11 Aug 2023

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by Sarah Buchmann

Notification to the authors:
1-In the title page of the manuscript, the symbol you used for Christopher J.L. Wilson, "†", is only used when an author is deceased and not in another case. Please remove this cross symbol from the manuscript.

- This has been removed from the manuscript

2-Please ensure that the colour schemes used in your maps and charts allow readers with colour vision deficiencies to correctly interpret your findings. Please check your figures using the Coblis – Color Blindness Simulator (https://www.color-blindness.com/coblis-color-blindness-simulator/) and revise the colour schemes accordingly.

- We have tested the figures with the "color Blindness simulator" and all record normal. In fact, before submitting the manuscript we recoloured many of the original primary images so they had distinct colour-safe combinations and clearly illustrate the features described in the text.

26 Aug 2023
Editor decision: Reconsider after major revisions (further review by editor and referees)
by Kaitlin Keegan
Public justification (visible to the public if the article is accepted and published):
Thank you for your submission of a revised manuscript. Please take all comments raised by the referees seriously when revising your manuscript. Specifically:
Please include the information you've provided to Referee #1your Authors' Response to RC1 regarding the liquid water content of the samples in the manuscript to help the general audience of The Cryosphere interpret the results.

- Both RC1 and RC2 raise the question of melt content in the samples. We have now calculated the melt content for each of the deformed samples and this is listed in a new table (Table 2). The change in water content has now also been described in the caption of Fig. 1 and Table 2 and referred to in two places in the text.

In addition to the sentence added to the Methods section (~L 106), please include a description of the errors associated with the microstructural and grain size data measured by the fabric analyzer.

- The error regarding grain size, grain number of correctly indexed grains is ±5.7% and this is now stated in the revised manuscript with an additional reference added, namely, Hammes and Peternell (2016). The errors with any CPO measurement is <1º, and has been pointed out in our other papers and this is definitely not critical to current paper.

- In order to reduce repetition portion of the original results section has been deleted and is in part included in the Methods section.

Adding general descriptions of the methods used from cited papers would be very helpful for the general audience of The Cryosphere that may not be familiar with the details of this field.

- Changes have been made to methods section including further clarification of the segmentation step and what is obtained from the reconstructed tomographic images. An additional two references (Chauhan et al. 2016 and Thomson et al.) have been added to the manuscript, as these papers will provide Cryosphere readers with clear descriptions of the processes and complimentary alternatives required to be undertaken during the extraction of data from tomographic images during the segmentation process.
- To avoid repetition portion of results section has been deleted and a few sentences have been included in Methods section.

Line 182 still confusing to me.

- This has now been rewritten as :-
  "However, this phase, Mix-3, is distributed as narrow rims abutting the D_2O matrix grains (Fig. 1d-e). Whereas, larger areas of Mix-2 and Mix-3 occur at the outer rims of the melt regions (Fig. 2a-c)."

Please address all of referee #2's comments in a more thorough manner.

- A revised response is below with additional comments in blue.

Due to the large difference in opinion between the two referees, we will seek a third review of your manuscript.

- We are very disappointed in this decision as we have now answered every one of the referee's concerns. A number of other minor changes have been undertaken to the text (e.g. deleting repletion between the methods and results section) and three additional references have been added.

Revised Response to R2: 'Comment on egusphere-2023-70', Anonymous Referee #2, 12 Jul 2023

This paper deals with the effect of melt on the rheology and permeability of ice sheets and glaciers ices. The authors make use of ice specimens made of a mixture of deuterium and water ices. This is a very interesting idea as both have different melting

point, and neutron scattering can distinguish between both allowing neutron tomography to be carried out.

The topic of the paper is relevant and very interesting, however I am asking for a rejection of this contribution as the authors largely over-interpret the experimental results that are shown. Many times in the paper, the figures do not provide any support to the text and interpretation, and therefore, after completing the reading, I am not convinced at all by the robustness of the results. Sometimes, the text is completely disconnected with the figures (experimental evidences) provided.

We completely reject the above statement. This reviewer has failed to question any of our scientific aims, techniques, arguments or the manner of presentation. All comments have been carefully evaluated and we have ended up making minor additional changes to the text in response to RC2, as highlighted in our response below.

