Review of Llorens et al. The Cryosphere - *MS No.: tc-2021-224*

Can changes in ice-sheet flow be inferred from crystallographic preferred orientations?

Overall, I find this paper is presenting some oversimplified steady-state models for the evolution of CPOs in ice, and their total applicability to ice-sheet flow has to be questioned. Both thermal conditions, strain cycling and strain localization, particularly within the middle sections of polar ice sheets are far from steady state. This poses a real challenge for interpreting CPOs and is a problem that is not mentioned in this paper. In this contribution the authors are avoiding and fail to point out these issues. Only if significant modifications are undertaken, then this paper will make a suitable contribution for readers of 'The Crysophere'.

The first two parts of this contribution (sections 1-2 & Abstract) presents a very poorly described introduction to a set of numerical models to simulate CPO evolution during ice flow. In these sections there are very significant problems with the construction and poor referencing to previous works, which are relevant to the focus of this paper. It is quite obvious that none of the 9 other co-authors with a good command of the English language have edited or read the first part of this manuscript.

It would be a benefit to the reader if there was some reorganization of the text. It would be good if sections 3.2.1 to 3.2.4 were taken out of the 'Methods' section and combined with the appropriate sub-sections in 4 'CPO evolution results'. This would provide a clearer pathway into what the modelling is trying to achieve.

I also feel that there is an over citation of papers, which have little value to the main thrust of this manuscript and should be deleted. Or are constantly cited for little reason and just disrupts the text; this is particularly so in the introduction and discussion.

In its present state, portions of this paper need some major rewriting and possible rearrangements. I feel the authors could modify their figures to reduce the magnitude of strains displayed, as these strains are not what is encountered in the majority of natural ice streams because the strain is competing with recrystallization and basal processes.

Title page & authorship:

The 'Title page" shows 10 contributing authors. It appears Llorens may have been the sole author for parts of this paper and that this version of the manuscript has not been thoroughly scrutinized by all her co-workers.

In fact, this co-authorship issue should accord with the Vancouver protocol: (http://www.icmje.org/recommendations/browse/roles-and-responsibilities/defining-the-role-of-authors-and-contributors.html). An author must have
- Made substantial contributions to the conception or design of the work; or the acquisition, analysis, or interpretation of data for the work; AND
- Been involved in drafting the work or revising it critically for important intellectual content; AND
- Provided final approval of the version to be published; AND
- Agreed to be accountable for all aspects of the work in ensuring that questions related to the accuracy or integrity of any part of the work are appropriately investigated and resolved.

Hence if these co-workers have not had input into this version of the paper (the AND), then many of these contributors should be listed in the acknowledgements and not cited as authors.

In the Title, some of the wording (e.g., **changes in ice-sheet flow)** seems inappropriate as the manuscript is more about **strain regimes**. Do the authors want to change the title?

Abstract:

This is terribly written and has absolutely no appeal for a reader. The authors don't tell us the type of CPOs developed and fail to explain the changes in the CPOs. They say they are looking at "influence of ice deformation history" during the creep of ice. Well there are other changes in the deformation history that are identified by Peternell & Wilson (2016), processes which are not even mentioned in the current paper. The Abstract needs to be restructured.

Introduction:

The first sentence is quite irrelevant to this paper and is essentially the same as Llorens et al. (2016). Again, the following sentence does not reflect the real focus of the paper. The following description does not highlight a problem that will be tackled in this manuscript. Instead it is an incredibly poor introduction to the activation of slip systems in ice and relationship to CPOs. Also get rid of all references to sea level changes – they are not relevant to this paper (e.g., Nerem, Golledge). Other references e.g. Katayama & Karato; and non-scientific articles such as Alley & Joughin should be deleted.

Line 74: It is inappropriate to say "Durham et al., 1983 and many others". Here you should properly cite previous experimental studies – "not many others". I should point out Wilson & Peternell (2012) did describe experiments with a pre-existing CPO.

Lines 71-80: This is an extremely poorly written and referenced section on previous ice experiments. The reference to Gao & Jacka should be deleted as this was a poorly designed set of experiments that go to very low strains. There are many more applicable and recent experiments that should have been referenced see Jacka & Li (2000), Wilson et al. (2020) + Fan et. al (2021) and references therein.

