

***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.***

**Johann F. A. Diener et al.**

johann.diener@uct.ac.za

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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 reaction

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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 behavior 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.

[Reply] We agree that many of our assessments could have been better supported and illustrated with a more detailed description of the microstructure, particularly as most of our conclusions hinge on this aspect. We have consequently added four new photomicrographs to Fig 3 and a new plate of backscatter electron images (Fig. 4) that thoroughly illustrate these aspects and hopefully address most of the reviewers' concerns.

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

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the dominant deformation mechanisms and, thus, to strengthen the discussion on the actual feedback mechanisms potentially active in retrograde shear zones.

[Reply] Agreed. We have added 10 new optical and BSE photographs to address this. CPO measurements are a good suggestion, but we chose not to conduct these as we do not see evidence of dislocation creep in the microstructures, whereas we now present evidence for grain boundary sliding, following the suggestions outlined by the reviewer's next point. We agree that CPO measurements would allow a more detailed analysis, but suggest this is outside the scope of the current paper, would not significantly alter its conclusions, but could form a topic for a future study inspired by the current paper.

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)?

[Reply] This suggestion proved very helpful, and we thank the reviewer for pointing out this obvious shortcoming in the paper. We now present ample evidence for heterogeneous nucleation of reaction products (Fig 4b-e), as well as potential evidence of porosity that is indicative of fluid-enhanced grain boundary sliding (Fig. 4f). We trust that the addition of these figures and the associated description adequately addresses the concerns and feel that it has significantly strengthened the conclusions of our work.

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.

[Reply] Honestly, no they are not. However, we do not see evidence of dislocation creep in the microstructures, and therefore do not think that CPO measurements would necessarily add information or change the conclusions. On the contrary, we now present

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evidence to suggest that grain boundary sliding is a dominant deformation mechanism. Nevertheless, we are in the process of examining the microstructures of the felsic rocks that make up the largest volume of shear zone material and intend to do CPO measurements as part of that work.

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.

[Reply] We thank the reviewer for pointing this out, and have made mention of second phase pinning as an additional feedback mechanism during shear zone development in section 5.3.

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).

[Reply] We re-examined the zoning and found that it is only observed in large grains that are generally not strongly asymmetric, such as in KMZ28. However, the composition of fine-grained and strongly recrystallised plagioclase is homogenous, and similar to the rims of large plagioclases. We have added a more detailed description of this in the Mineral Chemistry section, and follow the reviewer that this provides further evidence for syn-kinematic growth through a dissolution-precipitation mechanism.

I am also a bit confused by the synkinematic mineral assemblage. On page 4 it is

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written that chlorite and epidote do not have a preferred orientation, could that mean that they are the product of a late, static overgrowth?

[Reply] This is not due to late static overgrowth, but due to the different degrees of shearing manifested in the samples. All samples exhibit evidence for the growth of chlorite and epidote (after hornblende and plagioclase), but in the unsheared KMZ28 these new phases do not have a preferred orientation, whereas they definitely do in the other two samples. This is now more explicitly described in the Petrography section, and supported with clear photographs of non-oriented chlorite and epidote in the unsheared sample (Fig. 3d,e) and strongly aligned chlorite and epidote in the sheared samples (Fig 3 f,g; 4a,b,d,e).

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.

[Reply] We believe we have adequately addressed this shortcoming, as discussed in the replies above, by the addition of photomicrographs in Fig. 3 and BSE images in Fig. 4 showing growth of secondary phases in dilatant sites (Fig. 4b-e in particular).

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.

[Reply] Measured LOI consists of volatiles (H<sub>2</sub>O, CO<sub>2</sub>, organic compounds) that are removed, and O that is added from the oxidation of Fe. It is the first author's experience that LOI is not a particularly good approximation of actual H<sub>2</sub>O content (admittedly mostly from work with granulites), and that calculated T/P - MH<sub>2</sub>O pseudosections provide a more robust and consistent way of determining H<sub>2</sub>O content.

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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% H<sub>2</sub>O.

[Reply] The KMZ is indeed discordant to the granulite-facies fabric at its local margins, but also regionally. The point we are trying to make here is that fluid infiltration could not have led to shear zone initiation, but that it must rather have been the other way round. Hence we require a distinct mechanism for initiation. We do not have any evidence that the KMZ, or the larger MRPSZ, nucleated on favourably-oriented structures, and as mentioned, do not preclude any other form of initiation. We have expanded this aspect to include the possibility of pre-hydrated crust, and also add the point that shear zone nucleation need not have occurred in the study area, but that it could have propagated there from 100s of km along strike. We have removed the mention of re-activation from the abstract.

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.

[Reply] We have added a paragraph that points this out and argues that it is likely due to different degrees of fluid-rock interaction and metasomatism during KMZ deformation.

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

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than reactivation of favourably oriented existing HT fabrics (why would fluid preferentially infiltrate in those coarse-grained layered rocks?)

[Reply] As explained above, we do not feel that we have particularly strong constraints or evidence for a specific initiation mechanism, and leave the discussion of this aspect deliberately vague and open-ended. As the reviewer points out, there has to be a reason for the fluid to preferentially infiltrate these coarse-grained rocks, which is why we propose that deformation must have preceded fluid infiltration in order to first enhance porosity.

Minor comments

Page 2 lines 23-25: this is questionable. There are several examples of pseudotachylites in high grade rocks, which provided brittle discontinuities on which viscous shear zones could nucleate (see papers by Austrheim, JC White, and others).

[Reply] Addressed with comment and references stating the possibility of pseudotachylites being locally present in lower crustal rocks.

Page 4 line 26: possible subgrains in hornblende are not visible from the figure.

[Reply] Addressed with the addition of Fig. 3f,g.

Page 10 line 2: please clarify how crystal plastic deformation (dislocation creep I suppose) would lead to volume change.

[Reply] Statement removed.

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.

[Reply] Added

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