Comments on the manuscript: "Strain localization in ultramylonitic marbles by simultaneous activation of dislocation motion and grain boundary sliding (Syros, Greece)" by A. Rogowitz, J.C. White, and B. Grasemann

This manuscript presents a very interesting, important, and timely dataset, which has a great potential to help our understanding of the deformation of fine grained mineral aggregates. Therefore, I strongly recommend publication of the manuscript. However, before the ms can be published, there are some critical points which should be discussed or revised, so that major revisions are required.

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.

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.

For more detailed comments, please see below:

line 22: omit comma and "first"

lines 35, 36: omit commas

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

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.

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.

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.

line 166: please number the figures consecutively.

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.

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.

line 201: omit the word "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).

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

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.

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

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.

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

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

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.

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

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

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.

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

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 over-interpreted, because hey may be the result of later dissolution. The same problem is associated with all such interpretations made by Mancktelow and others authors.

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

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