

Response to Referee no. 2

Dear Referee no. 2,

we would like to thank you for your very constructive and detailed assessment of our manuscript. We further appreciate the time and effort that you and referee no. 1 have dedicated to providing your valuable feedback on our manuscript. We are able to include most of your suggested changes which will be highlighted in the revised manuscript.

Please find below our detailed responses to your individual comments.

(Reviewer comment; Author's reply)

The paper by Schwichtenberg et al describes a set of 3 long-term compaction experiments on pure NaCl, a layered sample of pure NaCl and a mixed NaCl/biotite layer, and a layered sample of pure NaCl, mixed NaCl/biotite and pure NaCl. It addresses the question of the role of biotite in pressure solution creep, which is a process relevant to the understanding of deformation processes in the Earth crust. It is not exactly clear how this paper differs in approach and conclusions from earlier work done by Macente et al in 2017 and 2018. The paper concludes that with the type of biotite used, the earlier indicated reinforcing effect of phyllosilicates on pressure solution creep has not been found. The methods and assumptions are valid, and results are probably sufficient to support interpretations and conclusions, provided the two major comments are fixed. Otherwise, the organization of the paper and details of the manuscript are mostly of appropriately high quality, though some edits (see specific and technical comments) are needed to fix what is currently not clear.

Apart from the apparent similarity to Macente et al 2017 and 2018, I have two major comments concerning the potential validity of this study.

Major comment 1 is related to the technical capacity of the DVC. How well can automatic processing, such as DVC cope, with material literally moving, or jumping, from one place to another? it is written for small amounts of lateral deformation and shape change of particles, so if material moves from one place to another, which the 2D analyses indicate, is DVC then capable of picking it up? The main part of the argument in paragraph 4.3.3 seems to be based on the fact that the code ran and indicated no massive problems, and therefore the answers are correct. This is not necessarily the case. A smaller part of the argument is that the interiors of the grains don't change. But what if new grains are created with a similar shape and size? And what if grains are completely dissolved? In the latter case, a correlation can be made with the neighboring NaCl grain, which looks otherwise quite similar, due to similar initial grain size.

Major comment 1 contains several arguments to which we will respond separately in the following paragraph:

- 1. How well can automatic processing, such as DVC cope, with material literally moving, or jumping, from one place to another? it is written for small amounts of lateral deformation and shape change of particles, so if material moves from one place to another, which the 2D analyses indicate, is DVC then capable of picking it up?*

The DVC analysis was conducted with SPAM which uses a linear homogeneous transformation function but no higher order shape functions. Hence, it can pick up displacements, rotations, zoom and shearing. The reviewer is correct that the change of shape may cause problems. However, in the present case the dissolution and precipitation process occurs along the grain boundaries while the grain centres are not affected by

deformation. As the texture of the sample is preserved throughout the experiment SPAM correlates very well.

In addition to that, we are looking at very small incremental amounts of deformation as we are comparing two successive time steps with each other. We agree that if we would try to compare the first and last scan of each experiment, we may encounter problems due to too much deformation. We have tried that in the past and it did not correlate well. But for the small deformation steps between successive compaction stages SPAM correlates very well.

- 2. A smaller part of the argument is that the interiors of the grains don't change. But what if new grains are created with a similar shape and size? And what if grains are completely dissolved? In the latter case, a correlation can be made with the neighboring NaCl grain, which looks otherwise quite similar, due to similar initial grain size.*

We agree with the reviewer and think this is a valid point. However, we are monitoring the experiments in 4D and any nucleation or complete dissolution of grains would have been visible. In a failed experiment we actually did observe dissolution which was rather easy to spot. New grains on the other hand can only nucleate in the open pore space, therefore will never reach both, same size and shape as the old grains.

