the model of fluid infiltration, the most plausible initiation mechanism appears to be brittle deformation enhancing porosity and triggering fluid infiltration, rather than reactivation of favourably oriented existing HT fabrics (why would fluid preferentially infiltrate in those coarse-grained layered rocks?).

Minor comments

âĂć Page 2 lines 23-25: this is questionable. There are several examples of pseudotachylytes in high grade rocks, which provided brittle discontinuities on which viscous shear zones could nucleate (see papers by Austrheim, JC White, and others). âĂć Page 4 line 26: possible subgrains in hornblende are not visible form the figure. âĂć Page 10 line 2: please clarify how crystal plastic deformation (dislocation creep I suppose) would lead to volume change. âĂć Page 11 line 33: reference to models of a dry and strong granulitic lower crust should be mentioned here, since many rheological models predict a weak granulitic lower crust.

Best wishes, Luca Menegon

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