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 #1:

Review of "Can changes in ice-sheet flow be inferred from crystallographic preferred orientations?" by Llorens et al.
This study uses a full-field model to examine the overprinting of ice-crystal fabrics when two strain regimes are experienced. The aim is to understand how/whether the fabric records the ice-flow history or if it is rather representative only of the in-situ deformation regime. Contrary to the title, I do not think it really evaluates "changes in ice-sheet flow," since simulations intentionally mimic idealized steady-state conditions. Nonetheless, this is an interesting topic that has received other attention in the last year, but this study uses a different approach that likely represents the fabric evolution more accurately. The paper is well organized and the writing is clear, with the exception of some unusual notation that created unnecessary confusion for me. The figures are exceptionally well done (Figure 9 in particular could be used to teach good science communication). The manuscript is relevant for *The Cryosphere* and it could be a valuable contribution once some important issues are addressed.

We are grateful for the reviewer's support and for the useful suggestions for improvement.

Specific comments:
1. I think a bit of consideration is needed surrounding what it means to "infer changes in ice-sheet flow," since the present manuscript does not actually address that question. Fundamentally, all simulations here are steady state; they follow idealized particle paths within a steady-state ice sheet, though of course those particle paths transit multiple strain regimes. Since we may be curious whether the fabric matches the in-situ conditions, the paper could simply be retitled to something like "Do crystallographic preferred orientations always represent in-situ conditions?" with the body essentially left as-is.

We have realised that the title and the use of "changes in ice-sheet flow" can be misinterpreted. The paper deals with changes in the flow kinematics (boundary conditions) that a volume of ice experiences through the ice sheet. We do not refer to a change of the flow of the ice sheet itself. Following the first reviewer's advice, we have now modified the article title to "*Can changes in deformation regimes be inferred from crystallographic preferred orientations in polar ice?*"

If the authors are instead set on addressing a question about flow changes (which is probably more interesting and relevant for a broader audience), then I think more simulations, as well as some explanation of what that would entail, is needed. **I would think some kind of change to flow is needed in the simulations to infer a flow change** (i.e. not a change along a particle path, but a change to the large scale flow through which the simulated parcel transits). For example, what does the model say about a transition from a dome to a ridge? How long/over what strain would such a change be evident in the CPO? How about formation of an ice stream? Along with those simulations, extensive evaluation like in line 440 would be warranted (i.e. do those changes manifest unambiguously in the CPO? what could we see in the CPO that allows inference of a flow change?).

The reviewer raises a number of interesting questions that could be addressed by our modelling approach. We, however, here indeed deal with the change along a particle path. This is actually still not well known at all, and needs to be addressed before looking at flow changes. Here we show how the CPO changes from one flow regime to another in four scenarios. From the modelling point of view, it actually does not matter if the change is because the ice-sheet flow changes, or because the material flows from one kinematic regime to another through a steady-state ice sheet. In both cases the material experiences a change, and this is what we analyse. Our simulations show that the amount of strain required to fully reset the CPO according to the new regime is not constant but depends on the starting CPO and the new steady-state CPO. It would indeed be of interest to investigate in the future the systematics of the amount of strain needed for a full transition. Even more challenging would be to investigate if it is possible to see if a CPO is not yet fully reset and can thus indicate a change in the flow conditions. We certainly intend to address such questions in the future, but modelling all these scenarios goes well beyond of the scope of the present contribution.

2. The results are hard to believe until dynamic recrystallization (DRX) is given more consideration. The two citations used to justify its exclusion are both modeling studies that in my view are outliers compared to the conventional wisdom on the effect of migration recrystallization on crystal fabric from ice cores (Faria et al., 2014a), experiments (Fan et al., 2021; Qi et al., 2019; Journaux et al., 2019), and modeling (Richards et al., 2021; Faria et al., 2014b). Migration recrystallization is often described as depending on the stress rather than the strain (Duval and Castelnau, 1995), and so may be particularly relevant for V2 and V4 (near the bottom of the ice sheet or in shear margins) where stresses are presumably high. Moreover, even if we were to assume that the effect of recrystallization were relatively small, why does excluding it better represent how CPO responds to a flow change (as implied by the current version of the manuscript)? This concern is intensified because this study shows that, under lattice rotation, development of the new fabric is strongly dependent on the previous fabric, so might a similar sensitivity apply to DRX? This issue is critical; if recrystallization changes the timescale/strain scale over which fabric persists, then a model of lattice rotation alone cannot accurately capture whether flow history can be inferred (or even whether the fabric matches in the in-situ stress and strain). I think this issue is sufficiently important that consideration of different mechanisms of DRX is needed (i.e. rotation and migration recrystallization). The large strains needed to overprint fabric seem to depend on the precise misorientation of the crystallographic axes relative to the new strain, and it seems plausible that even minor effects of rotation recrystallization could alter this misorientation and thus change the results, even if migration recrystallization does not lead to strong CPOs.

