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Interactive comment on "Strain localization in brittle-ductile shear zones: fluid abundant vs fluid limited conditions (an example from Wyangala area, Australia)" *by* L. Spruzeniece and S. Piazolo

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First of all, we would like to express our gratitude to the referees and editor for the critical but overall positive assessment of our manuscript. The constructive comments and suggestions greatly helped to improve this contribution.

In the revised version, we have incorporated most of the references and language corrections as suggested by Dr. F. Fusseis and clarified the mechanisms of the late stages of fabric development in the discussion. As suggested by the editor, we also performed additional cathodoluminiscence imaging, which has been added as a new subsection and a corresponding figure in the results section. Unfortunately, the collection of isotope data, fluid inclusion studies and synchrotron work is, as already anticipated by the reviewer, beyond the scope of this contribution. It would be interesting to test the proposed model with these methods, but it would require a separate study.

Below you will find our replies to the specific comments and suggestions raised in the reviews.

Kind regards,

Liene Spruzeniece and Sandra Piazolo

Reply to editor (L. Menegon)

Comment: I agree with both reviewers that the increased quartz content in the phyllonite is an intriguing feature that deserves a little more attention and possibly a more detailed discussion on its origin.

Response: As requested by the referees and editor, in the revised version of the manuscript we have extended the discussion on quartz incorporation and growth mechanism in the phyllonite fabric as well as added a schematical illustration of this process in Figure 12 (Fig. 12D). The abstract and conclusions are accordingly amended.

Comment: On page 1426 line 3-6 you refer to an increase in quartz grain size due to continuous precipitation in cavities. However, if cavities are dispersed in a polyphase matrix, grain growth is expected to be suppressed by second-phase pinning.

Response: The second phase particles would indeed inhibit grain growth, during dynamic recrystallization processes, where the second phases interfere with grain boundary migration by presenting an energy barrier for the moving grain boundaries.

In the presented model, however, we suggest that increase in qtz3 grain sizes in the polyphase quartz-muscovite domains is NOT related to grain boundary migration but rather occurs through addition of new material in cavities, created during GBS. If the cavity is created next to an existing quartz grain, the new quartz from the fluid will grow

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syntaxially on the surface of the pre-existing grains, which is the energetically favoured scenario when a surface of a same mineral is present in a supersaturated fluid. As a result the size of the individual quartz grains (Qtz3) in the polyphase mixture will increase and the interparticle spacing of the second phase (Ms2) will also increase as seen texturally in phyllonite A and B. To clarify this issue, we have added more detailed description in the discussion as well as an additional figure (Figure 12D).

Comment: Perhaps CL analysis might be an option to verify if there are sequential growth stages in quartz

Response: A new section on CL microstructures with a new figure (Fig. 11) is now added in the manuscript. A clear difference can be seen between the quartz microstructures in the shear zone margins and central domains, which we interpret to reflect the difference between the grain formation in dynamic recrystallization vs precipitation processes. The text has been changed accordingly.

Reply to referee #1 (F. Fusseis)

Comment: Obviously strain localization in granites has been studied extensively – to my surprise I found that some references that I think can add to the discussion, in particular of the microfabrics, are missing. Most notably these would be Fliervoet & White (1995), Fliervoet et al. (1997), possibly also Obee & White (1985).

Response: We have found these studies very relevant and added them in the discussion section and introduction.

Comment: Given the weight that the authors give to the effect of fluids and their sources, I did miss information on these. In particular the argument that the fluids involved in the formation of the orthogneiss were different from those infiltrating the phyllonite could be supported by stable isotope and/or fluid inclusion thermometry data. Andrew McCaig has demonstrated how we can elucidate on the nature of fluids affecting the deformation of granitoids. His work should certainly be discussed

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alongside Rob Kerrich's (McCaig et al 1990 and McCaig 1997 would be good starting points). While I see that stable isotope analyses might be beyond feasible at this stage, I would still invite the authors to describe the fluid inclusions and discuss the internally/externally derived fluids in greater detail.

