On behalf of the author team, I would like to thank the Editor, Nanna Bjørnholt Karlsson, for handling the review process of our manuscript. We thank the reviewers for their suggestions and comments, which have certainly helped to improve the manuscript. We have applied the changes in the manuscript and replied to the questions raised by the reviewers below. Replies to the reviewer are provided in green font and the new or modified text in the manuscript appears in green italic font.

Best regards,

Maria-Gema Llorens

Reviewer #2:

All the reviewer's suggestions for improvement have been considered and addressed. We have carried out significant modifications based on the issues raised. According to the journal's policy, we have not addressed those comments questioning authorship or asking for native speaker reviewing of the language.

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.

We have included more references on previous works, including the references suggested by the reviewer: Wilson et al., 1994; Wilson et al., 2012; Peternell and Wilson, 2016; Wilson et al., 2020.

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.

According to this, and a similar suggestion by reviewer #3, qualitative descriptions in 3.2.1 – 3.2.4 appear now at the beginning of the corresponding parts of the results section.

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.

On the one hand, the reviewer considers that some references in the introduction are overcited, and we should delete references. But on the other hand, this comment contradicts the previous request "*In these sections (introduction) there are poor referencing to previous work*". Now, with all the Wilson references included in the text, we hope that this issue is satisfied.

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.

As explained before, we found that dynamic recrystallization (DRX) does not have a first-order effect on CPO. We found that the deformation geometry dominates CPO evolution. Strain is a different entity than DRX. DRX cannot compete with strain, but only with the effect of strain on the microstructure or CPO. According to the suggestion by reviewer #1, the manuscript now includes a new section "6. *Model limitations*", where the influence of *DRX* in our model is discussed (see reply to reviewer #1 question)

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.

This is an offensive comment, and we are not replying to it. This has been discussed with the editor.

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.

This is an offensive comment, and we are not replying to it. This has been discussed with the editor.

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?

Many thanks for this suggestion. We agree that the title of the manuscript should be changed. We consider that *deformation* is more appropriated than *strain*, because strain is a reference to the symmetric part of the deformation. And in this work, we also perform simulations in simple shear conditions, which implies vorticity or asymmetric part of the tensor.

According to this, we have modified it to *"Can changes in deformation regimes be inferred from crystallographic preferred orientations in polar ice?"*

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.

Peternell and Wilson discussed the effect of strain rate variations, or non-steady state deformation, on the microstructure by the observed *DRX* processes. Accordingly, this is not a crucial paper regarding the approach used in our manuscript. We do not feel that the abstract was that terrible or should be totally restructured because of a previous publication by the reviewer, which only marginally relevant to our steady-state conditions and without *DRX*. However, in the revised version of the manuscript we now mention this paper as previous work related to the overall topic exposed here.

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.

The study of ice rheology has the ultimate aim of understanding ice-sheet flow, and thus its discharge to the ocean and impact on sea level rise. We consider that this aspect has to be pointed out in the paper in the general introduction because it has a direct implication for societal challenges. This is currently just mentioned in two sentences, including two general references (which only represent 9% of the introduction).

On the other hand, the activation of slip systems and CPO's, that the referee thinks that it is poorly explained, takes 39 lines (lines 45-84), which represents the 74% of the introduction.

We agree that Alley and Joughin reference was not appropriate, and we have replaced it to Alley (1998) following the reviewer's recommendation.

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.

See the reply to the following question.

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.

We have modified the references in this paragraph according to this and the previous suggestion, including all the references by the reviewer: *"Experimental studies have utilised ice to understand how CPOs develop and evolve under deformation (see Kamb et al., 1972; Wilson, 1982; Jacka and Macagnan, 1984, Wilson and Peternell, 2012; Budd et al., 2013; Montagnat et al., 2015; Vaughan et al., 2017, Fan et al., 2020). Most studies by far use bulk isotropic ice (i.e., with a random CPO) that is then subjected to a single deformation event. Due to the limitations of laboratory deformation experiments, to our knowledge only a few studies have used polar ice samples starting with a pre-existing CPO. Moreover, most of such studies focused on vertical uniaxial compression of samples with a pre-existing CPO that was formed by vertical compression (resulting in a c-axis cluster or cone) (Azuma and Higashi, 1984; Dahl-Jensen et al., 1997, Castelnau et al., 1998, Jacka and Li, 2000, Fan et al., 2020).*

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 other higher strain model studies that may be worth citing.

We have included more references of numerical modelling of ice according to this comment. However, we think that we should here cite only references to the state of the art on numerical modelling, so we will not include Wilson and Zhang (1994). The paragraph now includes Montagnat et al. (2011) and Llorens et al. (2017, 2020).

