Reply to the review of Ben Laurich RC#2

Dear Ben Laurich,

We would like to thank you very much for the thorough review and your suggested changes. Your constructive input strongly improves the quality of our manuscript. In the attached document, we present our changes and corrections to your individual comments. Your additional changes in the attached word document "se-2021-39-BL" are much appreciated; our comments and changes are listed below as well.

Kind regards,

Lisa Winhausen and co-authors

Main critical points:

1. Although dilation bands in OPA are shown for the first time, the finding of high permeability in the early strain evolution is not a novel finding. Veins along fractures in the so-called Main Fault of the Mont Terri Rock Laboratory have long been reported as indicators for paleo-fluid flow by numerous authors (see attached word file). This should be addressed.
   → We added this point to the text by including your suggested statement in 4.2.
   "At the same time, a local increase of porosity may imply an increased permeability. This is striking and important to note since µm-thin shear zones from the 'Main Fault' in OPA of the MT-URL usually present a decreased porosity (Laurich et al., 2017) and deformed OPA in direct-shear experiments shows a reduction in permeability (Bakker and Bresser, 2020). However, fracture-sealing veins (mostly calcite) in the Main Fault also indicate a paleo-fluid flow. Isotopic studies suggest that these veins formed contemporaneously with the initial fault activation, which supports or results in localised dilation in the early stage of shear development (Clauer et al., 2017)."

2. The use of the term "ductile": The stress-strain curve shows bulk brittle behaviour, yet there are two indicators for an uncritical strain evolution in parts of the sample: (1) bend and delaminated clay grains as well as (2) finely distributed brittle shear within deformation bands / cataclastic flow. These three scale-depended phenomenon (bulk, grain-scale and deformation band-scale) could be better distinguished and elaborated more. For each occasion of the term "ductile": To what scale does this refer to? Optional it could also be addressed: At what condition (P, T, strain rate) might a rather stiff mica grain bend? At what condition does a calcite grain break? At what condition does sub-critical fracture grow happen instead of seismical fracturing?
   → In section 4.1 we inserted the scales where processes are described. Furthermore, we inserted scale dependencies in the paragraph where we describe the deformation model.
   "Clay bending and intracrystalline fractures correspond to a coexistence of brittle and ductile deformation processes on the grain scale due to the contrasting mineral rheology." "These deformation bands are characterised by both brittle and ductile deformation in form of matrix cataclasis and reorientation of mineral fabric on the µm to tens of µm scale, even though on a bulk scale the sample deforms in a brittle manner."

3. Terminology: There is a little inconsistency in the use of deformation band, deformed zone, strain zone, shear zone, fracture and crack. I gave some suggestions on how to improve that in the world file. From du Bernard (2002), there is the term “dilation band”, too.
   → As also suggested by the other reviewers, we inserted a paragraph on our used terminology for describing the microstructures. We replaced ‘deformation zone’ by ‘deformation band’ and adapted, where possible, from Du Bernard 2002.

4. A statement on subsample positioning is missing. Might there be a spatial relation on strain localization and strain rate to the subsample position? The crack likely nucleated at the sample center, where there is a high abundancy of fractures, that distribute the bulk strain while at the sample edge there is only one fracture. Has brittle deformation started once that first single fracture reached the samples’ edge to create one sample through-going, cohesionless discontinuity? Might a shear zone at the samples’ edge represents a more brittle deformation, while a shear zone from the samples’ center might have preserved more of the prior-to-failure, uncritical fracture growth microstructure? A subsample off-center in the middle, such as in Fig. 6 providing insight in the most less and slowly strained shear zones?
   → We agree that there is a chance for different localized strain distribution. However, from our results we do not wish to relate the spatial distribution and the deformation mechanisms/microstructures due to missing evidence for this assumption. Presented deformation marker and MS has been found at several locations within the sample. Furthermore, many of the high-aperture macro fractures, i.e. where higher shear strain localisation is expected (distributed both close to the top and in the centre), were inferred to contain artefacts from sample preparation and are therefore excluded from analysis. We included a statement on this in the last section of the discussion 4.2:
   “We note here that the microstructure in vicinity of the larger macro fractures, i.e. close to the “main” fractures, where potentially most of the shear strain is accommodated, host microstructural artefacts from sample preparation (cf. section 3.2) and we hypothesise that with ongoing deformation shear strain will localise along a macroscopic shear band cross-cutting the sample with a reduction in porosity and grain sizes.”

5. The title sould be shortend, see attached word file
   - We changed the title according to your and the first reviewers suggestions.