- The paper severely lacks of a quantitative analysis. For example, the sample porosity and its evolution with strain, which has a central position in the paper, is never given quantitatively, although it can be estimated from neutron tomography. Porosity might affect the effective behaviour even more than the melt content (the melt content is also never quantify !). ex. line 193 state an 'increase in porosity', but no evaluation is provided.
- Numerous quantitative steps have been necessary to process all the data sets, even before the results were presented e.g. Visualisations (e.g. Figs 1-3); Pore densities (Fig. 4), Textural changes (Fig. 5), Stress-strain relationships (Fig. 6), Microstructures (Fig. 7); Grain numbers (Fig. 9). Further changes to Methods section and additional references highlighting techniques that were used during the segmentation process. These involve the use of attenuation coefficients to provide the quantitative and qualitative results presented in the above figures. Table 2 and changes to caption of Fig. 1 point out the percentage of water and HDO mixes in the samples.
- As for porosity and its influence on strain, well this is illustrated in Fig. 8b, and discussed in the text.
- It is not clear to me how the author can distinguish between water ice and melt water with tomography… Is this possible ? How much of the specimen really melts during the experiment ?
- This is clearly pointed out in the manuscript in sections 2.3 and 3.1. The manuscript points out how this was undertaken with appropriate references to the processes that we have used, especially during segmentation e.g. Kahn, et al., 2012; Wang et al. 2015; Andrew, 2018). This could not have been undertaken unless there were different attenuation coefficients for H2O (2.4 cm-1) and D2O (0.35 cm-1) as pointed out on line 135-136 in the original manuscript.
- The amount of melt in any portion of the sample is difficult to define, because of the degree of proton exchange between deuterium and hydrogen, and depends on the where strain is localised. However, we have now been able to re-access the server at the University of Mainz where segmentation was undertaken. As a result we now provide a table (Table 2) of the total volume of melt and mixed phases in the deformed samples. A reference to Table 2 has now been incorporated in a number of places in the manuscript and in the Fig. 1 caption.
- Line 179 is a typical example of over-interpretation of the results, occurring too many times in the paper : "The frozen-in melt-enriched regions or segregations predominantly

occupy conjugate shear bands (Fig. 1)". First of all, the strain field has not been measured (eg. with DIC or DVC) so that I don't understand how the author can decide whether a specific feature is a shear band or not. Second, the ellipse show 'concentration of Mix-2', not really aligned at 35° as indicated; this could be due to the sample preparation, or simply due to some random process, etc… this really needs to check and quantify further.

We strongly disagree with the above statement. The shear bands can be clearly identified and located during the segmentation process and this is based on the identification of the different mixes. If we had not undertaken the segmentation, then we would not have identified their location. There is no way they are due to preparation processes. In fact, areas that contain the shear bands will be progressively rotated as the sample is progressively shortened. This is why there are variations in angles as pointed out in the Discussion section of this manuscript and in modifications we have undertaken to Fig 10b-d.

- Legend fig 1, at point A and B it is said that there is an increase in porosity, but one do not see anything in the figure and no quantitative estimation is provided !
    - It is impossible to provide a quantative estimation on the 2D surface. However, with careful examination and our evaluation of all representative slices we stand by our statement. It was impossible to present all the data even as supplementary material, but are available from the AAD Data centre (a comment to this effect has been added to the acknowledgements).
    - Figure 4 and the new data in Table 2 certainly provided quantitative values.
- Line 185 'low pressure regions or non-deforming region' : pressure and deformation field have not been measured, so how can the authors estimated that some regions do not deform, and that some are at lower pressure ? One even don't know whether the porosity is open or closed (althrougth this might be accessible by tomography).
- Anyone who has worked with unconfined samples during deformation recognises that the outer surface of the sample has to be a 'low pressure region'. We have not inserted strain gauges in samples to measure localised stresses. Therefore, we stand by our text. Similarly, the greater majority of voids within the polycrystalline sample have to be closed.
- Line 203 : "This bimodal distribution into shear and compaction bands are all part of a connected network". No proof for bimodal distribution, nor shear band / compaction band, nor proof of a connected porosity network in the data …
- The word 'bimodal" has been removed.
- Line 205 "increasd porosity" but the porosity is not quantified…

We completely disagree with this comment. Quantification is clearly shown in Fig. 4. We have now also added extra wording to the text clarifying our qualitative observation of how Fig. 3e relates to Fig. 3d. The wording has been changed to:-

For instance, this is identified in the edge close to the indenter, as in areas A–D in Fig. 3e. If this is compared with the porosity distribution in the YZ slice (Fig. 3d) a marked increase in the volume of pores is noted.

- Line 223 "(fig 2d). the network consists of pores situated on grain boundaries" => grain structure (and thus grain boundaries) are not indicated in figure 2…

- We disagree with the referee here as they can be clearly identified in area highlighted as melt segregation.
- Line 231 why should quartz (not deforming by basal glide) should be a good analog for ice (deforming mostly by basal glide) ?
- We are not elaborating here as this is clearly explained in Kronenberg et al. (2020) but we have added in text “references therein” as this paper references all the classic papers on this subject.
- Line 251 I find really strange that the stress increases as the temperature is increased up to +2°C, as part of the specimen melts and ice should become softer. What is the reason for this stress increase ?
- A sentence has been added to text to explain this, namely:
- “This modest increase in stress we attribute to an expansion of the aluminium piston as the temperature is increased.”
- Line 252-255 “This stress drop we attribute to the softening of the ice with the onset of melting, grain boundary migration and initiation of the deformation bands. This ductile to shear transition can be explained by the competition between different time scales corresponding to the relatively slow melting of the H2O ice, and broken bonds as the HDO mixes were generated” => not clear at all. We agree with the first part of this comment and feel there is no need to elaborate.