As for major comment 1 we will address the individual arguments of major comment 2 separately in the following paragraph:

The second major comment is related to the starting porosity, a critical element for compaction experiments, and a notoriously difficult one to control. The initial compaction was 9 to 18%, but the starting porosity of the samples is quite different (Figure 12). In the mixed samples this porosity is not homogeneously distributed. Since pressure solution is heavily affected by porosity, how does this affect the rates and results you indicate? and on this note, the term steady state compaction is misleading, since the compaction rate should continuously decrease (see references in the manuscript). It is also not entirely clear how porosity is determined: is this like Macente et al from a 400^3 voxel subvolume in the CT scan? If so, include in the method section. Is the determination of the 2D porosity and 2D presence of NaCl per slice, but for the full sample, and for the 3D volumetrics on subvolumes only?

- 1. The second major comment is related to the starting porosity, a critical element for compaction experiments, and a notoriously difficult one to control. The initial compaction was 9 to 18%, but the starting porosity of the samples is quite different (Figure 12). In the mixed samples this porosity is not homogeneously distributed. Since pressure solution is heavily affected by porosity, how does this affect the rates and results you indicate?*

A higher initial porosity compared to the pure NaCl sample was observed for the SBS sample (~3%) and could account for higher stresses at grain contacts hence, higher strain rates. However, the initial porosity of the SB sample was lower than the one of the pure NaCl sample, and yet the strain rate was accelerated. We expect other factors such as the effective load to influence the strain rate as well.

- 2. and on this note, the term steady state compaction is misleading, since the compaction rate should continuously decrease (see references in the manuscript).*

We agree with the reviewer and will change “steady state” to “apparent steady state” throughout the revised manuscript.

3. *It is also not entirely clear how porosity is determined: is this like Macente et al from a 400³ voxel subvolume in the CT scan? If so, include in the method section. Is the determination of the 2D porosity and 2D presence of NaCl per slice, but for the full sample, and for the 3D volumetrics on subvolumes only?*

We determined the porosity as 2D porosity per slice of the sample. In case of the salt distribution we used two different approaches. The first one was equivalent to the 2D porosity measurement (per slice of the whole sample) and the second approach determined 3D volumes from subvolumes. We will clarify that in the revised manuscript.

Specific comments:

Line 15: this is the only place where the length scale is actually quantified, whereas it would make sense to include it in the discussion paragraph 4.1.

Thank you for pointing this out. In the revised manuscript this will be added to the discussion. There, we already mention that the diffusive transport occurs on length scales of multiple grain diameters. A true quantification, however, is difficult as we cannot trace the dissolved material from source to sink, we can only identify the source layer in contrast to the sink layer.

Line 73: please add a clarification on the different aspect ratio of the biotite flakes. Which dimension is 200-500 microns?

Line 73 will be updated. The dimension of 200-500 μm is the grain size of the biotite grains, hence the maximum diameter of the grains.

Line 76: dry NaCl?

We will clarify this in the revised manuscript. For the pure NaCl experiment we used dry NaCl straight from the container of the chem. compound. In contrast to the NaCl used in the preparation of the layered samples, the “dry NaCl” was not mixed with brine into a slurry.

Line 80: simple insertion of the piston, or already with a specific applied force?

As suggested by the reviewer, we will add details about the process in the revised manuscript. The piston was inserted into the sample cell by twisting. The cells were then flushed with pressurized brine. To avoid that the piston was pushed out of the cell by the fluid, a load was applied to the top piston, that kept the piston in place but was low enough so that the effective load on the sample remained zero. The load was calculated for the individual setups of different samples.

Line 86-91: out of curiosity, why is there a difference between SBS and SB samples in the design of the pumping system? Is there a different brine used? Or is it just one of those things that happens when experiments progress?

We had to switch to a different pumping system as the glass column of the initial transfer vessel broke during the preparation of the second experimental suite. Further, it was reasonable to operate the system at a lower pore fluid pressure because we aimed to raise the effective stress in individual samples.

Line 92: what was the fluid pressure? Was this the same for all three experiments?

The fluid pressure was 5 bar in the SBS experiment and 2 bar in the SB and S2 experiment. In order to make this information accessible in the revised manuscript we will add a summarizing table to the supplementary material.

Line 98: why is there a difference between the constant effective load for SBS (6.64 MPa) for SB + S1 (10.5 MPa)? What is the load during the experiments? Please add here.