For a point-to-point discussion, we inserted the major comments (typos, spelling mistakes etc. have been taken over as suggested) from the word file:

- reference to Busch et al. (2017)
  → We did not only compare to MIP measurements but to most of the methods used for porosity measurements by the authors (Busch et al. 2017), i.e. water content porosity, He-pyn., neutron scattering. We added the missing techniques to the text.
  “ […] OPA of 15.3 % (Houben et al., 2013, 2014). This value is slightly lower than petrophysical porosities measured by various methods such as water content porosity, Helium-pycnometry and neutron scattering techniques (Busch et al., 2017).”
- nomenclature on fractures/cracks/joints/shear zones, etc.; apature/width; general remark point 3)

→ We inserted a paragraph on the use of terminology for the manuscript (first paragraph in section Results). We decided to use the term fracture instead of crack, as many times it is not clear if fracture has a shear component or not. For describing fractures we used the term aperture, for shear /deformation zones we used width as suggested.

- This is interesting to observe in this rather rapid laboratory test. I looked for these "non-fractured" microstructure but couldn't find them in in-situ OPA. Maybe this is as only slickensides expose themselves easily. Finding this non-fractured but strained volume is like the famous needle in the haystack. I'd suggest to stress this finding.
  Plus, to me "ductile" behaviour of OPA was mostly thought of as finely distributed brittle shear (cf. scaly clay). Here, instead, it seems to be a grain-scale mechanism that differs from brittle. What is it though? Passive grain rotation as in the S/C bands of gouge? If so, these def. structures must be more dilatant in their early evolution to allow for grain rotation. See also main critical point #2.
  → We stressed the scale-dependent behaviour by including the scale for the different mechanisms at multiple locations in the text (and also the conclusions). We also included the meaning of 'ductile' behaviour where used in the text, e.g.:
  "Some of these deformation structures showed less dilatancy, mostly ductile deformation and a more pronounced shear component on the grain scale (Fig. 5(c)). Here, the SPO of non-clay minerals and clay aggregates was oblique with respect to the bedding and curved according to the shear sense of the micro-fracture."
- Such a drastic rotation can be two-fold: either, as in your fig 6, by the rotation of individual grains. Or, as within scaly clay, by the rotation of rigid microlithons. In the latter case, the MS is unchanged but rotated as a whole. In your case of fig 6c, I'd suggest to rewrite: "….the original SPO was broken up and elongated grains reoriented to a parallel alignment with shear movment."
  → We changed the text based on your suggestion.

- Where in Figure 6c is your "deformation zone"? I'd say the entire 10µm is a "shear zone", including the broken-up SPO grains.
  → We inserted the orientation of the shear zone to a micrograph (e) which applies for all the micrographs in this figure (mentioned in caption). From this, it can be seen that the SPO has rotated to be (sub-)parallel to the shear zone.

- So deformation bands are precursors to shear zones? There are dilatation bands first (du Bernard, 2002) and they evolve further?
  → We define our terminology in a newly added paragraph which hopefully clarifies the terminology used and avoids confusion. But yes, we interpret that shear bands form from deformation/dilation bands. One has to keep in mind that we do present a microstructure where "shearing", in a sense of localised shear strain along a distinct plane/broader zone, has just initiated; therefore, we do not call the "bands of deformation" rather "incipient shear bands".

- According to the paragraphs above, ductile indicators are: (1) bend mica and (2) delamination of clays, (3) maybe passive, sub-critical rotation of grains (see my comment above). However, (2) and (3) could be justified as brittle processes, too (see your next sentence). The problem here really is the lack of a good, comprehensive definition for "ductile" that is valid on all scales. Anyhow, at least the qz and ca grains do not seem to be deformed in a ductile manner. So I suggest to reword this sentence.

→ We agree on that and changed the sentence to stick to our definitions of brittle or ductile deformation, and to be consistent throughout the manuscript.
"[…], the deformation of both the clay-rich matrix and larger quartz, calcite and mica grains as well as pyrite aggregates is governed by brittle or ductile processes. On the one hand, grain scale clay splitting, grain abrasion and intra- and intergranular fracturing points to cataclasis localised in the deformation band. On the other hand, reorientation of clay aggregates and bending of elongated grains indicate structural reworking on grain scale to enable granular sliding."

- "… the ratio of porosity within and outside the deformation bands remains constant." I do not see any justification for this assumption. The deformed zones are cohesionless, the undeformed matrix has (little) calcite cementation. Maybe argument by using references that quantify the shaly facies' cohesion. Bear in mind that Houben et al. (see above) argue contradictory for unchanged, in-situ resembling MS & porosity no matter what drying method used.
  The idea of stored elastic energy from diagenetic bonds by Corkum et al. explicitly state that a break-up of the bonds is necessary, correct?
  → We agree that the equality of elasticities is an assumption and cannot be proven by our data. We changed the sentence accordingly by rephrasing it for a more hypothetical view. Nonetheless, we explain many aspects which bring strong evidence that the porosity in the deformation bands is increased in the experiment.
  "If we assume that during unloading the compressibility is roughly equal in and outside deformation bands and, then the ratio of porosity within and outside the deformation bands remains constant. Keeping all the above in mind, we infer that the deformation bands formed a local increase of porosity during the experiment."

