Reply to the review of RC#3

Dear reviewer

We would like to thank you very much for the thorough review and your suggested changes. Your input strongly improves the quality of our manuscript. In the attached document, we present our changes and corrections to your individual comments.

Kind regards,

Lisa Winhausen and co-authors

Main comments:

- The authors should consider the role that mechanical anisotropy has on their results. It is mentioned briefly in the introduction, but then very little attention is given to this thereafter. I feel that some of the observations, for example the orientation of tensile cracks being oblique to the loading direction, could be explained by the mechanical anisotropy present within the material. I have suggested some studies which the authors may find useful on the subject.
  → We agree with this point. Therefore, we added a paragraph on the influence of anisotropy on the fracture propagation/deformation in shales by also including your suggested references in the introductory part. We also discuss the mechanical anisotropy in the discussion 4.1 + 4.2 while suggesting that more work is needed on different sample configurations (load direction to bedding orientation) to constrain dependencies.
  1. Introduction:
  "Additionally, Ibanez and Kronenberg (1993) distinguished deformation modes based on the loading direction with respect to bedding. Individual and conjugate fractures with orientations between 33° to 73° towards horizontal were found in samples loaded normal to bedding, whereas samples loaded parallel to bedding are prone to develop macroscopic kink bands with orientation of 47° to 75° and material rotation. Shear zones with orientations of 56° to 46° including enchelon fractures are typically observed for sample where maximum load is oriented by 45° degrees to bedding. The influence of anisotropy on fracture mode and propagation has also been studied in fracture toughness experiments on shales and shows that the direction of fracture growth is dependent on i) the orientation of anisotropy, ii) the extend of anisotropy and ii) the loading mode (Forbes Inskip et al., 2018; Nejati et al., 2020)."
  4.1 Discussion:
  "Fracture kinking or deflection is a common process in anisotropic rocks and is due to the competing influences of applied load direction and the direction of anisotropy plane (Forbes Inskip et al., 2018; Nejati et al., 2020). Our observations suggest that both factors play a role for fracture orientation and propagation since, on the grain scale, fractures cross the bedding and form along bedding (Fig.4a, Fig. 5). However, more work on samples with various loading configurations is required to quantify the influence of mechanical anisotropy on the fracture orientation."
  4.2 Discussion:
  "Tensile microfractures form as obliquely – instead of vertically – oriented fractures along grain boundaries, which is inferred to be a result of loading direction and the orientation of anisotropy plane."
  "[…].These anastomosing fracture networks also show the influence of mechanical anisotropy as dilatant fractures form along the bedding plane as opposed to the general orientation of the fracture network (Fig.6(a))."

- There is an inconsistency when describing the orientation of structures or processes, where the authors use the sample orientation, bedding orientation and loading orientation throughout. I found this confusing when reading the MS and I would recommend that the authors use the bedding orientation only. There is one point (which I highlight below) where it is also appropriate to refer to structures/processes relative to the loading direction as well, but apart from that I would just stick to bedding orientation.

→ We understand the confusion and changed the orientations in the manuscript to fulfill our definition of angles measured. We measure all angles from the horizontal and inserted a sketch in Fig.3 for clarification. We decided to measure the angles from horizontal as this is – to our understanding – more convenient since the bedding is inclined as well and one would always have to calculate what orientation this would be towards horizontal (or vertical, i.e. principle stress directions). Additionally, we inserted for all micrographs the orientation for both fracture sets measured on the macro scale to better relate the orientations on all scales described. We hope that you will agree with this.

- At the end of the MS the authors run the risk of undermining their own great work by suggesting that their results do not compare well with naturally deformed samples of OPA. This may be the case, but then you need to stress why your contribution is still valuable, and give more details on why you think there are differences, and what can be done in any further work. The authors give a vague answer to this, but I would suggest that they end on a stronger note.

  → We completely agree with this point and changed parts of text by discussing the differences but also presenting some more similarities (Last third of 4.2). Furthermore, we included a comparison to artificially induced (excavation induced) fractures from the EDZ and BDZ (last two paragraphs in 4.2). We also included some more suggestions on what to improve in future experiments. (central paragraph in conclusions) and incorporated these points to the conclusion.