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.

Llorens et al., papers focus both recrystallisation microstructures and CPO development. However, we have moved these references to the previous paragraph (see reply above), but we have kept Montagnat et al. (2014b) and Piazolo et al. (2019) because we consider them two relevant review papers referring to numerical modelling of polycrystalline ice.

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.

Z is now shown as the vertical axis.
See comments below.

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

Line 111 has been moved to the end of the paragraph, and the description of the considered deformation regimes is now at the beginning of the paragraph.

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?

We have included in this section the CPO description from natural observation at different regimes in this chapter 2:

- *"Observations from ice core natural samples at these domains, range from a vertical single maximum to a vertical girdle (Montagnat et al., 2012; Weikusat et al., 2017)"*
- *"In depth, observations from ice cores indicate a vertical single maximum."*
- *"Inside a glacier, ice stream or in a flank flow (Voigt, 2017), flow acceleration may dominate, resulting in extension along the flow direction (zone III in Fig. 1a), observed by a vertical girdle in ice core samples (Voigth, 2017)"*

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

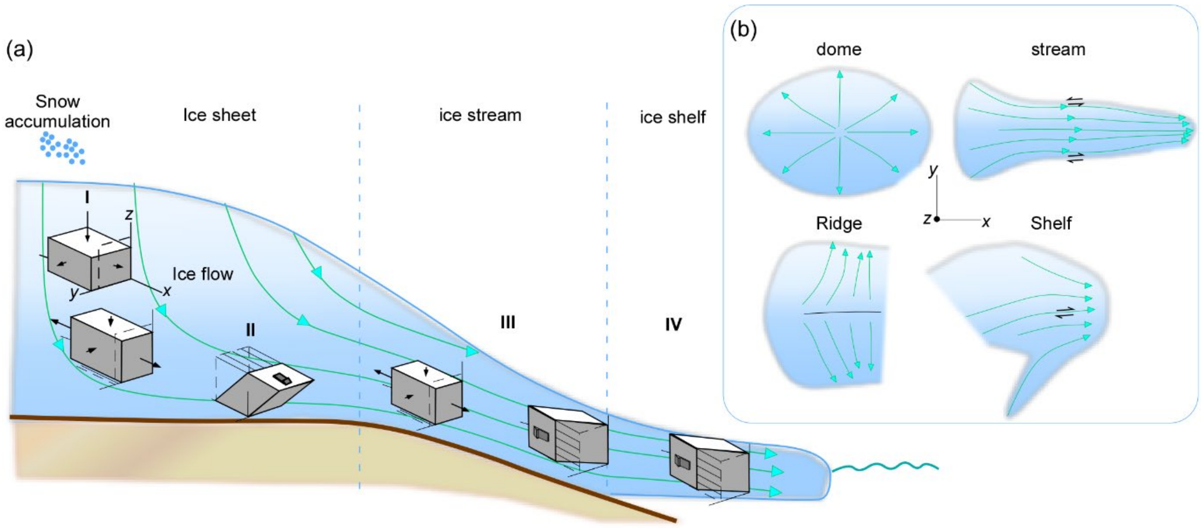
Removed.

Lines 137-139: should be in figure caption.

Moved to 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.

In the revised version, the figure caption now includes the description of the xyz coordinates. We have modified the sketch of regime IV to indicate the vertical shear plane. The *z*-coordinate is now the vertical. This is a simplified diagram, as explained in the text. In order to transfer simulations conditions, we think we need these simplifications here. Of course, we agree, that more complex scenarios can occur in nature, e.g. in small ice caps, but this goes beyond the scope of this contribution. However, according to this comment and also from reviewer #3, we have now more clearly stated in the text that the applied velocity gradient fields applied are assumptions.



3. Methods:

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

We have shortened this paragraph by removing the following sentence: "*ELLE has been successfully used for the simulation of a variety of studies of rock microstructure evolution during deformation and metamorphism (see Piazolo et al., 2019).*"

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

We believe this line to be the appropriate place to explain the slip systems considered in our simulations.

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).

Point taken. We have corrected the notation in the whole manuscript.

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

We have removed the strain rate from the figure caption, as now the approximately time to destroy a fabric is calculated and included in the text, assuming the natural strain rates for every ice-sheet domain (see reply to reviewer #1).

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.

The caption has now been extended including the references to the ice cores: *"**Table 2**. Deformation regimes applied to the different series of numerical simulations, including idealised deformation regimes in drill cores and examples. References of ice core* *description: GRIP (Thorsteinsson et al., 1997), GISP2 (Gow et al., 1997), Dome C (Durand et al., 2007), Dome F (Azuma et al., 1999), Talos Dome (Montagnat et al., 2012), NorthGrip (Faria et al., 2014); EDML (Weikusat et al., 2017), NEEM (Montagnat et al., 2014a), Vostok (Lipenkov et al., 1998)"*

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.

