Dear editor and referees,

We would like to thank you for the thorough reviews on our manuscript. All the comments of the reviewers have been addressed, either as rewritten and/or added text in the manuscript or rebuttal comments. Below you can find a reply to each comment/question (answers are in blue). A file with the modifications performed, highlighted in blue is provided together with a clean version of the manuscript.

We hope that this new version of the manuscript will satisfy entirely the requirements for publication in Solid Earth.

Yours sincerely,

Anna Rogowitz and co-authors

Reply to reviewer comments

Comments anonymous Reviewer 1

General comments:

The first main general point, which requires discussion and revision, is the relationship between the mechanism of grain size sensitive deformation (gss) and the stress level determined. In an earlier paper (Rogowitz et al. 2014) stresses have been determined using the piezometer and wattmeter relationships form the literature. The strain rates are determined using diffusion creep laws. Here lies a major problem of this approach: The piezometer is obviously defined for dynamic recrystallization, i.e., for dislocation creep. If gss or diffusion creep deformation occurs, the grain size evolution will follow an unknown relationship, so that for this deformation the stresses determined from the piezometer do not apply - there is no piezometer relationship for gss deformation. From this point of view it is impossible to apply the stresses obtained from the piezometer to the diffusion creep law. The De Bresser assumption of the grain size to follow the creep law boundary in the deformation mechanism map is a so far untested hypothesis. This problem alredy semes to exist in the 2014 paper and it persists in this mansucript.

It is correct that in both manuscripts (Rogowitz et al., 2014 and the current discussion paper) the differential stress has been calculated by use of the paleowattmeter (Austin and Evans, 2009) and paleopiezometer (Schmid et al., 1980). The strain rate was then calculated by plotting the mode grain size of the ultramylonite together with its corresponding differential stress in a deformation mechanism map calculated by the use of the composite flow law after Ter Heege et al. (2004) using creep laws for diffusion (Herwegh et al., 2003) and dislocation creep (Renner et al., 2002). We argue in both manuscripts that the grain size of the fine grained ultramylonite developed by recrystallization processes and make the reasonable assumption that during GSS the grain size is not significantly modified (e.g. Bestmann and Prior, 2003, JSG 25 1597-1613). Therefore, we think that it is feasible to correlate the attained differential stresses to the strain rates. Further we agree that the field boundary theorem of De Bresser is a hypotheses but we think that this study is a good example to test it and it seems to be in good agreement.

The second general point is the absence of recovery/recrystallization concluded in this manuscript for the ultramylonite (see also the specific points mentioned below). The microstructures in Fig. 7 show plenty of substructures, some of which are ordered and most likely constitute subgrain boundaries. This observation is consistent with the misorientation diagrams of Fig. 4, which show a fair number of small angle misorientation boundaries. In addition, the microstructure of Fig. 2 b,c show a clear shape fabric, which generally is typical for rotation recrystallization microstructures, at least in quartz. From all of these different data sets, it seems likely that recovery has been active during the deformation of the ultramylonite.

We agree with the reviewer, strain energy reducing mechanisms are active. Nevertheless, we observe that rather cross slip than dislocation creep is the dominant strain reducing mechanism. We rephrased parts of the manuscript so that minor recovery processes by dislocation creep are not completely excluded.
We do not state that no subgrain rotation recrystallization has been active but we observe that the dominant recrystallization process resulting in the development of the ultrafine grained ultramylonite is bulging recrystallization (Fig. 6). Nevertheless, these recrystallization processes are the cause of the ultrafine grained ultramylonite and are not of great importance for its further deformation and therefore the focus of this manuscript.

Detailed comments:

line 22: omit comma and "first"

We deleted the comma and ‘first’

lines 35, 36: omit commas

We deleted the commas.

line 45: the reference of Barreiro et al. (2007) should be added here.

As suggested we added the reference of Barreiro et al. (2007).

lines 47-53: The connection of gbs and gss deformation is fine in that gbs definitely occurs during gss deformation, but it cannot necessarily be concluded the other way around: GBS occurs during most viscous deformation processes to some extent. It is active during dislocation creep as an accommodation mechanism, particularly when only few slip systems are available. So, gbs is not restricted to gss deformation, although it is a necessary condition for gss behaviour.