  4.2 Discussion:

  "[…]. Some structures found in this study, such as the anastomosing fracture network, strained fossil shells and bending of phylosillicates resemble those found in highly strained OPA (Laurich et al., 2017; Orellana et al., 2018) […]"

  "[…]. This is striking and important to note since µm-thin shear zones from the 'Main Fault' in OPA at 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)."

  "Many of these processes become effective at long time scales under natural conditions. In tunnelling however, low-permeable rocks such as OPA are subjected to undrained loading conditions during excavation. Thus, the triaxial compression tests have been performed as classical UU tests without prior saturation and consolidation. The pore water pressure development and effective stress inside the sample remain unknown. The test provide the undrained shear strength representative for the nearfield of a tunnel excavated at the consolidation conditions found at the MT-URL. Therefore, micro-deformation processes observed in the sample are considered similar to those found in the near-field of a freshly excavated tunnel in Mont Terri. This is further corroborated by the similarities between microstructural EDZ and BDZ (borehole damage zone) observations (Bossart et al., 2002, 2004; Yong et al., 2007; Nussbaum et al., 2011) and microstructures found in this study.

  These studies show that purely extensional fractures coexist with shear fractures in the EDZ/BDZ. The latter – compared to tectonic fractures – are characterised by poorly developed striations but indicate re-oriented clay particles (Nussbaum et al., 2011). Furthermore, BDZ structures in a resin-impregnated over-core from the shaly facies of OPA at MT-URL showed branching fracture patterns on a mm-scale similar to the structures observed in our study (Kupferschmid et al., 2015)."

Specific and Technical Comments

Line 18 – Why test at this orientation? This is also a general question, not just specific to this line.

→ This contribution aims at giving a first insight to the grain-scale deformation mechanisms of OPA from a single test, which has been performed in the past. The sample originated from the study Amann et al. 2012, which have tested a series of S-samples (bedding normal to sigma 1) under various confining stresses. We fully agree that this is only one end-member and more tests are required to study the influence of anisotropy on the deformation behaviour and processes. This is stated in the text twice.

Line 28 – Could you briefly state what the major effect on permeability evolution is in the abstract?
→ we added this information to the sentence:
" […], with an inferred major effect on permeability by an increase in hydraulic conductivity within the deformation band."

Line 51 – Are the bedding parallel cracks likely to exist at depth? Or are they only present on exhumed material that has therefore been unloaded? This may be pedantic but the way this reads at the moment suggests that these exist at depth, like the pore space in the fossils, pyrite or clay matrix (which all presumably exist at depth).
→ We included the prefix 'SEM-'visible void space to underline that this description refers to samples analysed in laboratory. Fr the case of bedding-parallel fractures, we discuss their origin in section 3.2, second parargraph.

Lines 61-64 – There has been a renewed interest in looking at failure in shales at different orientations see the following papers:
Nejati, A. Aminzadeh, F. Amann, M.O. Saar, T. Driesner (2020) Mode I fracture growth in anisotropic rocks: Theory and experiment. Int J Solids Struct, 195 pp. 74-90, 10.1016/j.ijsolstr.2020.03.004
Gehne S, Forbes Inskip ND, Benson PM, Meredith PG, Koor N (2020) Fluid-driven tensile fracture and fracture toughness in Nash point shale at elevated pressure. J Geophys Res: Solid Earth 125:1–11. https ://doi.org/10.1029/2019J B018971
Forbes Inskip ND, Meredith PG, Chandler MR, Gudmundsson A (2018) Fracture properties of Nash Point shale as a function of orientation to bedding. J Geophys Res: Solid Earth. https ://doi. org/10.1029/2018J B0159 43
Chandler MR, Meredith PG, Brantut N, Crawford BR (2016) Fracture toughness anisotropy in shale. J Geophys Res: Solid Earth 121:1–24. https ://doi.org/10.1002/2015J B0127 56
Although these studies mainly use unconfined tests, they demonstrate how the fabric, and alignment of grains in shales has a significant effect on fracture propagation and mode of failure. Although I do not think an in depth discussion of these papers is required, they should at least be mentioned here.
→ We thank the reviewer for these references. We cited them in the text as they provide high quality research to the deformation (anisotropy) of clay-rich rocks/shales.