We will implement the change as suggested by the reviewer. The effective load of each sample remained constant throughout the experiments. The conditions for the first experimental suite (SBS) were chosen to be similar to the ones used by Macente et al. (2018) which allowed comparison of the data with each other. Afterwards the effective load was increased for the second experimental suite (SB and S2) in order to increase the strain rate of the deformation process according to the rate law for diffusion controlled DPC (Spiers et al. 2003).

Line 142: is for this type of microtomograph the gray scale belonging to 100% NaCl density always the same, regardless of scanning conditions? Because in some CT scanners the grey signal "floats", and in some scanners it is fixed. How is that for this scanner?

In our system the grey value "floats". To minimize this effect, we chose constant scanning conditions for each scan of a sample. That means we used a constant peak energy, and target power loading as well as exposure time and source-camera distance. Hence all time steps were scanned under constant illumination. Further, we used the same reconstruction parameters for each scan of a series. Hence, the grey signal for an individual phase should in theory be the similar for the scans of a series. Minor differences only affect the segmentation process as classifiers may need to be adjusted between the individual scans of a sample.

Line 155-157: I do not understand the size of the 3rd dimension for the 3D NaCl subvolume.

We agree with the reviewer that this needs clarification, and we will do so in the revised manuscript. We picked a biotite grain at the top and one at the bottom of the biotite-bearing layer which were easy to identify in every compaction step. The distance between those two grains defined the 3rd dimension of the NaCl subvolume and decreased with increasing compaction/ progress of deformation.

Line 176: How do SPAM and TomoWarp deal with grains which change shape themselves? They do not only rotate and rearrange but can also change shape due to dissolution and precipitation (major comment 1).

SPAM uses a linear homogeneous transformation function but no higher order shape functions. Hence, it can pick up displacements, rotations, zoom and shearing. The reviewer is correct that the change of shape may cause problems. However, in the present case the dissolution and precipitation process occurs along the grain boundaries while the grain centres are not affected by deformation. As the texture of the sample is preserved throughout the experiment SPAM correlates very well.

TomoWarp on the other hand is based on displacements measured by SPAM and therefore not affected by the change of shape as long as SPAM correlates.

Line 186-187: all samples were under a constant and similar effective vertical load during this compaction time? This doesn't become clear from the preceding sections. What is the starting porosity of the sample? Is it homogeneous throughout the sample? Does each sample have the same starting porosity? (major comment 2)

We agree with the reviewer and we will include the missing information in the revised manuscript as a summarising table in the supplementary material (vertical loads) and in the results section (starting porosity) of the manuscript.

The effective load was constant for the entire duration of the experiments but varied between the individual samples.

S2 : 10.5 MPa; SB : 10.5 MPa; SBS : 6.64 MPa

The starting bulk porosities (at t=1hr) of the samples are not the same but they are similar to each other.

S2: 27%; SB : 24.3% (Bt), 25.3% (NaCl); SBS : 30.8% (NaCl-top), 30.8% (Bt), 33.0% (NaCl-bottom)

The biotite-bearing layers have a lower initial porosity than the pure NaCl layers which we explain with a higher packing density of bt-grains in combination with NaCl-grains.

Figure 3 and line 198-206: why the smooth connection between datapoints in Figure 3a? What is the highest resolution in vertical strain rate you can obtain with your measurement method? The fact that a plateau is reached can also mean you have reached the measurement capacity of the setup. In principle, in a pressure solution type of process, based on theory (citations in the manuscript), one would expect a continuously decrease in strain rate with porosity. In other words, it is a steady state in the length of the experiment, but if you could measure indefinitely, the rate would continue to decrease. So is it really a 2 stage process, or is it actually a visual artefact caused by measurement resolution and experiment duration?

The smooth connection between the data points is a spline interpolation. The reason for choosing an interpolation rather than connecting the data points with each other is that we do not have measurements in between data points. Although we expect the compaction to follow the depicted trend, we cannot exclude positive or negative deviations.

The highest possible resolution of the strain rate is a shortening of one slice over the entire duration of the experiment. That is a strain rate of $8.16e^{-11} \text{ s}^{-1}$ for SBS, $8.97e^{-11} \text{ s}^{-1}$ for SB and $1.33e^{-10} \text{ s}^{-1}$ for S2. Hence, the minimum strain rate reached in the experiments is still orders of magnitudes higher than the resolution of the measurement.

