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Interactive comment on "Strain localization in ultramylonitic marbles by simultaneous activation of dislocation motion and grain boundary sliding (Syros, Greece)" *by* A. Rogowitz et al.

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Strain localization in ultramylonitic marbles, Syros By Rogowitz, White and Graseman.

This is an interesting paper that contributes to our understanding of ultrafinegrained deformed rocks. The microstructural observations are detailed and well presented, and cover light optical to electron microscopy.

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?

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

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?

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.

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.

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?

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.

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.

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 con-

trolled 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.

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

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.

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?

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

Technical comments. - Abstract line 11. Why "evolution"? Not Development? - P.2666 line 17. Use MPa, not kbar. Sentence above uses GPa. - P.2671 line 25. Check sentence. - Fig. 1b not very clear. Porcelain? - Fig. 8. Piezometer of Rutter?

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