In our simulations, at ε_I=0.92, the microstructure develops an approximately 80% of end-member CPO (P or G). We consider that it fits with reaching the "secondary creep" quasi steady state in deformation tests at ca. 1% (Bud & Jacka, Treverrow).

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.

According to this, and a similar suggestion by reviewer #3, we have merged sections 3 and 4. Qualitative descriptions in 3.2.1 – 3.2.4 appear now at the beginning of the corresponding parts of the results section.

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.

According to this comment, and a similar comment by reviewer #3, we have extended the description of the methods in section 3 and moved sections from 3.2.1 to 3.2.4 to section 4. Results. See reply above.

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'*

Brackets are corrected in the whole manuscript (see reply above). It is a mistake; it should indicate *black* instead of *red* in the text.

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?

This is a very inappropriate comment, and we are not replying to it following the editor's recommendation.

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.

This issue was already addressed in our answer to reviewer #1. Clearly, when a volume of ice is piggy-backed, it does not experience a change of flow regime, or at least not one that reaches any significant strain.

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.

We have included the reference to Peternell and Wilson in the discussion between experiments and simulation results.

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.

We do agree that these abbreviations may appear confusing to readers that are not used to this terminology. Therefore, these are now explicitly written throughout the text. It is customary in geology to denote the finite stretching axes, which are the principal axes of the finite strain ellipsoid by *X*, *Y*, and *Z*, from longest to shortest. The *Z*-axes (capital) is this the direction of maximum finite shortening, as is explained in the text. The *Z*-axis is not the same as the z-axis (lower case) of the coordinate system. By writing out the terms in full, we think there should be no confusion.

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.

Our simulations do not include DRX, as explained in several replies above (it is now explained in the manuscript in chapter 6. Model limitations). Wilson et al., 2020 address the influence of temperature and strain-rate (i.e. DRX) on ice deformation. However, we have included this reference in the text as a general reference for ice deformation experiments.

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.

We are not considering DRX or variations in strain rate, as Peternell and Wilson did. Addressing this issue, we have included a new section *6. Model limitations* (see reply to reviewer #1).

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

See reply above.

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

Yes, we do.

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

There is no “it” in line 458.

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.

The reviewer acknowledged in his comments that volumes of ice may experience changes in their flow regime, as investigated in this paper, and by others. The reviewer did not provide any arguments why transitions in CPO are difficult to establish in nature. We see no reason to address this comment.

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?

The figure caption has been expanded, including the explanation of the vertical dashed line: *“Evolution of the relative activities of basal, pyramidal and prismatic slip systems during deformation for all series presented, calculated from Equation (1). Transition of deformation regimes are marked with a vertical dashed line. In the A, B and D series, the second flow regime produces a prominent increase in basal slip, while the pyramidal slip system activity is reduced. However, in series C the basal activity remains constant, and the prismatic slip is increased”*

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?

It is irrelevant indicate the temperature gradient, as we are not considering temperature variations in our approach (*DRX* processes are not included in the models).

On the other hand, we disagree with the suggestion to modify this figure, because the reviewer finds it as an *oversimplification*. In agreement with reviewer #1 we consider that this figure does not need modifications. As reviewer #1 correctly interpreted, the purpose of this figure is to summarise the results of this work for a broad audience in a simple and understandable way. Reviewer #1 says: ***The figures are exceptionally well done (Figure 9 in particular could be used to teach good science communicatio***

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.

According to this suggestion, and a similar suggestion by reviewer #1, we have included the new section “*6. Model limitations”* (see reply to reviewer #1).

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.

We have changed “imposed deformation” to “imposed boundary conditions”.

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

This statement is based on the results of our study. We therefore do not think we need to modify the text here.

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

We find the suggestion by reviewer #3 more correct than this one by reviewer #2. We have followed the suggestion by reviewer #3, modifying this line in order to provide an answer to the question expressed in the title of the manuscript “*Can changes in deformation regimes be inferred from crystallographic preferred orientations in polar ice*?”. The line has been modified to: *“According to our results, CPOs are reliable indicators of the current flow conditions, as they usually adapt to them in a relatively short time. However, caution is warranted when a volume of ice may have experienced complex (multi-stage) deformation histories.”*

References:

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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.

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Wilson, C.J.L., Peternell, M., Hunter, N.J.R., Luzin, V., 2020. Deformation of polycrystalline D2O 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

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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.