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# ***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.***

**Sebastian Wex et al.**

[sebastian.wex@erdw.ethz.ch](mailto:sebastian.wex@erdw.ethz.ch)

Received and published: 18 April 2018

1) The first has to do with organization and lack of necessary background information. Much of the necessary information appears to have been published already in Wex et al. (2017, Tectonics), but more than citations to that work need to be presented here. For example, there is a distinct need for some description of the “starting material” that was reworked in this shear zone, or at least some description of representative lithologies, assemblages, and estimated equilibrium conditions for those starting assemblages (presumably Musgravian). This has important implication for the authors’ interpretation that the fluid source was the lower plate and that the shear zone sys-

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tem was essentially “closed.” The lower plate rock is currently described as “upper amphibolite-facies during Musgravian without further details. But were these rocks re-worked by the Woodroffe deformation in a retrograde or prograde sense? If prograde, then they could conceivably produce free fluid internally through dehydration reactions. But if in a retrograde sense, then they would have consumed water, if present, rather than produce it.

AC/MC: Our geological introduction was indeed very concise. Following the reviewer’s suggestion, we have extended the geological introduction of the manuscript to provide additional information regarding the Musgravian Orogeny “starting material” and the estimated metamorphic conditions, also in comparison to the metamorphic overprint during the Petermann Orogeny.

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2) The authors could call upon dehydration-driven fluids from deeper levels in the footwall, but then it is not a “closed system.” The whole calcite and O and C isotope story seems to point to an external source of fluid too.

AC: Carbon indeed cannot have an immediately local source, because the studied rocks are initially entirely non-carbonaceous. With regard to carbon the system is clearly not closed. We tried to further constrain the origin using stable isotopes. Unfortunately, the whole calcite isotope story is not conclusive, and our current conclusions based on this data therefore rather speculative. However, our observations and data clearly indicate that the source of the hydrous fluids and that of carbon are not the same. The external carbon source does therefore not provide any constraints for the source of the hydrous fluids. We do not argue that dehydration-driven fluids originate from deep levels in the footwall, but immediately from within the local, exposed footwall units. This would represent a closed system with regard to the aqueous fluids.

MC: The interpretation of the calcite isotopic data, has been formulated into more restrained statements (comment 25) and moved to the appendix in the revised version.

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[Discussion paper](#)

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**Interactive comment**

3) Some clarification is needed, and probably a summarization of what Wex et al. (2017) learned about these rocks in the previous study would help a lot.

AC: Where necessary, Wex et al. (2017) is cited and, where appropriate, we now provide additional information, e.g. that P-T conditions were determined via conventional geothermobarometry rather than just stating the derived numbers. We have avoided providing an extensive and detailed report of the results previously published in Wex et al. (2017), as that would result in unnecessary lengthening of the manuscript. The current manuscript was submitted subsequently to the published companion paper of Wex et al. (2017) in order to allow a systematic development and avoid repetition of previously published data.

MC: Additional information on how Wex et al. (2017) constrained the syn-kinematic P-T conditions and the interpreted meaning of these values has been added.

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4) This suggestion extends to the pseudotachylyte system as well, which is used in the current study but with very little description what their role in the evolution of the shearing history is.

AC: The pseudotachylyte system and associated microstructures are only of marginal relevance to the current paper, which is why they were not presented in full detail. These aspects will be thoroughly presented in a follow-up companion paper, which particularly focuses on the brittle-ductile interplay (pseudotachylytes vs. mylonites) and pseudotachylyte development at mid-crustal depths. The proposed manuscript has already been compiled and is ready for submission once the current manuscript is published. The current manuscript will provide some relevant background information for this forthcoming paper, as the paper by Wex et al. (2017) has provided necessary background information for the current submission.

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Interactive  
comment

5) The authors mention evidence for two stages of deformation at different conditions in the shear zone and give P-T conditions, but does that really reflect distinctly different time periods?

AC: The mentioned two stages of deformation are differentiated on the basis of microstructure and petrology and interpreted to represent different time periods during shearing along the Woodroffe Thrust, as discussed in detail in Wex et al (2017). We agree that mentioning this aspect might be confusing to the reader, because it is not crucial in the current manuscript.

MC: Since this information is not relevant to the current manuscript we have omitted those parts making reference to the presence of two different stages of deformation.

6) What is the basis for “regional temperature gradient” that is shown in Fig. 8 and 10??? There is no description of where that came from other than a citation to Wex et al. (2017), but it basis is pretty critical this proposed story and interpretations here too.

