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# *Interactive comment on* "Deformation mechanisms and evolution of the microstructure of gouge in the Main Fault in Opalinus Clay in the Mont Terri rock laboratory (CH)" *by* Ben Laurich et al.

**J. Hadizadeh (Referee)**

hadizadeh@louisville.edu

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Laurich et al. have presented a reasonably well-written manuscript with a wealth of microstructural information on clay-rich gouges from a relatively young fault zone. In this sense it is an interesting contribution to our understanding of clay-rich gouges, which are often associated with more mature fault zones. The authors use a variety of analytical techniques and microstructural analysis all of which are adequately described. Based on this review and considering comments by two other reviewers, I believe the authors should make some relatively minor changes before the manuscript is ready for publication in Solid Earth.

1. The proposed hypotheses: The authors arguments regarding the “smear” hypothesis are vague at best. For example, why is it necessary for the possible source of the smear to be calcite-free (line 6 in 4.1)? Do the authors suggest the “smear” hypothesis as an alternative explanation only for the existence of the dark, calcite-free gouge type? What would be some relevant and expected characteristics of such “smeared-in” gouge? This hypothesis may even be considered untestable if a source for the smear cannot be identified, or is so highly speculative. The “authigenic generation” and/or “reworking of OPA” hypothesis on the other hand, is supported by evidence of progressive deformation (sharp difference in calcite content between the two gouge types; continuously traceable reduction in Riedel shear angles with rescept to shear zone borders, going from the calcite-rich gouge to the dark gouge; low clast-matrix ratios indicating higher strains in the dark gouge). As the authors have noted, different microstructural domains in the gouge indicate a number of deformation mechanism transitions that lead to relatively low-friction localization zones. This alone, questions the possibility of a “smearing” process since under steady P-T conditions smearing implies low friction of the smeared material. I agree with the authors that pure shear has been a factor in late development of the gouge layer geometries both due to change in deformation mechanism toward a less dilatant behavior as well as volume change. Such processes are more likely to be in-situ (or “authigenic” as the authors put it) as opposed to resulting from microstructural evolution of a “smeared” gouge. The evidence of amorphous $SiO_2$, which is clearly presented in Figs. 15 and 16 of the manuscript, is consistent with late stage hydrothermal $SiO_2$ in the gouge in agreement with comments by the anonymous reviewer regarding the stability of amorphous silica with T and age of the gouge. I suggest that the “smearing” origin of the studied gouge, as a distinct hypothesis, be removed. Given the authors current information, one may only include the possibility of spatially limited smearing event(s) within framework of the reworking hypothesis.

2. To strengthen the reworking hypothesis the authors need to strengthen the argu-

ments that support OPA to scaly-clay transitional stage during progressive deformation. For this, they may need to carefully reexamine Figs 2, 3, 4 for microstructural relationships between scaly wall rock (OPA) and the rest of the gouge types and chemical map of calcite related to these boundaries. The section 4.2.1 describing the transition from OPA to scaly clay appears to me inadequate in view of its significance as mentioned above.

3. In agreement with comments by anonymous reviewer about comparison of porosity measurements obtained via different methods, the use of mercury porosimetry in particular (cited estimates by the authors) is controversial because of uncertainties in non-fracture porosity values (usually overestimations) caused by elastic deformation of pores of different size. Estimates via careful image analyses is more reliable than this method. However, the authors have left readers to eyeball an estimate of the gouge porosity by looking at very few images in the manuscript (lines 5-8, P7), which could vary depending on the reader's experience with microstructural porosity. From Fig. 12e it is possible to suggest that higher porosity of OPA is due to primary porosity of mineral fragments (e.g. calcite and quartz) in the clay matrix.

4. Other comments: –I strongly agree with the anonymous reviewer about combining sections 3.3.4 (frictional granular flow) and 3.3.1(cataclasis and abrasion).

–In Fig. 3B, traces of internal shears seem to have a light green color. If this is not a compositional color coding, it is better represented in thin black lines.

–Fig. 8 lacks description for part D.

–Fig. 17e- where you have "(often with calcite clasts)", do you mean: common in calcite clasts?

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