Response: We agree that the suggested methods can help to clarify the exact fluid sources and contribute to a better understanding of the larger picture of the fluid flow in the middle crust. However, it is indeed beyond the scope of the current manuscript which is mainly focused on the microstructural and chemical processes in the fluid affected shear zones and the consecutive rheological consequences. Nevertheless, the references to the work by McCaig et al., 1990 and Kerrich et al., 1980 are added to the introduction and discussion, respectively, as suggested.

Comment: A similar set of retrograde hydration reactions from a very similar setup has been discussed by Pryer & Robin (1995). Although Spruzeniece and Piazolo do not report on flame perthite, the "cyclic reactions" of Pryer and Robin could still be a good frame for a discussion of their own, in particular since they integrate element transport over distances of many grain diameters.

Response: Thank you for the reference. The alteration patterns in Wyangala shear zones bears many similarities with textures observed in Grenville Front Tectonic Zone in Pryer's & Robin's (1995) paper. Although in our samples the flame structure in perthites were not well pronounced, the morphology of albite lamellae in some parts, associating with fractures and forming asymmetric rims around K-feldspar prophyroclasts suggest a replacement origin rather than exsolution, which fits in the "cyclic reaction" model. The reference is now incorporated in the discussion. We have also improved the readability of the chemical equations by following the example of Pryer and Robin (1995), where the abbreviation of the mineral instead of the chemical formulae is used in the balanced equation and the exact chemical formulae are shown in a separate table (Table 2).

Comment: And lastly, I still think Win Mean's short 1995 paper is an excellent background for a discussion of the rheological evolution of the Wyangala shear zones.

Response: We have now extended the discussion for the shear zone development incorporating the ideas of Means (1995).

Comment: The authors involve creep cavities and a granular fluid pump to transmit fluids through the phyllonites, however, no pores are ever shown. While I am the strongest advocate of their significance, I would like to see the authors support their claims by images of contextualised grain boundary porosity acquired by either SEM or, ideally, x-ray microtomography. Billia et al (2013) have recently demonstrated how synkinematic porosity can be described using the same methods that Spruzeniece and Piazolo applied and I would encourage the authors to attempt something similar to support their interpretation. It should be born in mind that transient fluid-pathways, especially during volume-changing rehydration reactions, can be created by other mechanisms than creep cavitation.

Response: As stated above (in the general response) even though such a study would be very interesting, it is beyond the scope of this contribution.

Comment: I was wondering whether there might have been different ways to locally increase the quartz content in their phyllonites? – The first that came to my mind is quartz veins obliterated by the mylonitic deformation? However, I do acknowledge the significance of their claim and would encourage the authors to support this aspect by more detailed observations.

Response: The possibility of a different protolith for the phyllonitic domains (including quartz veins) has been previously considered by the authors and discussed in the section 5.1. Although this scenario cannot be completely ruled out, no evidence was found to support it. Acknowledging the referee's concerns, we have restructured the section emphasizing the evidence on metasomatic origin of the phyllonitic domains. We have also added a reference to Figure 2, where the very gradual fabric development is seen between the phyllonite A (which is still relatively similar to the wall rock chemically) to phyllonite B (which is significantly enriched in quartz), atypical to what would be expected from a vein-derived protolith of phyllonite B.

Minor points:

Comment: Pg 1409, line 11-12: “but do not display significant displacement in respect to each other”. Why is the displacement of the grains WITH respect to each other relevant, when you describe their internal fragmentation?

Response: The question of the reviewer is based on a slight misunderstanding; we are referring to the individual fragments of the same grain, where the lack of displacement indicates that little total strain is accommodated in the fracturing process. This is now clarified in the text.

Comment: Line 23: aspect ratios

Response: Corrected

Comment: Pg 1410 line 15: Why is the direct contact to feldspar grains relevant here / out of context.