We agree that the term steady state might not be appropriate for the data, a better term would be apparent steady state. We will correct this in the revised manuscript accordingly and replace “steady state” by “apparent steady state”.

The two stages of the process are rather related to a transition from a loosely packed aggregate where mechanical compaction significantly contributes to the strain rate, to an interlocked aggregate dominated by chemical compaction.

Line 225/Figure 7: as Figure 3 and line 198-206: is it caused by steady state or measurement resolution?

Here, again the maximum resolution of the measurement is defined by a minimum displacement of one slice/pixel per time interval. As discussed for the strain rate in the comment above, the z-displacement rates plotted in Fig. 7 exceed the resolution of the measurement by orders of magnitude.

Figure 7: this is z-displacement rate. In the NaCl-biotite-NaCl sample both NaCl layers have a different thickness than the mixed layer, where in the NaCl-biotite sample they are of similar thickness. If you would plot strain rate instead of z-displacement rate, would the trend then change?

Plotting the bulk strain rate gives a similar result as the DVC analysis. Both strain rates are similar to each other.

Figure 8-9-10: why did you not do the DVC for all time steps? How certain are you that the time steps shown are representative?

We did the DVC for all time steps and selected the three data sets shown in figures 8-10 after thorough inspection of the results.

Line 229-245: please be more precise in your description, and in labelling if you are looking at compactive or dilative strain maximum in this paragraph. In Figure 8 (SBS), I see deviatoric strain maxima in the center of the sample, correlating with positive volumetric strain (dilatation), and overall more activity in the bottom half of the sample. In Figure 9 (SB) I see similar high deviatoric strain in the center, but more activity in the top half of the sample. There is barely any dilatation. In Figure 10 (S1), there are high deviatoric strains in the center, and both dilation and compaction, with more activity in the bottom half of the sample. Moreover, what would be the minimum strain needed to be measurable? The samples overall do look blue, but how blue does it need to be to be sufficiently away from zero?

We will thoroughly revise the section and state more precisely where strain maxima occur within the samples and how they correlate with each other. In addition to that, we will identify the minimum strain for each sample.

Table 2: in all three figures, there are three plots for the DVC, but only two data entries for each sample in this table.

As suggested by the reviewer we will add the third value to the table.

Line 236: I would consider the use of the word “trend” with only two data-points per sample too strong.

We agree that indeed two values do not define a good trend.

Line 243: “deviatoric strain maxima corresponded to the location of biotite grains as well as open pore space and pure NaCl clusters” – in other words, there is no correlation between the location of the deviatoric strain maxima?

Yes, that is correct.

Line 247: the correlation is not absolute: the maximum loss of porosity in the SB sample (1932 hr) is from slice 500-925 or so, and the biotite layer ends at slice 1000. For the SBS sample, the maximum loss (1932 hr) is from slice 800 to slice 1550, and the biotite layer is from slice 750 to 1350. How does the location of the maxima compare to the data from the DVC?

The biotite layers have curved boundaries rather than straight ones, so pure NaCl measurements influence the porosity measurements as well. That is the reason why the limits of the maximum porosity loss do not match exactly the boundaries of the biotite-layer. In order to compare the location of porosity maxima to the DVC we suggest to plot the porosity distribution on top of the DVC results and add the figure to the revised manuscript.

Figure 12: the starting porosity is quite different for the samples. How would this affect the average compaction curves of Figure 3?

The porosities measured in the very first scan after 1 hour of compaction are ~30% (SBS), ~25% (SB) and ~27% (S2). A higher initial porosity compared to the pure NaCl sample as observed for the NaCl-biotite-NaCl sample (~3%), could account for higher stresses at grain contacts and therefore higher strain rates. However, the initial porosity of the NaCl-biotite sample was lower than the one of the pure NaCl sample, and yet its strain rate was accelerated.

