

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.

F. Füsseis (Referee)

florian.fusseis@ed.ac.uk

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Review of Wex et al “Inverted distribution of ductile deformation in the relatively dry middle crust across the Woodroffe Thrust, central Australia”

The authors present a further contribution on this spectacular thrust system, exploring the apparent asymmetry of the shear zone width with respect to the incorporation of the foot- and hanging wall rocks. They confirm that the inverted scenario that favoured mylonitization of the footwall over those of the hanging wall rocks is due to a slightly increased water content in the former, especially in the northern parts of the explored thrust system.

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The paper is very well written, beautifully illustrated and a captivating read even late at night. Clearly the paper is companion to other papers from the group, in particular of course the recent work of Wex et al (2017), and I agree with Kevin Mahan that certain shortcuts that arose from this should be corrected to improve the readability of the presented manuscript.

Two aspects struck me as “loose ends” though:

First, I find it curious that the authors miss the opportunity to discuss the significant increase in shear strain due to the narrowing of the Woodroffe thrust from south to north in light of the strain softening mechanisms they envisage. The relatively lapidary last sentence of the discussion (line 463) does, in my eyes, not explain why the Woodroffe thrust is six-fold narrower in the north, especially if the wetter conditions do not account for the pronounced strain localization there. The observation that the thrust narrows so dramatically would invite a more detailed discussion of the microstructural developments along the strain gradients in the north and south, or, in other words, the strain-dependent evolution of the mylonites. Which mechanisms accommodate strain softening in the north and south? Is it possible that the shear zone progressively narrowed during shearing, in analogy with Means’ (1995) type 2 shear zones? If so, does this apply to the entire Woodroffe thrust? And if not, why not? I would invite the authors to include a more detailed microstructural description of the evolution from host rock to ultramylonite in both, northern and southern sections, and then integrate these into the discussion of their findings in light of the questions above.

Second, the mantle source of the CO₂-dominated brines. If these are indeed mantle-derived, how did they migrate along the shear zone? Was there some form of synkinematic porosity? If there was a fluid migrating along the Woodroffe thrust, what was its micromechanical effect in both, the northern and southern sections?

With respect to the determination of the modal abundance of the hydrous minerals (S1.3) – global thresholding on the basis of grey value histograms is rather primitive and

prone to substantial errors – Fiji/ImageJ offers much more sophisticated segmentation algorithms, in particular trainable WEKA segmentation, a machine learning toolbox.

Florian Füsseis

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