Line 86 – What is the range of confining stresses tested? You quote the strain range (>20%) so you should also quote the stress range.
→ We inserted the stress range used in the references cited.

Line 99 – This should be "gouge" not "gauge"
→ Corrected

Line 101 – I don't think "modelled experimentally" is the best phrase here, perhaps "has been analysed experimentally by use of direct shear tests on samples sheared both parallel......"
→ We changed the sentence according to you suggestion.

Line 128 – Why did you choose to core the material in this orientation? Are your tests on samples in this orientation the most representative of what you would expect on structures found at Mont Terri and elsewhere in the OPA? As you mentioned earlier, and in the references I provide above, loading orientation is known to be a significant contributor to failure (fracture mode, strength etc) in transversely isotropic rocks. It would be good to show the reasoning for your sample orientation choice here.

→ Here we would like to refer to comment #1 (line 18) above. We certainly agree that the bedding orientation towards loading plays a fundamental role, but at the time this manuscript has been prepared, we have not finished our study on different loading configurations - it is currently in progress and submission will be in 2021/2022 to constrain a broader spectrum of loading configurations.

Line 134 – Similar to my comment above, why did you choose a circumferential displacement rate of 0.08 mm/min. Many failure processes are heavily rate dependent, are these rates relevant to the application of your study? Or is this rate defined in an ISRM suggested method?

→ The test aimed for undrained conditions. i.e. the test needed to be conducted with a deformation rate appropriate for the sample size to fulfill undrained conditions. As suggested in a draft for IRSM (Fairhurst and Hudson, 1999) Amann et al. 2012 adapted the strain rate slightly from 1E-6 1/s as suggested for radial controlled strain deformation to 4,8E-6 1/s, i.e. 0.08 mm/min. We inserted the reference to the text.

Line 139 – This section relates only to sample preparation for the image analysis, and so should be titled as such. Something along the lines of "Sample preparation for image analysis". Furthermore, I would also change section 2.1 to something like "Material description and core sample preparation" or "Material description and sample preparation for mechanical testing" if you want to keep the two types of sample preparation separate. Either that or you could remove lines 126 – 130 from section 2.1 and incorporate it with section 2.3 for a more general sample preparation section, but this should then come before the section on Triaxial testing.

→ We agree and changed the section title according to your seggestions:
2.1 Material description and core sample preparation
2.2 Sample preparation for image analysis and BIB-SEM

Line 153 – Do you really need to use the acronym ROI for regions of interest? I do not think it saves much in the way of words, and in my opinion the use of another acronym is confusing for the reader. You also only use it once in the whole MS.

→ We agree and deleted the acronym.

Line 163 – I would use either sub-horizontal or parallel to bedding, and not both. My preference would be to always use an orientation related to the bedding, so parallel in this case. The reason being that the bedding relates to the fabric of the rock and will remain (relatively) constant, whereas horizontal or vertical can be different depending on how you orientate your sample. Also be consistent then with how you define both fracture sets, i.e.oblique to bedding (fracture set 1) and parallel to bedding (fracture set 2).

→ We see a point in relating the orientation towards the bedding direction and would usually agree. But as the bedding, in this case, is oriented approximately 9° from the horizontal (not an 'ideal' S-sample) it would make things more complicated. Therefore, we measure all angles from horizontal (for clarification we inserted a sketch in Fig.3). We changed all text parts, where possinle, to make this consistent throughout the manuscript.

Line 205 – This should be "saddle reef pores" rather than "raddle reef pores" I think. I have to admit, this is not a term I am familiar with. Do you need to define this, or at least annotate examples in Figure 7?

→ Typo corrected. This term stems from (micro)structural geology and has been used elsewhere, we inserted the citation and inserted an annotation to Fig.6

Lines 211 – 214 – It is not clear to me where these porosities have been measured. Could the sub-areas used in this analysis be marked on Figure 7?

→ We inserted the locations for the porosity measurements in Fig.7 (blue boxes, dashed lines). We also inserted a reference in the text. There is one regions missing on the figure, which was used to calculate the porosity of the deformation band; we decided not to show this image as which would have caused information overload of the figure, it is however located in the deformation band 7(d)

Lines 219 and 224 – I think you have mixed up fracture sets 1 and 2 here. In section 3.1 you define fracture set 1 as the set oblique to bedding while fracture set 2 is parallel to bedding. Please amend to be consistent.