Response: As we further interpret the epidote to be a breakdown product of the igneous plagioclase, the lack of direct contact between the two phases indicates transport distances for the reaction components. This is now incorporated explicitly in the discussion.

Comment: Pg 1411 line 14: All muscovite is referred to as Ms2

Response: The sentence is deleted and the paragraph reformulated

Comment: Line 25-26: Consider reformulating for style

Response: Reformulated

Comment: Pg 1413 line 8: I believe there should be no comma after Qtz1 in “Qtz1,

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typically”

Response: Corrected

Comment: Pg 1417 line 1: “recycling” in a “closed system” conditions – remove “a”

Response: Corrected

Comment: Pg 1418 line 20: Double-check referencing style where you write “documented by (Menegon et al., 2011)”.

Response: Corrected

Comment: Pg 1419 line 23: “Minor fraction of” should be “A minor fraction of”

Response: Corrected

Comment: Pg 1421 line 27: I am almost certain that the gentleman is called J.D. Fitz Gerald

Response: Corrected. Our apologies to the highly honoured gentleman.

Comment: Pg 1422 line 28: “as being indicative to deformation” should be “as being indicative of deformation”

Response: Corrected

Comment: Pg 1424 line 8: “is interpreted to represents the” should be “is interpreted to represent the”

Response: Corrected

Comment: Line 9: “highly permeable” is a very vague descriptor that may mean completely different things to a reservoir engineer than to a hard rock geologist. Possibly reformulate?

Response: We agree that the term “highly permeable” may be misleading, hence it is now deleted, and the text slightly restructured.

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Comment: Line 10: “rock with a restricted pathways” should be “rock with restricted pathways”

Response: Corrected

Comment: Pg 1426 line 2: In which sense does a pressure shadow represent a fabric anisotropy, and why does fabric anisotropy lead to more (“higher”) dilation?

Response: The reviewer is correct, we used here a misleading term. It was meant as rheological anisotropy instead of fabric anisotropy, where the dilation sites are created between the rigid clasts and the soft matrix. We have restructured the sentence.

Comment: Pg 1427 line 4: I believe there should be a comma after “As a result”.

Response: Corrected

Comment: Pg 1427 line 16, 18: Castlemaine’s barley pop is correctly acknowledged as XXXX.

Response: Unfortunately we had no success in obtaining funding or other types of support from Castlemaine’s XXXX.

Reply to referee #2 (J. Marsh)

Comment: It is postulated that Phyllonite B (Ph-B) develops from a Ph-A-type state through a prolonged episode of fluid influx and precipitation of Qtz, enabled by the dynamic permeability associated with the creep-cavitation (CC) process. It appears that most of the available data are compatible with this model. The authors go on to describe that at some point enough Qtz is deposited to arrest the GBS process and cause a transition to a dislocation creep (DC)-dominated mechanism. This transition makes sense, and implies that this is a self-limiting process. So I am wondering A) how the process is initiated and becomes localized within an originally ∼uniform Ph-A zone (as depicted in Fig. 10B), and B) why GBS and localized fluid flow would persist for long enough in Ph-B to cause a 20x change in volume before the transition to DC.

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It may be worth discussing if some type of intra-Ph-A heterogeneity or perturbation is required to nucleate the CC-fluid influx process, or if it has only developed locally for some other reason. Then, possibly address why Ph-B would continue to evolve toward a mineralogical/microstructural state that seems less favorable to GBS, and if there may be some mechanical reason why GBS-CC may actually be favored by more Qtz up to some threshold.