Line 254-259: how did you determine the NaCl distribution? 100% minus porosity minus biotite? Or did you also segment the NaCl grains individually? What is part of the NaCl remains in solution as supersaturation, as indicated in the discussion as a potential part of the process?

NaCl was discretely segmented using the Deep Learning tool of Dragonfly as “simple” segmentation by Trainable Weka was not possible.

We can only speculate about the amount of NaCl that remains in solution.

Please see the following calculation for an exemplary estimate:

We had a 5mm OD x 10 mm column of NaCl with 25% porosity, and that porosity was filled with saturated brine at room T then we have:

Volume of column = 196 mm²; Volume of NaCl = 147 mm²; Volume of brine = 49 mm²

Concentration of saturated NaCl brine = 6.15 M

Moles of NaCl in brine = 0.30 millimoles; Moles of solid NaCl = 7 millimoles

% of NaCl in the column present in brine = 4.2%

Desarnaud et al (2014) and Zimmerman et al (2015) indicate maximum supersaturation of 1.6x. Hence, the maximum additional salt in solution through supersaturation would be an additional 2.5% of the total mass of the solid NaCl if all of the solution were at the supersaturation limit for homogenous nucleation.

That calculation sets a maximum upper limit as we’re not dealing with homogenous nucleation. The reality would likely be much closer to the limit set by the saturation, and thus well within the likely segmentation error of the NaCl segmentation.

Line 260-264: Unclear phrasing: if the assumption is made that biotite is an insoluble internal standard (line 261), it makes sense that the analyses show the biotite content to be standard... And can you show somewhere in a Figure where the subvolume is taken (this would also solve line 155-157)?

We assume that the reviewer means “constant” instead of “standard”. Line 261 will be rephrased accordingly. In addition, we will add a map for orientation in the supplementary material of the revised manuscript.

Line 273-275: it is not clear to me why this is interpreted a change in deformation mode, instead of it being a continuous log-linear decrease in rate (same comment as in the description of the results).

Please see response to the comment concerning figure 3. We agree that this should not be interpreted as a change in deformation mode and will adjust the revised manuscript accordingly.

Line 278: This needs more careful phrasing, since even the current description of results indicates that strain maxima occurred mainly within the biotite part of the sample (line 233).

In line 232-233 we write “strain maxima in the biotite-bearing samples were located within the biotite-bearing layer, but not exclusively. Pure NaCl domains were also affected by high

strains...” While we appreciate the reviewer’s feedback, we respectfully disagree. We do not think it gives the impression that strain maxima were mainly located in the biotite-bearing layer. In the revised manuscript however, we will rephrase it so that it becomes even clearer that strain maxima occurred in the biotite-bearing domain as well as in the pure NaCl domains. Line 278 will be adjusted accordingly.

Line 294: unless one takes it that the patterns of Fig 8, 9 and 10 do show there is more strain localization in the biotite... Or that the DVC actually doesn't cope very well with the material transport (major comment 1).

Thank you for pointing this out. We agree that locally higher strains might occur in the biotite-bearing layer. However, the bulk magnitude of strain is not higher. Further, the correlation fields in the supplementary material show that DVC correlates very well even in chemically changing samples.

Line 329: This wasn't clear to me in the results on the DVC, though the concentration of deformation was mentioned in Figure 12 and 13. Perhaps it would help to add arrows or boundaries to Figs 8-9?

We agree that Figs. 8 -9 do not display the location of shear strain on the grain scale clearly enough. In order to better visualize the correlation, we will implement the reviewer’s suggestion and highlight biotite-NaCl phase boundaries in the figures.

Line 333: I do not understand how figure 5 demonstrates the efficiency of this process

In figure 5 you can see a single NaCl grain which is in contact with two biotite grains. With progressing deformation, you can see that the NaCl grain is reduced in size without showing any signs of brittle deformation. We interpret this to happen due to pressure solution along the interphase boundaries between the NaCl and biotite grains. We will adjust the figure caption in the revised manuscript to clarify the case.

Line 334: ah, that's what the Lambert plots did (technical comment line 180)! But if there is no significant rotation, then why is the deviatoric strain so high in the biotite layers? Another reason could be that many of them are already fairly horizontal, so that might also be why there is no strong realignment.