- Circular argumentation. Elastic strains are recoverable by definition. If what is meant is the incomplete recovery from the strain before elastic yield limit (deviation from linearity in Fig.1 / hysteresis) than it cannot be inferred from your observations, so maybe add a reference.
  There is even an indication that shear zones from differing OPA facies have a differing tendency for strain recovery, i.e. hysteresis, in that pre-elastic-yield region:
  Laurich, B., Kaufhold, A., Hertzsch, J.-M., Gräsle, W., 2018. DB Experiment: Rock mechnical lab testing on samples from the BDB-1 borehole, Technial Note Mont Terri Consortium. Hannover, Germany.
  → We agree that our sentence was meaningless and inappropriate for the discussion; hence, we deleted it and changed the next sentence to fit the logical statement intended.
  "Elastic deformation took place by deformation of the solid component and of pores – this was inferred to be reversed by unloading the sample."

- Why non-localised? Would that mean finely and manifold distributed crack nucleation?
  You write above that the orientation and distribution of cracks remains unknown. However, triax-samples often fail somewhat in the middle of the sample length. In almost any case (as with this sample), there is a locally distinct failure. I reckon that this plastic failure is preceded by a local accumulation of elastic strains, too.
  Plus, Fig. 3 shows a higher fracture abundance in the center, while only one fracture reaches the sample edge. This indicates sample internal failure / strain initiation. With cohesion overcome by the first (and only) fracture reaching the sample edge, displacement sets in rapidly (Fig 1) and

doesn't form new fractures but offset only on the existing fracture (argumentation for a dominance of brittle processes close to and after bulk sample failure)
See also your own argumentation below.
→ The crack/fracture nucleation, i.e. at an early stage of deformation, is interpreted to happen in a non-localised manner in the sample with a denser accumulation in the center of the sample. This has been demonstrated for brittle deformation rocks (e.g. Kranz 1983 and references therein) measured by micro-acoustic emission tests and was observed on our sample by multiple micro-fractures of set 1(green) with tensile splitting. We do not state that the fracture coalescence and later shear strain development is non-localised, we rather say the opposite. You state that the failure is along a larger fracture in the middle of the sample – and this is true – but in this sentence, we do not refer to the failure of the sample. We added the reference.
" […], and it is interpreted as a non-localised process within the sample as reported in other studies (Kranz 1983 and references therein)."
In section 4.3, we changed the sentence slightly in agreement with the observations and our interpretation
"These fractures form in the whole sample with preferred orientations crossing the bedding and with ongoing deformation they concentrate with a higher density in the centre."

- Only an optional suggestion: I don't think there is any evidence-micrograph for this in any study. But if so, I suggest to cite it here. Why is this important to me? I reckon that these uncritical crack propagations are controlled by several environmental factors, foremost by strain rate. Insight here could justify the comparability of lab and in-situ crack formation.
  → We missed to insert the reference to Fig. 5C, where indeed ductile deformation, in close to fracture tip regions and at the boundary between deformation zone and wall rock were observed. We agree that strain rate plays an important role for the deformation structures and related mechanisms. We added a paragraph that to the discussion 4.2 second last parargraph and conclusion.

- Can you provide an estimate on the displacement, too? How many µm? For a shear zone from the MF in MTURL, I found a tool mark of about 2 cm length.
  → As stated in the manuscript, we could not find correlation indicators, hence no estimate on the displacement was possible.

- Not necessarily: The def. band can change from dilation, leaving space for first particle reorientation, to compaction band, wherein µm-thin shear zones with slip-aligned particles and reduced porosity can also cause the bulk compaction by a reduction in the bands' width.
  → This is a possible hypothesis; however, in our study we could not find any indication for the closure of the deformation band. Hence, we did not include this into our discussion.

- Why is a deformation band preservation favourable over the development of a single, through-going shear zone such as the Y-shears? The later would be energetically more favourable, as frictional forces would be reduced. And if I get it correctly, that is what you observe in Fig. 5c and Fig. 6 a, c, d: Movement on a distinct deformation band boundary. Why is this important to me? Does this relate to the formation of in-situ scaly clay? There it is one potential evolution that the internal structure of a band (in the scaly clay case an S/C band) form passively in between a (growing) relay of two bounding µm-thin shear zones that accommodate most of the displacement.

Here, it seems to be the other way around: dilation band first, then further localization at a boundary. Could this effect of grinding down microlithons during the preservation of a deformation band have a lower size limit? If so, will it eventually form gouge or a single, through-going fracture or, as you seem to suggest, maintain a smallest microlithon size? (cf. Laurich et al, 2017, 2018).

→ We agree that the full analysis of the formation and development of the deformation bands is still poorly understood and that more research (more tests and MS analyses) is required here. However, we present our interpretations based on this only test and will not hypothesise beyond the scope of this study in much more detail regarding their deformation mechanisms as analysed by Laurich et al. Again here, one has to keep in mind that the BC used for this test are somewhat different from the in situ conditions for naturally deformation OPA. We discuss why naturally deformed OPA is different from our observations at several parts in our manuscript and name additional effects that might play a role.