Lines 84-100: Again, a poorly referenced section on numerical models applicable to ice that does not elaborate on the different types of models. There are small strain models such as Wilson & Zhang (1994) and references therein, and othe higher strain model studies that may be worth citing.

Line 89-90: Surely, we don't need all the self-referencing to the Llorens papers and the Piazolo et al. 2019 paper as these are more about recrystallization microstructures than CPO development. These references are really not necessary here or elsewhere in the following text.

2. Flow regime:

I have an issue with this section as I do not believe this text or Figure 1 have been constrained by a clear description and appropriate references to what happens in a longitudinal section through an ice stream. I would suggest the authors look at the paper of Donoghue and Jacka (2009) and references therein. With a proper literature search the authors will find there are also other papers o longitudinal strain and shear stresses in ice sheets that need to be consulted. Also, Z is normally not vertical.

Line 111: Why not state at the onset what your different examples are?

There is no clear description of the type of CPOs identified in these different regimes. Is it possible to summarise what is found in nature?

Line 136: the references to Yong, LeDoux, Lutz are not needed here as they are cited again on line 253.

Lines 137-139: should be in figure caption.

Figure 1: This is a very much oversimplified diagram, it is probably worth the authors reading Donoghue and Jacka's (2009) description of the changes along a flow line. We all know there are localized zones of high shear strains in any ice stream (e.g. Thwaites et al. (1984) as such can this diagram be made more realistic. The caption lacks any description of what XYZ stand for and what are the broad arrows on the boxes stand for? I don't follow the changes from regime II to III? I can't see a vertical shear plane in regime IV? Why is Z vertical? We normally consider vertical flattening at the upper levels of ice sheets with the ice deforming mainly by compression along the vertical direction.

3. Methods:

Lines 140-153: Could this be shortened as there is major overlap with Llorens et al. (2016a)?

Line 170: Up to now there has been no discussion of three slip systems.

Line 195: Remove {brackets} and replace with (0001). This comment applies to figures 2 to 6. I see that in other papers by Llorens et al. that there is also a use of {} for (0001). Crystallographers use these {} brackets where there are multiple axes and not the unique 0001 axis, hence I would suggest using (). The only justification of using {} would be if you are describing a collection of diffraction reflections (001) + (002) + (003).

Table 1; Where does the strain rate come from in caption? There is no discussion of this in the text.

Table 2: The caption is extremely poor and needs to be expanded. A series of ice cores and examples are provided in table, but there is no documentation or reference as to who have described these examples. Also ($\varepsilon 1=0.92$) is specified but there is no explanation as to what $\varepsilon 1$ is here or in previous text. In fact, the authors should justify why such high strains are used for an average ice stream.

The different modelling scenarios discussed in section 3.2 are well written and are good summaries. However, it would be better if they were linked to the description of the results.

4. CPO evolution results:

Why not take sections 3.2.1 to 3.2.4 out of the 'Methods' section and combined with the appropriate sub-sections here? It would improve the readability of the manuscript.

Figures 3, 4, 5: Again, in this figure and caption why is (0001) put in brackets? On Line 277: "y(red)"? It is not clear how the red associated with '*y*'

The results in these sections are well presented and it is obvious that some authors other than Llorens may have input into writing these sections. I then ask myself, who was not involved and is there justification for them being co-authors on this manuscript?

My major concern here is all the models have been run to very high strains which are really quite unrealistic for the majority of ice streams where transported ice (with a pre-existing

CPO) is piggy-backed on basal ice that is undergoing extensive recrystallization and is in a higher stressed environment.

5. Discussion:

The introduction to this section is poorly written and is very biased as to which literature the authors quote regarding previous experimental and numerical simulations. There are constant strain rate ice experiments described by Peternell & Wilson (2016; e.g.Fig.3) up to 20% shortening or 0.2 natural strain, which clearly replicate the pattern produced in the PGR diagrams described in this paper. A comparison to such experiments could be made.