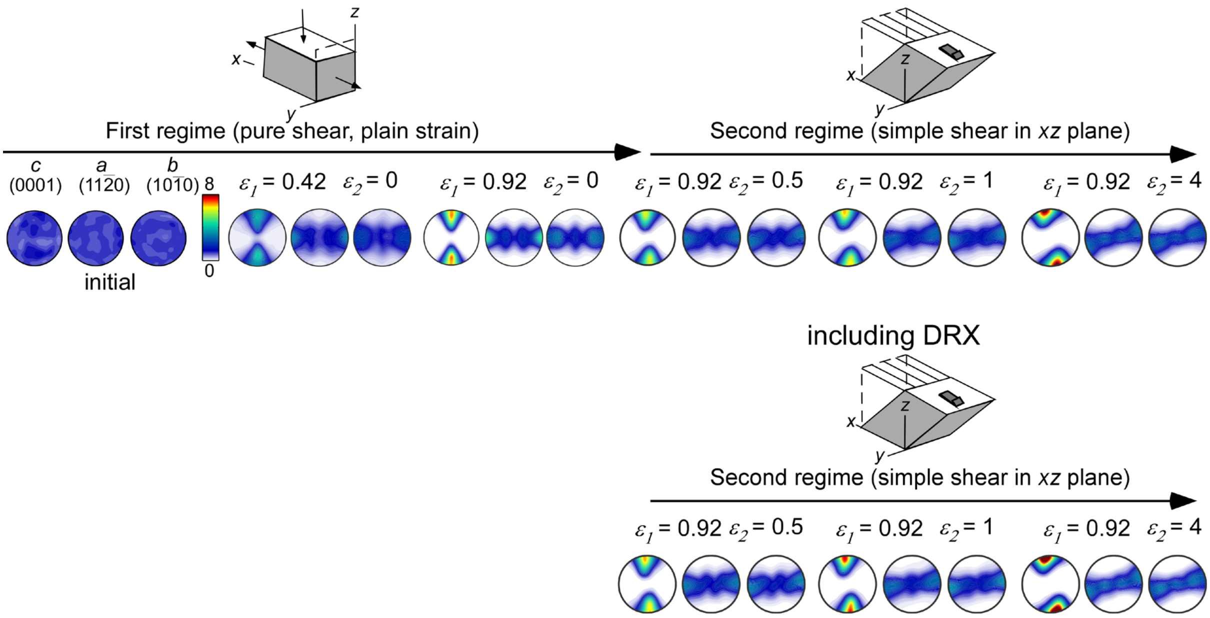
According to Fan et al., 2021, under high temperature and high stress conditions, strain weakening in ice is dominated by CPO development, where grain size reduction plays only a minor role.

We have carried out extensive work on simulating dynamic recrystallization processes, including grain boundary migration, recovery and polygonization, in ice and other minerals. As shown in our previous publications (*e.g.*, Llorens et al., 2016a, 2016b, 2017; Steinbach et al., 2016; Gomez-Rivas et al., 2017). These studies demonstrate that dynamic recrystallisation processes have a minor effect on the CPO development (orientation and strength). Therefore, the implementation of DRX in our model would not change the time scale during the development of a preferred crystallographic orientation significantly. However, recrystallization could reduce the time scale of fabric adjustment in case of (1) nucleation of new, strain-free grains, or (2) due grain boundary sliding causing rotation of small grains. In the first case the microstructure could be influenced by the orientation of these new crystals (Thorsteinsson et al., 2003), weakening the CPO. As shown in experiments (Fan et al., 2020), the activation of grain boundary sliding in the small grains also reduces the CPO strength.