AC: As discussed in comment 3), we have now added the information that P-T conditions were constrained from conventional geothermobarometry. We provide this information where we first referenced Wex et al. (2017) in reference to the estimated P-T conditions. Then, for subsequent references to the temperature gradient and to the P-T estimates (for instance in Fig. 8 and 10), a simple reference to Wex et al. (2017) should be sufficient.

MC: Additional information on how Wex et al. (2017) constrained the syn-kinematic P-T conditions and the interpreted meaning of these values has been added.

7) I suggest the authors start early in the manuscript with a description of the sampling

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[Discussion paper](#)



strategy as essentially on a N-S transect like that shown at the bottom of the current Fig. 8 – that is easy to see and easy to keep referring to, but show it in a simplified form earlier than in the 8th figure.

AC: The locations of all samples are clearly given in Figure 1, the very first figure of the paper.

MC: In section 3, we now state explicitly that samples were collected “along a N-S traverse, parallel to the direction of thrusting”.

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8) Right now, the fact that sample localities are broken into various northern and southern plus/minus central groups based on different datasets is convoluted. For the bulk Th measurements, “northern” is locations 1 through 6. According to Fig. 8, station 7 is plots more northerly than 6 so you really should include 7 too. But for plagioclase stability, “northern” is stations 1 through 9. And for abundance of hydrous minerals, “northern” is only stations 1 through 5. Stop all of the group attempts and simply show how the various observations and data change along the transect – the trends will be the same and much easier to follow.

AC: We agree that this aspect can cause confusion to the reader.

MC: We followed the reviewer’s suggestion and stopped our attempts of grouping locations. We now only refer to trends.

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9) The final major point is that the beginning of the discussion on feldspar breakdown reactions and fluid activity in sections 8.1 and 8.2 is vague and not particularly strong. How can reactions 1 and 2 be pitted against one another when they reflect different chemical systems? One has K in it and the other does not. There is another vague reference to Wex et al. (2017) for plagioclase chemical variability during recrystallization (dynamic?) but no details – this information is important to the discussion and should

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[Discussion paper](#)



be explained in more detail, either here or earlier in the manuscript in a summary of the earlier study.

AC: We only pit reactions 3 and 4 against each other as these are competing reactions. Reactions 1 and 2 are not meant to be pitted against each other, but to explain the different microstructures 1 and 2, which potentially arise due to the different availability of K.

MC: As additional information, we now state the composition of recrystallized feldspar and also the fact that recrystallization was dynamic.

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10) There is a claim of “coeval development” of a (synkinematic?) dry assemblage and a wet one in the same rock with reference to S5. But the figure in S5 only shows a dry assemblage, and it is a static texture (could that be Musgravian even?).

AC: The static microstructural overprint in S5 is consistent with the observations made in other deformed and undeformed samples which have been overprinted during the Petermann Orogeny. We agree that the appendix figure was actually more confusing than helpful to the reader, which is why we dropped it.

MC: Appendix S5 and its respective reference in the manuscript have been omitted.

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11) The claim that fluid activity has been “quantified” is not justified; this is a qualitative evaluation, not a quantitative one. However, there is still a convincing case that there was indeed more water in the north than in the south; but the case is currently overstated in terms of what has really been constrained petrologically.

AC/MC: A valid point: we have replaced “quantified” with “estimated”, reflecting the fact that the estimations are only qualitative.

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12) Lines 75-80: What is the significance of these earlier mylonites?

AC: We included these earlier mylonites for the sake of keeping the geological history complete. However, we agree that this is only confusing to the reader. The mylonites are described in more detail in Wex et al. (2017), but are not relevant to the current manuscript.

MC: The mentioning of the earlier mylonites has been omitted from the manuscript, because they are not relevant here and were not further discussed.

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13) Lines 90-95: Two stages of deformation and P-T conditions are given, but only one long set of “stable” mineral assemblages is given. Stable with respect to which stage?

AC: We agree that this aspect is confusing to the reader. As discussed in comment 5), the mentioned two stages of deformation are irrelevant to the current paper, which is why only a single list of minerals, those stable during the Petermann Orogeny, is given. Since the mentioning of the two stages has now been dropped, a single list of minerals is sufficient.

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14) Section 5.1: Can an estimated error be quoted for the Th measurements?

AC/MC: Measurements were run until the error was < 10% (stated in appendix S1). We now provide this information also in the manuscript.

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15) Line 187: switch 6 ppm and 8 ppm

AC/MC: done.

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16) Fig. 4: how is the lower bound of the mylonite zone defined and how well is that



constrained?

AC: The lower bound of the mylonite zone is defined by the initial appearance of Petermann mylonitic foliation well characterized by the trend of the stretching lineation and by the top-to-north kinematic indicators. Due to the gradual and irregular nature of the mylonite zone, with high-strain shear zones surrounding less to little deformed low-strain domains on the metre- to kilometre-scale, the lower bound is less well defined than its upper counterpart, but on the whole still reliable.