We agree with the reviewer that right from the beginning many biotite-grains are already horizontally aligned, however if a biotite grain is point loaded on one side and is not subject to effective dissolution one would expect that the mechanical compaction of the aggregate causes rotation of the biotite.

The deviatoric strain can result from e.g. grain boundary sliding during DPC. Rotation is not the only source that can cause deviatoric strain. Also, grain boundary sliding should cease in a denser aggregate, which we observe as deviatoric strain rates are decreasing over time.

Line 345: can you add here that Macente reported a first order effect (i.e. why would you expect a first order effect), and which observations showed there is no first order effect?

Unfortunately, Macente (2017) and Macente et al. (2018) did not analyse the biotite composition in their study and therefore, we cannot add as requested by the reviewer, that they reported a first order effect.

The analysis of the chemical composition of the two types of biotite shows that although the compositions are different, our results are comparable to Macente et al. (2018). Both studies show a similar effect of biotite upon the porosity. In theory a different chemical composition can affect the dissolution process by either enhancing or impeding it. This was not investigated in our study, and consequently no effect could be observed.

Line 367: why/how does Figure 11 show that local maxima correspond to sites of precipitation?

The volumetric strain maxima correspond to the NaCl-pore interface which is located in between e.g. the biotite grains. We interpret the NaCl-pore interface as active site of precipitation. We suggest the addition of a plot of the porosity on top of the volumetric strain pattern to clarify the correlation.

Technical comments

Line 62: “which are described in Macente (2017)”: Since the description is actually below, this phrasing is slightly misleading

We agree with the reviewer. Line 62 will be rephrased to “...oedometer cells (Fig. 1). A detailed description of the cell design can be found in Macente 2017.”

Line 105: for clarity, it would be nice to add if the samples were compacting in the same building (I assume so), or if they were transported by car throughout Edinburgh or the UK or even from France (looking at the affiliations of the authors). Given the composition of the author team I imagine the transport between CT scans and compaction location was done carefully, but the explicit mention of the location of the tomography instrument somehow gives the impression that the scans were done somewhere far, far away... Which would have consequences for their validity.

We agree that this has a potential effect on the study. Significant transportation of the cells for each scan would have had consequences for the study, which is why we didn't do the experiments and scans in separate locations.

Line 106-107: how many scans and compaction time for the S1 sample?

Thank you for pointing this out. We will add the parameters for the pure NaCl sample in the revised manuscript. We took a total of 5 scans over a duration of 1089 hours.

Section 2.5: this section would be easier to read if there was a flow diagram that briefly labels all the steps and different softwares

We think this is an excellent suggestion. We will add a flow diagram in the revised manuscript.

Line 136: please mention your figures in order of appearance. Fig 12 now follows Fig 2. Fig. 12 doesn't contain the error, though that is suggested by this part of the text. Idem for Fig 13 and Fig 14

We appreciate the reviewer's feedback on the order of figures. We respectfully disagree with a change of this order as the text in line 136 (methods section) refers to a figure in the result section in order to give a visual example of the plotting method. Hence, we cannot avoid that Fig. 12 follows Fig. 2.

We also agree that according to the text errors should be plotted in Figs. 12, 13 and 14. We will add these in the revised manuscript.

*Line 159: given the name (digital *volume* correlation) I assume this approach is only valid for the 3D volumes, correct? Please add.*

The reviewer is correct, DVC is valid for 3D volumes in contrast to DIC for the 2D case. We will add this accordingly.

Line 160: can you indicate in 1-2 lines which operations or calculations are performed by SPAM and which by TomoWarp2?

We agree with the reviewer that this would help the accessibility of the method therefore, we will add this to the revised manuscript. SPAM calculates the displacement field, while TomoWarp uses the displacement field as an input to calculate the strain field.

Line 180: this is my own ignorance: how does one read a Lambert projection? As the reader, what would it tell me? Can you add a reference here so the non-knowledgeable reader can read up on the importance of these plots?