→ That's true. We corrected the false numbering.

Line 226 – Here you go back to defining features from the horizontal rather than bedding. Switching between the two is confusing, so please stick to one or the other. As mentioned previously I think orientating with regards to the bedding is better.

→ We decided to measure the angle with respect to horizontal (see comment above). We are therefore consistent throughout the manuscript.

Line 230 – Further to my previous comment: You now use the bedding direction rather than horizontal. You use both within the same paragraph, please be consistent.

→ We inserted a comment, which states that these fractures are oriented with ca. 9° towards the horizontal, which is in our case the bedding orientation. We feel that expressing the orientation in relation with bedding is needed, as desiccation cracks are prone to develop along the bedding.

Lines 233 – 234 – "In general, the deformation of both the clay-rich matrix and larger quartz, calcite and mica grains is brittle, ductile or a combination", brittle, ductile or a combination covers everything and so this sentence in its current form is rather redundant in my opinion. I understand what you mean, in that you do not just have one type of deformation and that both occur, but maybe then consider re-writing the sentence to something like "We observe both brittle and ductile deformation in the clay-rich matrix and larger quartz, calcite and mica grains, and so deformation is not solely brittle or ductile."

→ As also criticised by the other reviewers, we changed the sentence to express the information is a better sense.

"is governed by brittle or ductile processes."

You also list clay-rich matrix, quartz, calcite and mica here, what else is there of significant quantities? Do you not see deformation in the Iron rich minerals and/or feldspars? If not is this because they account for <10% of the rock (from section 2.1.), and therefore could this simply be a sampling bias? I just wonder if you need to specifically list clay-rich matrix, quartz, calcite and mica, or if you could just say that you observe both brittle and ductile deformation. Finally, I personally do not think you need to say a combination of both, as I believe it is covered when saying that you observe brittle and ductile deformation. However, if you feel strongly about keeping that in that is fine.

→ We agree with your point and added pyrite aggregates to the sentence since these present also deformed patterns. Feldspars are rarely observed in our samples; this could be certainly be due to sampling bias and the fact that feldspar is only a minor component. Nevertheless, we would like to keep this sentence as is, and list the mineral component to stress that deformation has been taken place in all mineralogical phases regardless the rheology contrast.

Lines 265 – 268 – Similar to my comments above, the way that the first sentence here is written is a bit redundant as elastic and inelastic deformation covers everything. Again, I understand what you mean in that both occur, but then consider re-writing to something like "We observed that both elastic and inelastic deformation occurred during testing, and that both occurred simultaneously". You mention in line 268 that both occur simultaneously but have you got evidence for this? Also could pore compression (line 266) not be inelastic rather than just elastic? Or do you consider inelastic pore compression as pore collapse?

→ This sentence, also according to the other reviewers comments, has been changed based on your suggestion. We changed the following sentence as well to make the argumentation more consistent. We can see that plastic and elastic deformation occurs simultaneously as the deviation of the volumetric stress-strain curve starts early (at low axial strain) and is therefore inferred to occur simultaneously. We also state that bulk compression (i.e. clay matrix compaction and pore collapse) is a part of plastic deformation (= inelastic).
"In our triaxial experiment, we observe that elastic and irreversible inelastic deformation occurred, and that both occurred simultaneously. Elastic deformation took place by deformation of the solid component and of pores – this was inferred to be reversed by unloading the sample. From our SEM investigations, plastic deformation was governed by damage accumulation in deformation bands, but also by homogeneous matrix bulk compaction (cf. Allirot et al., 1977). Bulk compression in clay-rich rocks is associated with clay matrix compaction and pore collapse (Schuck et al., 2020). […]"

Line 274 – Here you now use angle with respect to the maximum principle (presumably – but need to state this, as in engineering σ1 is commonly the maximum principle tensile stress) compressive stress σ1 in addition to angle from bedding and horizontal as before. I can understand why you may want to use σ1/loading direction here as well, but then you should also indicate which fracture set these structures relate to (Fracture set 1?).