  Has gbm been observed (not shown in the figures) ? Grain boundary migration is definitely observed and shown in Figures 7 and 8. In addition on lines 274-275 in the original manuscript, the grain size from ~0.5 to 3-5 mm is described.

- initiation of deformation bands => strain localization into deformation bands starts at the very beginning of the deformation (before 1% strain), see the paper of Grennerat et al. Acta Mater 2012. What is a ‘ductile to shear transition’ ? shear deformation is not in the ductile regime ? ’different time scales’ => could you explain what is meant here ? ‘broken bonds’ => do you mean atomic bonds ?? any evidences ?
- Unfortunately, we do not have evidence to support the reviewer’s statement that shear bands initiate at 1% strain. We definitely disagree with the referee here – The shear and compression bands observed in our experiments are definitely ductile features. We have added the word “atomic” to the text of the manuscript
- .Line256-260 : in fig 6, one do not see as written in the text, for LDH-20, a transition from hardening to weakening within the -7°C regime. And one do not see, for LDH-35, a decrease of the flow stress at +2°C (even modest)
- Thanks for comment. We have added “after” to clarify this. In addition, we have added a sentence to explain why there was a subtle increase in stress.
- I really don’t understand paragraph 278-284 and what are the supporting informations. One do not see ‘white lines’ in fig 8c. What do you mean with ‘refraction of shear bands’ (line 280) ??

  Thank for the comment “the white lines” should have in fact read “black lines” and has been changed in the caption. The word “refraction’ is quite clear in its meaning that there is a change in direction, e.g. as a light ray passes through an object or as a refracted cleavage in rocks.

- Line 287 what is meant with ‘evaluated through statistical analysis of particular angular positions and hkl reflections’ ?? I guess this is related with the neutron diffraction experiment, but this is really not clear
- In the original text, a reference was made to Wilson et al. (2019) which describes the novel method used. This is based on acquiring crystallographic data during the in situ deformation experiments As pointed out in the methods section (line 103) this technique was described in Wilson et al. (2019, 2020). To overcome the referee’s concern the sentence has been modified to:
- “….was evaluated through statistical analysis of particular angular positions and *hkl* crystallographic reflections collected during the in situ deformation (Wilson et al., 2019; 2020).
- Line 294 : one do see, as stated in the text, ‘a strain dependent increase in grain nucleation up to 14.6% strain’ for both DH29, LDH20, and DHC06 (grain size clearly decreases before this strain level). Same for ‘a variable but decreasing number of grains in the final deformation stage (15.4 – 20% strain)’, not observed for dhc06, dh29, dhc23…
- Yes, we agree with this statement. However, to overcome the referees concern we have added “…of new recrystallised grains”
- Line 310 ‘as these experiments have shown, the movement of meltwater’ As far I understand, the meltwater movement has not been observed here. Only few static tomographic images have been acquired

  We thank the referee for this comment. The wording here has been modified to:

  “Moreover, as these experiments have shown, the diffusion of hydrogen, which is a tracer for the location of meltwater, through conjugate shear zone formation or basal compaction, contributes significantly to overall meltwater transport.”

- Line 315 ‘shear induced failure mode’ => do you have observed any cracks in the specimen (not shown/discussed in the figures) ? why invoking suddenly failure modes ?

  There are definitely no cracks in any of the samples and this is why it is not mentioned in any figure. The deformation is purely plastic and confined to the ductile and recrystallised deformation bands as illustrated in Figures 7d. and 8. We could have included numerous other e.g. of such microstructures, but because of brevity we have restricted the number of images. Any material scientist would acknowledge these deformation bands represent one form of a failure mode in a deforming material. Our wording here is completely appropriate.

- I find really weird that the word “recrystallization” does not appear even once in such a paper dealing with microstructure evolution at the melting temperature…

  In many places in the text we describe grain nucleation, grain boundary migration and diffusion processes which create the mixed grains. These all contribute to the vague term recrystallisation and its connotations. Because of the resolution of the tomographic images we are definitely not getting into the debate on whether the microstructure is evolving via sub-grain nucleation vs specific new grain nucleation. For the referees benefit we have added the word recrystallisation in a couple of places in the revised text.

- Etc…