Thank you for pointing this out. A Lambert projection is a conic map projection. Here, the projection sets a cone over a sphere and projects the surface conformally onto the cone. The cone is unrolled, and the parallel that was touching the sphere is assigned unit scale. (From Wikipedia)

In our case the parallel that was touching the sphere represents the equator of the sphere. Similar to a stereonet, the Lambert projections used in this paper show the upper half of a sphere. The center point of the Lambert projection is the pole of our sphere. Similar to a stereonet, the outline of the Lambert projection represents the orientation on a plane ("dip direction") while the circles represent the "dip".

Reading the Lambert projection is similar to reading a stereonet for poles.
We agree that a reference should be added to the revised manuscript.

Figures 4 and 5: why did you choose this specific vertical slice? Where is it located in the 3D sample? Would we see the same if you choose any other slice?

We chose the middle slice of each sample because it was the easiest to identify in every compaction step. However, other vertical slices show similar patterns and could have been used equally.

We suggest adding a sketch of the location of the slice in the sample to fig. 4 as it is the same for all three samples.

Figure 5: can you add the red markers to all 5 panels? It would help guide the eye. The lower biotite grain seems to also change curvature between the panels, or is that simply due to the unfocused visualization?

We appreciate the reviewer's feedback and will add them to all grains. The curvature of the biotite grain can be either the result of unfocused visualization or a cutting effect, after all the system is reacting and we cannot guarantee 100% to always find the exact same slice through the grain.

Figure 5: Why do you not have panels also to show if similar things happen in the SB and SI samples?

The same effect can be observed in the SB sample and we can add such a panel to the revised manuscript and also one of the pure NaCl sample for comparison as the process is less pronounced there. We decided against doing so in the original manuscript as it would not add indispensable information to the manuscript.

Figure 6: to my non-Lambert-trained eye, figures a and b look very similar... Why could you measure so much more grains for a versus b? Is that because there were more grains in b to keep the layers of equal thickness?

We agree with the reviewer, the figures are indeed very similar. Which shows that biotite grains in both samples are already horizontally aligned at early stages of the experiments and do not rotate much.

The reason why we were able to measure more grains in b than in a is that we always included 20wt% biotite in our biotite-bearing layer. The layer is thicker in the SB than in the SBS sample hence, it contains more biotite grains.

Line 229-234: For readability, please treat the descriptions in the same order as the figures are shown for clarity, and in Figure 8 9 and 10 please add the sample name in the caption or in the figure. This could be improved throughout the paper: sometimes the pure salt sample is described first, and sometimes the salt-biotite-salt sample.

We agree that the reviewer's suggestion can improve the readability of the manuscript. We will rearrange figures and text passages to a consistent order which will be used throughout the revised manuscript.

Figure 8 9 10: compaction in rock mechanics experiments is often denoted positive, whereas here the negative values are compaction (line 234/second-last line of caption).

We appreciate the reviewer's assessment. The reason for this switched notation is that SPAM has its origin in engineering rather than rock mechanics. We agree it can be confusing when coming from a rock mechanics background, however, to be able to compare published DVC results with each other which to our knowledge all use the engineering definition, we would like to keep compaction negative.

Figure 9: typo: "cumulative"

We will change that.

Paragraph 4.1: the title of the paragraph, combined with the question of the introduction, gives the reader the impression the length scale will be quantified, whereas this is actually a more qualitative interpretation.

We will rephrase the title of the subsection 4.1 in order to clarify that the length scales of diffusive transport during DPC are not quantified in this manuscript.

Line 374 – 388: OK, but how can you then be sure for the rest of your sample that the values are correct? You probably can I'm sure, but I don't see it straight away. What am I missing?

After thorough revision of the section we decided to remove it in the revised manuscript. The presented mathematical derivation of the isotropic strain is based on the small strain theory while we used the large strain theory for the strain determination in our DVC analysis. While it does not explain the positive volumetric strain in the glass beads layers (SBS sample, first increment), it does however explain why we chose large strain over small strain as the basis for our strain measurements.

Given the length of appendix A2 and how crucial the terms are, I suggest to move this definition to the method section.

We will include the definition of strain as used throughout the manuscript in the methods section of the revised manuscript.