We totally agree with the reviewer. We do not state that GBS is restricted to GSS deformation mechanisms but argue that it can be active simultaneously to dislocation creep. Nevertheless, GBS itself is a mechanism being more efficient at small grain sizes.

lines 54-63: please state briefly how the strain rates wee obtained. It is described in the earlier paper, but it is easier for the reader to see it, and it is an important point.

We added information on the used flow laws for determining the strain rates.

lines 147-157: An easier method to determine the dislocation density is to simply count the number of dislocations in an image and divide by the area. This method yields a number per area count, which is avoids the potential error of the determination of the foil thickness.

Both methods are correct and can be used to determine the dislocation density, since the calculated dislocation densities are in a small range we stick to the results in this manuscript. The error depending on foil thickness is in this specific case relatively small.

line 166: please number the figures consecutively.

We removed the reference to figure 6.

line 171: The features in Fig. 3 g,h look more like normal etch pits on a surface. To call them Zener-Stroh cracks needs more observations or data.

At this point in the manuscript we solely describe the observed features as small cracks having the same orientation, later on (lines 415-422) th possible origin is discussed. Nevertheless we changed the corresponding Figure caption.

lines 181-190: There is a trend for low-angle-neighbour-relationships to be more frequent than expected in Fig. 4b. In addition, the misorientation map of Fig. 4c shows many small angle boundaries of a few degrees. Together with the observed shape fabric, these features seem consistent with rotation recrystallization.

Yes, the reviewer is right. There is a small trend for low-angle-neighbour relationships which we describe in lines 183 and 184. But we disagree on the observation of small angle grain boundaries in figure 4c. The misorientation deviation map shows that most of the grains have a uniform strain distribution so almost no subgrain boundaries occur. Even within the grains having a greater misorientation, the misorientation is rather smooth than resulting in a subgrain boundary. We do not observe recrystallization of the fine ultramylonitic calcite grains. The fine grained ultramylonite is the result of recrystallization processes but does not recrystallize itself.

line 201: omit the word "density"

We deleted ‘density’.

lines 221-223: There are clearly straight and partly well-ordered dislocation arrays in Figs. 7a,e,f, probably constituting the development of subgrains. This observation is consistent with those indicated in Fig. 4 and 3 (see above).

Yes, Figure 7f shows small cells in calcite, formed by arranged dislocations. Similar structures have been observed by Kennedy and White (2002) and differ from ‘typical’ well-ordered subgrain boundaries as they are less distinct and sharp.

lines 260-262: "b" is missing from the equation.

‘b’ is representing the Burgers vector length and was included in the equation. Due to comments of reviewer 2 (Hans De Bresser) the equation has been removed from the manuscript.

lines 261-282: Why not use only the grain size and the piezometer and wattmeter relationships? These seem to be much easier to use and yield more reliable and consistent results.

Yes, we agree that the paleowattmeter and piezometer result in more consistent and reliable results. However, the stresses calculated by use of dislocation density result in similar values showing that recovery had to be minor.

lines 283-288: yes, the results are better (see above).

See comment to lines 261-262.

lines 289-304: you cannot choose your slip systems according to what fits the stress values obtained. This leads to arbitrary results and circular arguments. First, at least one of the operating slip systems should be determined from the CPO data, and then the stress can be estimated for the Burgers vector determined this way. Probably it is best to omit the stress estimates form dislocation densities altogether.

As suggested we removed the results on calculated stress values using the theoretical relation between stress and dislocation density and therefore the discussion part on possible dominant slip systems and stress.

lines 306-308: here you come to the conclusion yourself: easier and more consistent results form piezometers and wattmeters.

See comment to lines 261-262.

line 315: the term "ductile" should be avoided here. The better term is "plastic" or "viscous".

We replaced ‘ductile’ by ‘viscous’. Plastic just refers to permanent and includes also non-viscous processes.

lines 320-325: When discussing these aspects, it is important to have some information about the grain size distribution. I have seen in the earlier paper that you have this data, and there is a large number of measurements. Please include the grain size distribution for the ultramylonite here. Important is the mode, not the average size. There seems to be a mode of about 3 microns. This number should be used for further calculations.