The authors need to remove FSE, FSA & ISA from text and figures (e.g. Fig. 7). In fact, it may incorrect to say FSA are finite stretching axes, then why is Z a shortening axis? The reference to Passchier (1990) is not warranted as such axes were described well before him.

Lines 390-405: Can I suggest you also consult Wilson et al. (2020) as there are aspects in this paper and references therein that have been overlooked by the current authors. I feel this whole section on comparison to the CPOs in natural ice needs a lot of rewriting.

Line 405: Delete "plausible scenarios". They are not plausible as you don't consider the processes of strain cycling as described in Peternell & Wilson (2016) and the effect of temperature.

Line 410: Delete "(i.e., long…FSE)" also delete FSE on lines 433, 435, 462, 463 etc

Line 418: Do you need to have all these references?

Line 458: What is "it" – correct English.

Overall, I find this discussion section is very biased and the authors should have some statements as to the complication observed in nature and why their transitions in CPOs are difficult to establish in natural ice masses.

Figure 8:

The caption here needs further expansion – it is not clear what this diagram is really telling us. Why not remove some of the text (lies 440-450) and put this in caption. What does the vertical dashed line represent?

Figure 9:

This is an oversimplified conceptual diagram lacking a lot of detail. It should better show where the areas pure shear vs simple shear, indicate any temperature gradient, hence areas of annealing and zones of higher shear stress. The caption is far too brief and there is no clear indication of what figure B stands for, nor is this referred to it in the text?

Overall this discussion section is very disappointing as these results really need to be compare to real scenarios, e.g. Donoghue and Jacka's (2009). In addition, limitations to the application of the models should also be highlighted.

6. Conclusions:

Lines 477-479 point 1: “imposed deformation” This is incorrect as deformation conditions include temperature, strain cycling etc. I would delete this whole statement as it won’t be a “quick” process and is not a conclusion coming from these models.

Line 485: Surely it is even lower than this value? See my earlier comments.

Line 493, Point 4: This should be deleted as there are many other complications.

References

Line 507: Alley & Joughlin should be pp 551-552. However, it is a very generalised article. If you need a reference, then I suggest the instead use of: Alley, R. B.: Flow-law hypotheses for ice-sheet modeling, J.Glaciol., 38, 245–256, 1992.

There are a large number of papers that should be deleted and other papers that the authors may also consider:

Donoghue, S. and Jacka, T.H. (2009) The stress pattern within the Law Dome Summit to Cape Folger Ice Flow line, inferred from measurements of crystal fabrics. In Hondoh T. ed. *Physics of ice core records II*. Hokkaido University Press, Sapporo, 125-135.

Fan, S., Prior, D. J., Cross, A. J., Goldsby, D. L., Hager, T. F., Negrini, M., & Qi, C. (2021). Using grain boundary irregularity to quantify dynamic recrystallization in ice. *Acta Materialia*, *209*, 116810. https://doi.org/10.1016/j.actamat.2021.116810

Jacka TH and Li J (2000) Flow rates and crystal orientation fabrics in compression of polycrystalline ice at low temperatures and stresses. In Hondoh T ed. *Physics of ice core records*. Hokkaido University Press, Sapporo, 83–102.

Peternell, M., Wilson, C.J.L., 2016. Effect of strain rate cycling on microstructures and crystallographic preferred orientation during high-temperature creep. *Geology* **44**, 279-282.

Thwaites RJ, Wilson CJL and McCray AP (1984) Relationship between bore hole closure and crystal fabrics in Antarctic ice core from Cape Folger. J. Glaciol., 30(105), 171–179.

Wilson, C.J.L., Peternell, M., Hunter, N.J.R., Luzin, V., 2020. Deformation of polycrystalline D_2O ice: Its sensitivity to temperature and strain-rate as an analogue for terrestrial ice. *Earth and Planet. Sci. Lett,* https://doi.org/10.1016/j.epsl.2019

Wilson, C.J.L., Peternell, M.A. (2012). Ice deformed in compression and simple shear: control of temperature and initial fabric. *Journal of Glaciology* 58, 11-22.

Wilson, C.J.L. and Zhang, Y., 1994. Comparison between experiment and computer modelling of plane strain simple shear ice deformation. *J. Glaciol.*, 40 (134), 46-55.

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