Response: Based on the enquiries from both referees we have extended the discussion on the early stages of quartz incorporation in the phyllonite fabrics (Section 5.5.3). Answering to Dr. J. Marsh:

A) Judging from the microstructures, the first stages of the silicification in the phyllonite fabric seem to be related with porphyroclasts. As seen in phyllonite A, the initial fine grained matrix, created by the feldspar and biotite breakdown reactions, contains little quartz. However, the strain shadows and extensional sites between the two separating porphyroclasts (Fig. 6d-f) often contain fine grained quartz-muscovite mixtures, indicating that SiO2 is mobile and transported by fluid to low pressure sites. The rheological contrast between the rigid clasts and the soft matrix may be the initial factor promoting the formation of dilational gaps, pulling in pore fluid down in the pressure gradient, where the precipitation can happen more easily. With continuous elongation of these quartz-muscovite tails and necks, the individual grains will loose connection with porphyroclasts and transit to GBS due to the small grain sizes, localizing further fluid flow, as the intergranular porosity and cavities are created during the GBS process. In the manuscript we discuss the newly obtained CL images (Fig. 11) to show the intrinsic quartz zoning patterns in these settings, which we suggest to reflect growth from hydrothermal fluid.

B) GBS would persist as long as grain sizes are sufficiently small. Consequently fluid flow will be localized in these domains due to the creation of intergranular cavities created during GBS, which creates the necessary perturbations in the pressure gradient to attract pore fluids.

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Comment: The development of the marginal orthogneiss (to fairly high strain it looks like) is envisaged to have occurred simultaneously with shearing and early stages in the development of Ph-A. This is interesting from a rheological perspective because in this scenario Qtz would be behaving very differently in concurrently deforming adjacent regions. In Ph-A Qtz is stronger than the micaceous matrix, but in the gneiss it is weaker than the feldspar. If you do an A>B>C type reasoning then Ph-A should be weaker than the gneiss, and was initiated first by the envisaged fracture process. Yet a significant amount of deformation is accommodated in the gneiss. Why isn't all of the strain localized in Ph-A? I wonder if this could be used to qualitatively discuss the relative strengths of these 2 common crustal rock types and nature of strain localization (Newtonian vs. non-newtonian behavior) and their relationship to specific deformation processes (GBS vs. DC). I also wonder if it is possible that deformation in the gneiss could have happened after stage 3, once the Ph-zones hardened?

Response: The question concerning the timing of the deformation in orthogneiss is a valid and interesting point to make. However, from the data available at this stage we can only tell that the deformation in the phyllonite and orthogneiss is relatively contemporaneous as the products of the syn-tectonic reactions are very similar. We assume that although most of the deformation was indeed localized in phyllonite, the directly adjacent orthogneiss domains were also affected, but to a minor amount over a larger area. This scenario is consistent with our data and the most simple, hence we believe this interpretation is valid.

Comment: The influx of aqueous fluids seems to have played a major role in the evolution of deformation processes in this shear zone, as they do in many other geological processes. I am wondering if it is possible to estimate the volume of fluid that would be required to deposit that much Qtz and remove that much Na from the system. There may be too many uncertainties to calculate an actual value, but it would be nice to get a feel for the efficiency of fluid transport by the GBS/CC mechanism relative to channelized (fracture) flow or static porous flow, for a given solubility of Si at the ambient conditions.

Response: We agree that it would be an interesting exercise to estimate the fluid flux necessary to produce the structure we observe, however, as the reviewer already pointed out, it is very difficult to constrain the fluid composition which would be necessary for this task. A fluid inclusion study would probably help, but is unfortunately outside the scope of this contribution.

Minor comment: A table describing the defining characteristics of each type of a particular phase (Qtz1, 2, etc.), and where they are found could be helpful. They could be laid out more clearly in the text as well.

Response: We believe that referring back-and-forth between such a table and the text would be an inconvenience for the reader. Instead we have improved the text by repeatedly referring to the characteristics (in the result section) and interpretation (in the discussion) of each type of quartz. For example, in the revised version with markup on page 11, line 8 - “the monomineralic, fine-grained aggregates of Qtz2” and on page 25, line 21 - “dynamically recrystallized Qtz2”. We hope this change will be satisfactory.

Please also note the supplement to this comment:
http://www.solid-earth-discuss.net/7/C752/2015/sed-7-C752-2015-supplement.pdf

Interactive comment on Solid Earth Discuss., 7, 1399, 2015.