We added a grain size histogram to figure 4.

line 344: Fig. 5a does not show textures, you mean Fig. 4a?

We thank the reviewer for the correction.

lines 352-353: How do you conclude absence of grain growth? It is not trivial and no reason is given here.

The microstructures are described in detail in section 4. We do not observe any features that indicate grain growth due to grain boundary area reduction. Curved grain boundaries can be directly linked to strain energy reduction (Fig. 6). Additionally results from an earlier work (Rogowitz et al. 2014) show that subgrain size and recrystallization grain size are almost identic indicating that grain growth is absence.

lines 389-390: What do you mean by "different orientation of dislocations" - different slip systems? Edge or screw character? This is not clear from the text.

We rephrased the sentence and hope it is now clear that the various orientated visible dislocations are assumed to be related to the activation of different slip systems.

lines 390-391: but you do have substructures in Fig. 7a,e,f.

We rephrased the sentence so that minor strain energy reduction by dislocation creep processes is not completely excluded. Nevertheless, the presence of ‘classical’ subgrain boundaries is minor.

line 417: again (see above), the shape of cracks seems to be the result of surface etching by a fluid, so that the shapes of pores should not be overinterpreted, because hey may be the result of later dissolution. The same problem is associated with all such interpretations made by Mancktelow and others authors.

In case of later dissolution processes we would expect to see a similar pattern on both sides of the grain boundary and not limited to only one grain as observed in the ultrafine grained ultramylonite (Fig. 3g, h, 5f). Together with the identic orientation of the pores we think that the interpretation as Zener-Stroh cracks is reasonable. Nevertheless, we do not limit the development of cracks to one mechanism but discus different possible mechanisms in section 5.4.

line 429: How do you know that the conditions were dry? What is the evidence for that?

We do not see any mineralogical and/or textural evidence for the presence of fluids during the studied deformation period. Mineralogical analyses via EMP and CL analyses show a homogeneous chemical composition over the complete studied structure. Additionally we do not observe positive evidence of fluids by inclusion trails or fluid inclusions.

lines 432-436: again, substructures seem present, so that recovery cannot simply be ruled out.

See comment lines 390-391 and lines 221-223.

Comments Reviewer 2, Hans De Bresser

Specific comments:

I feel that the introduction would strengthen if the work is presented in the framework of a problem that needs a solution. The flanking structure studied is small scale, why is the result of the study of larger importance?

We would like to stick to the more general introduction since we feel that the manuscript might therefore be of interest for a broader audience. Instead we added a paragraph to the conclusion section.

P.2665 line 25. Please add a line how the strain rate mentioned was determined.

We added information on the used flow laws for determining the strain rates.

P.2667 line 5. It is not fully clear to me how these values of 80 and 1000 were inferred. Should I see that on Fig.1?

Assuming simple shear conditions a displacement of 120 cm over a width of 1.5 cm results in a strain of 80. If we assume further that most of the strain was accommodated by the ultrafine grained calcite layers having a total thickness of 0.12 – 0.48 cm the shear strain would reach at least 250 but could be as high as 1000. This value is clearly the uppermost bound but taking the ultrafine grain size into account not completely unrealistic. We added a reference to an earlier publication discussing the strain calculation in more detail.

P.2673 line 10. Why using the theoretical relation with assumed values for the parameters if there is a calibrated relation available for calcite? Note that De Bresser 1996 presents eq.3 also in the form of eq.2, so with the theoretical exponent of 0.5.

As suggested we decided to stick to the experimentally calibrated relation linking flow and dislocation density. The paragraph discussing the theoretical relation has been removed.

P.2674 line 7. I think the rex grain size piezometer of Rutter 2002 is a better one, and at least should be added as a reference. Also include what rex mechanism the piezometer is meant for. See also Rutter.