MC: The information that the lower bound is defined by the initial appearance of mylonites with Petermann kinematics is now provided.

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17) Fig. 5: Airborne Th maps – how sensitive is this measurement to depth? What is the thickness of the Kelly Hills klippe? Could that be a contributor to increased Th signal?

AC: As stated by Jones and Schreib (2007): “The gamma ray signal for natural radioisotopes in rocks comes almost entirely from the top 35 cm (IAEA, 2003)”, thus making the thickness of the Kelly Hilly klippe irrelevant to the increased Th signal.

D G JONES AND C SCHEIB. 2007. A preliminary interpretation of Tellus airborne radiometric data. British Geological Survey Commissioned Report, CR/07/061. 70pp.

IAEA. 2003. Guidelines for radioelement mapping using gamma ray spectrometer data. International Atomic Energy Agency IAEA-TECDOC, 1363, pp. 173.

MC: This clarification, together with the references, has now been added.

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18) Line 207: Why assume all PST is in the hanging wall?

AC: We did not want to give the impression that pseudotachylites were exclusively

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Discussion paper



restricted to the hanging wall, since this is clearly not true. However, it is evident from field observations that the largely unsheared pseudotachylite breccias in the hanging wall are locally sheared and dragged into the Woodroffe Thrust mylonitic zone.

MC: The statement has been reformulated into: “Similar field relationships, such as progressive downwards mylonitization of units clearly forming part of the hanging wall, also indicates limited reworking of the Fregon Subdomain at locations 4 and 6 (Fig. 5)”.

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19) Section 6.2: indicate whether these measurements are in interpreted hanging wall or footwall

AC/MC: This information is now provided in the Appendix Table S1.

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20) Section 7: why do this only on the PST? Why not directly on the host rocks?

AC: Pseudotachylites have been identified as preferred discontinuities for nucleation of shear zones under mid- to lower crustal conditions.

MC: This concept is now included at the beginning of section 7 with an extensive list of supporting references.

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21) Fig. 9 – not needed

AC/MC: Fig. 9 has been deleted.

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22) Line 320: Na is not considered either. Iron could also come from Bt or garnet.

AC/MC: The equations presented are indeed very simplified but are only taken as indicative of the reactions involved. Biotite and garnet have been added as potential

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[Discussion paper](#)



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23) Fig. 8 and 10 – where does the regional temperature gradient come from and what is its interpreted meaning?

AC: This aspect is discussed in comments 3) and 6).

MC: Additional information on how Wex et al. (2017) constrained the syn-kinematic P-T conditions and the interpreted meaning of these values has been added.

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Interactive comment

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24) Line 360 – did not quantify water activity

AC: We have replaced “quantified” with “estimated”.

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25) Section 8.3.2 – this all supports an external fluid source; so how to reconcile?

AC: Our data does not allow us to reconcile towards a single fluid source. Our observations and results indicate that the H<sub>2</sub>O and CO<sub>2</sub> each originated from different sources. We argue in favour of an internal source for the hydrous fluids, whereas the fact that we observe syn-kinematic calcite growth in otherwise non-carbonaceous rocks clearly indicates that carbon was introduced externally. Additional stable isotopic analysis on calcite, unfortunately, did not provide any significant constraints, allowing no further conclusions to be drawn without speculation.

MC: The interpretation of the isotopic analysis have been shifted entirely into the appendix with only a short summary of the isotopic results provided in the main manuscript. The potential mantle source as the most likely source has been dropped. The different possibilities are still presented in the appendix, however we reconcile by not trying to over interpret our isotopic results, since they are simply not conclusive

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26) Line 422-23: distinguish “water weakening” from simply a rheologically weaker assemblage (e.g., higher mica abundance?)

AC/MC: done.

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Interactive comment

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27) Line 437: this also argues against a “closed system”

AC: We agree that our argumentation, that the generally low abundance of hydrous minerals in both hanging wall and footwall in the “dry” southern locations potentially promoted a similar rheological response in both units, is indeed rather speculative. It is, in fact, also not supported by our Table 4.

MC: We decided to drop the mentioned statement and now solely argue that the local reworking of the lowermost hanging wall was guided by the presence of pseudotachylite.

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28) Line 450-451: clarify why increasing fluid-rock interaction would result in volume loss?

AC: We admit that there is no evidence for volume loss associated with fluid-rock interaction.

MC: The statement is omitted in the new manuscript version.

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Interactive comment on Solid Earth Discuss., <https://doi.org/10.5194/se-2018-9>, 2018.

Discussion paper