→ We inserted the information to what fracture set we are referring to and added – to be consistent the orientation towards horizontal.

Line 284 – It is interesting that your microstructural analysis suggests that tensile cracks form as obliquely rather than vertically (parallel to the loading direction) orientated cracks. You do not mention this here but could this not be down to the transversely isotropic nature of the material, and that you load the sample oblique to bedding? Literature on the fracture toughness of shales at different angles to bedding suggest that during crack growth there is a competition for a tensile fracture to form parallel to load and parallel to the plane of weakness i.e. the short-transverse orientation. As a result, the actual fracture orientation lies somewhere between the two. Again, see the following:

- Nejati, A. Aminzadeh, F. Amann, M.O. Saar, T. Driesner (2020) Mode I fracture growth in anisotropic rocks: Theory and experiment. Int J Solids Struct, 195 pp. 74-90, 10.1016/j.ijsolstr.2020.03.004
- Forbes Inskip ND, Meredith PG, Chandler MR, Gudmundsson A (2018) Fracture properties of Nash Point shale as a function of orientation to bedding. J Geophys Res: Solid Earth. https ://doi. org/10.1029/2018J B0159 43
- Chandler MR, Meredith PG, Brantut N, Crawford BR (2016) Fracture toughness anisotropy in shale. J Geophys Res: Solid Earth 121:1–24. https ://doi.org/10.1002/2015J B0127 56
  Again, these studies are unconfined but I think that the formation of (apparent) tensile fractures in these studies may explain some of the observations that you have here with regards to the orientation of tensile cracks.

→ We agree and comment on this in the discussion 4.1. See also your main comment #1.

Line 292 – Should be "changed" not "changeed"
→ Typo corrected.

Line 320 – You use the British spelling of characterised here (with an s rather than a z) but use American spelling elsewhere (line 14, 84). Generally you use American spelling throughout, but be consistent.
→ We made a spelling/grammar check and converted all inconsistencies to BE.

347 – You use the term obliquely orientated here, but obliquely orientated to what, bedding, horizontal or σ1? You use all three in the MS, and it may be that the cracks are oblique to all three, but you should state what the cracks are oblique to here.

→ We inserted *"to horizontal"* to be consistent within out MS.

Lines 351 – 352 – Here you state that there are few similarities with naturally deformed OPA. The manuscript and study are really interesting, however as you point out in your abstract important questions remain, particularly in relating data gathered in the lab to that observed in the subsurface. Your statement here suggests that you are not able to apply the results of your work to subsurface processes on a larger scale, which in my opinion undermines the great work that you have done. Although it may be true that your results do not corroborate well with naturally deformed OPA, you should expand on why you think this is, and how then your results are relevant. In the final lines of the MS you briefly suggest what could be done next, but you could expand on this to suggest how this study can better inform what types of experiments should be carried out next to tackle these open questions.

→ We completely agree with this comment and as suggested also by the other reviewer, we changed the text and point out (besides the differences) what similarities we found. Furthermore, we included a paragraph on in-situ excavation induced fractures which show also similarities and the significance to the results found in the study. We additionally comment on the experimental BC and relate them to the in-situ BC and conclude therefore structural difference to other studies. Finally, we shortly summaries what experiments should be carried out and – beyond those BC – further processes may take place in the deformation process, which have not been considered in the analysis of this work. A summary of these points is presented in the conclusions as well, see also your main comment #4.

section 4.2: "This indicates that the matrix compaction was dominating the bulk deformation process to this stage. 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 at 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). The local increase of porosity in the deformation band leads to a local reduction in pore water pressure under undrained conditions and a related local increase in effective stress in the shear zones. The increase in effective stress involves a shear hardening effect, which represents the competing effect of shear softening due to the loss of cohesional bonds. Here, further research is required to investigate the dependency of permeability and porosity evolution with increasing amount of shear stress and the effect of undrained and drained conditions within the sample.."