In a previous work (Rogowitz et al., 2014) we discuss different piezometer linking differential stress and calcite recrystallization grain size resulting in the conclusion that the paleowattmeter (Austin and Evans, 2009) and paleopiezometer (Schmid, 1980) seem to be the most accurate ones in this specific case while values calculated by the paleopiezometer of Rutter result in overestimated values. These higher values would predict deformation by brittle processes rather than ductile as observed in the microstructures. Therefor we would prefer to stick at this point to the used piezometers.

P.2674 line 15. It is probably a good idea to explain in a few sentences what the basics of the paleowattmeter are and how this relations differs from the more classical rex grain size piezometer. Also, Austin and Evans discuss in some detail specifically the application of the paleowattmeter to calcite rock, since it runs a bit different than the ones for other materials. Does this have consequences for the approach in the current paper?

As suggested we added information on the basic idea of the paleowattmeter and paleopiezometer.
The observation of Austin and Evans (2009) that grain growth rates of calcite during diffusion creep and static grain growth do not differ and therefore the mechanical work done by diffusion creep dissipates is interesting and important but does not have consequences for this specific study since diffusion creep is anyways neglected as deformation mechanism.

P.2675 line 3. Where does this idea of cross slip comes from? Only on p.2678, line17, the evidence is presented that leads to the cross slip suggestion. See also below.

Due to a comment of reviewer 1 and the paragraph has been removed.

P.2675 line 24-28. Fig.9 puts the ultramylonite exactly on the regime boundary. That would indicate that the two mechanisms both contribute to the strain rate, perhaps even almost equally (i.e. composite flow). However, in the remainder of the text, GBS is presented as the controlling mechanism, dislocation mechanisms only accommodating. Does not seem to be fully consistent. Of course, the boundary is in fact not a sharp line but rather a zone of a certain width. Please clarify your thoughts here.

We thank the reviewer for this comment and tried to modify the manuscript taking it into account. As the title of the manuscript already says the study observes simultaneous activation of dislocation movement and GBS, therefore the reviewer is right. The ultrafine grained ultramylonite deforms by both mechanisms.

P.2678 line 14-19. The part on cross slip is a bit confusing. Also cross slip is a thermally activated process, though influenced by stress, so can result in recovery controlled creep. In other words, it can take the place of dislocation climb in strain energy reduction. Further, the dislocation creep field of the deformation mechanism map, at the higher stress, is the power law break down of Renner et al, and that can be well explained by cross slip as controlling mechanism. But then we are talking about a full mechanism contributing to the strain rate (see point above) and not just an accommodation mechanism. Please clarify the role of cross slip.

We agree fully with the comment of the reviewer. By adding information on cross slip we tried to clarify its role as strain energy reducing mechanism in this study.

P.2679 conclusion 1. See earlier point on the shear strain. This range is now somewhat misleading.

We adapted the information on shear strain.

Conclusion 2. See points above on the difference between composite flow and pure GBS accommodated by dislocation mechanism. If text is going to be sharpened, this conclusion might need reformulation.

We modified the conclusion slightly so that it becomes obvious that GBS and dislocation activity are both operating simultaneously.

Conclusion 3. See point above about the thermally activated nature of cross slip, compared to climb. Reduction of strain hardening still possible, or is this what you want to say anyway?

Yes, we observe that the internal strain energy reducing mechanism is rather cross slip than dislocation climb. We hope that this is clearer after the adaption of the manuscript.

Conclusion 5. Suddenly Hall Petch appears. If not treated in the Discussion it should not be part of the conclusions.

We removed the paragraph from the conclusion.

Technical comments:

Abstract line 11. Why "evolution"? Not Development?

We replaced ‘evolution’ by ‘development’.

P.2666 line 17. Use MPa, not kbar. Sentence above uses GPa.

We converted kbar in GPa.

P.2671 line 25. Check sentence.

We corrected the sentence.

Fig. 1b not very clear. Porcelain?

We replaced ‘porcelain like’ by ‘ultrafine grained white appearing layer’.

Fig. 8. Piezometer of Rutter?

See comment to P.2674 line 7.

Additional changes made by authors

1. Due to some changes within the manuscript section 5.3 and 5.2 have been switched.
2. Table A1 has been adapted.