[…]

"Many of these processes become effective at long time scales under natural conditions. In tunnelling however, low-permeable rocks such as OPA are subjected to undrained loading conditions during excavation. Thus, the triaxial compression tests have been performed as classical UU tests without prior saturation and consolidation. The pore water pressure development and effective stress inside the sample remain unknown. The test provide the undrained shear strength representative for the nearfield of a tunnel excavated at the consolidation conditions found at the MT-URL. Therefore, micro-deformation processes observed in the sample are considered similar to those found in the near-field of a freshly excavated tunnel in Mont Terri. This is further corroborated by the similarities between microstructural EDZ and BDZ (borehole damage zone) observations (Bossart et al., 2002, 2004; Yong et al., 2007; Nussbaum et al., 2011) and microstructures found in this study.

These studies show that purely extensional fractures coexist with shear fractures in the EDZ/BDZ. The latter – compared to tectonic fractures – are characterised by poorly developed striations but indicate re-oriented clay particles (Nussbaum et al., 2011). Furthermore, BDZ structures in a resin-impregnated over-core from the shaly facies of OPA at MT-URL showed branching fracture patterns on a mm-scale similar to the structures observed in our study (Kupferschmid et al., 2015).

Studies on samples deformed under undrained consolidated conditions to higher strains are currently under investigation and will address the effects of pore pressure, effective stress conditions, and sample orientation on the development of deformation microstructures."

section 5: "Our high-resolution study gives insights into the macro- and microscale structures with some similarities to excavation or naturally induced structures. However, OPA - deformed up to peak stress - also shows structural differences to naturally deformed OPA, which shows drastically reduced porosity. Strain amount and rate in natural fault systems, as well as elevated temperatures or clay-mineral hydration processes may influence the deformation mechanisms and structures. On the other hand, natural veins as an indication for paleo-fluid flow in the early faulting stage (Clauer et al., 2017) support localised dilation as similarly found in this study. Future experiments covering a broader range of boundary conditions on consolidated samples are required to analyse the deformation processes under shallow upper crustal conditions, and to formulate an effective stress dependent deformation model. "

Line 521 - 522 – change "was conducted circumferential-displacement-controlled..." to "was conducted using a circumferential-displacement-control rate of 0.08 mm/min...".

→ We changed this sentence in incorporation your and the other reviewers suggestion. *"The strain-controlled test (circumferential-displacement-control rate of 0.08 mm/min) was performed at 4 MPa […]"*

Figure 3 right – The axis is labelled "angle" but what angle is this referring to? It is not made clear in the figure caption and given that the horizontal and the bedding direction are within 10° of each other it isn't immediately obvious which angle you are referring to. Please re label the axis "fracture angle to bedding/horizontal (as appropriate)"

→ We inserted a sketch in Fig.3 illustrating the definition of angles used in our MS. We refer in the text – if not explicitly mentioned – always to the definition as shown here.

Lines 536 – 537 – Here you just say "the oblique orientated fracture set" If you compare both fracture sets to the horizontal (which you do in line 537), they are both oblique i.e. not parallel or perpendicular to the horizontal. This also goes back to my previous comments of use one frame of reference only, bedding or horizontal, throughout. Bedding would be my preference.

→ We agree that both fracture sets are oblique and we deleted "oblique" in line 536-537 and added the missing information of what fracture set we are referring to. However, we would like to stick to our definition for angles as explained above.

Figure 4 – Is Figure 4 (b) an inset from Figure 4 (a)? If so indicate where it is from. Also put a scale in Figures (c) and (d)

→ No 4(b) is not an inset from Fig.4(a). Scales for (c) and (d) were added.

Figure 6 – Should the text in Figure 6 (a) be "Bent mica" rather than "Bend mica", this is also true for the figure caption and line 195 in the main text.

→ All mentioned text passages were changed.

Line 565 – Here you reference (a, a', a") but only a and a' are shown in the figure. Also this may seem pedantic, but in the figure you use the prime symbol whereas you use the apostrophe in the figure caption, this should be consistent.

→ a'' was deleted (remnant from a former version of the manuscript). As the font used in the figures is Arial (as suggested by EGU Solid Earth guideline), the two symbols appear the same unfortunately (as opposed to the text where Time New Roman is used as suggested by SE).

Figure 9 – I really like this figure, it explains nicely the features that you observe in your samples and describes the processes that have taken place. You also show an example of a saddle reef pore, but is there a 'real life' example that you could show in figure 7?

→ We inserted a label in Fig.6 to show a 'real life' example of a saddle-reef-pore.