To keep the system simple, we did not include spontaneous nucleation or grain boundary sliding in our model. Since, as pointed out by the reviewer, this issue is important, we now discuss it in a new section of "*6. Model limitations*", including a *supplementary figure 4* with the comparison between simulation with and without DRX:

"6. Model limitations

A number of studies that used the VPFFT-ELLE modelling approach (e.g., in Llorens et al., 2016a, 2016b and 2017; Steinbach et al., 2017 and Gomez-Rivas et al., 2017) have shown that dynamic recrystallisation processes, including grain boundary migration, recovery and polygonization, have a minor effect on the CPO development (orientation and strength). The implementation of DRX in our models does not change the time scale during the development of a preferred crystallographic orientation, as shown in supplementary figure 4, where series with and without DRX are compared. However, this may depend on the particular recrystallisation process, for example, (1) spontaneous nucleation (i.e., nucleation of new, strain-free grains), or (2) rotation of small grains due to grain boundary sliding. In the first case the microstructure would be influenced by the orientation of these new crystals (Thorsteinsson et al., 2003), weakening the CPO. As shown in experiments (Fan et al., 2020) activation of grain boundary sliding in the small grains also reduces the CPO strength. It however remains unclear to which extent these two recrystallisation processes play a role in the natural deformation of ice"



Supplementary figure 4.

3. I do not think that V3 is an accurate representation of a ridge. Almost by definition, a ridge experiences confined compression/extension rather than pure extension, so the deformation gradient at the ridge itself is

$$\nabla u = \begin{pmatrix} a & 0 & 0 \\ 0 & -a & 0 \\ 0 & 0 & 0 \end{pmatrix}$$

for some a. Of course, some areas can have flow convergence as ice leaves the ridge, in which case we have something like

$$\nabla u = \begin{pmatrix} a+b & 0 & 0 \\ 0 & -a & 0 \\ 0 & 0 & -b \end{pmatrix}$$

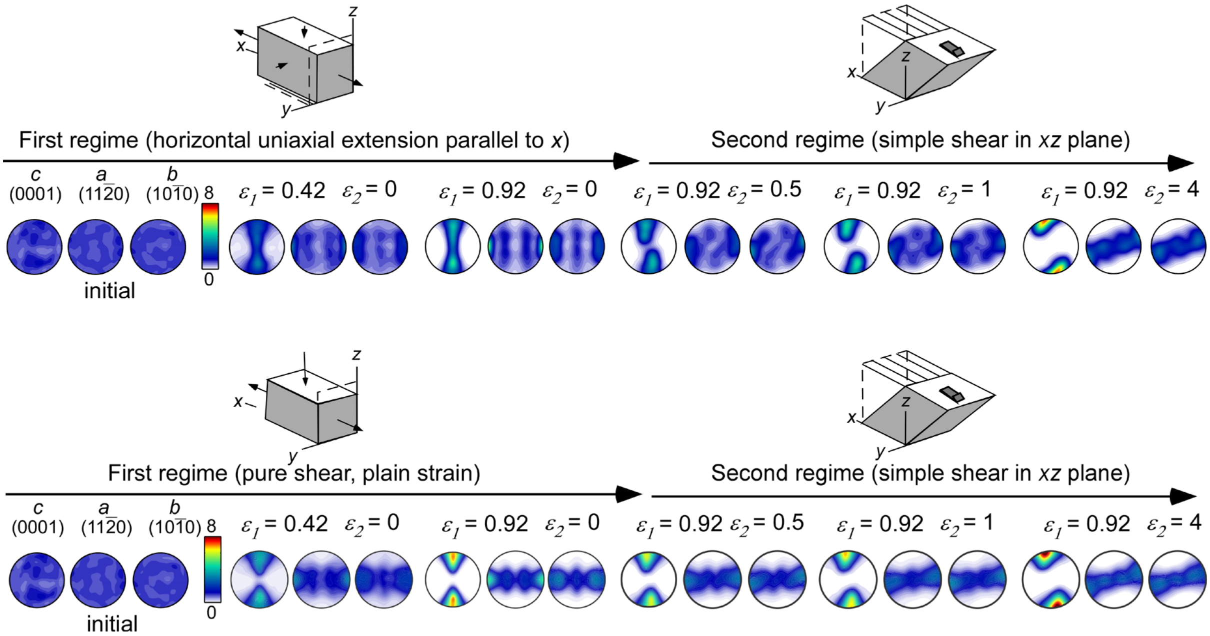
but to my knowledge $b < a/2$ in such areas; the same would be true for ice streams. The

$$\nabla u = \begin{pmatrix} a & 0 & 0 \\ 0 & -a/2 & 0 \\ 0 & 0 & -a/2 \end{pmatrix}$$

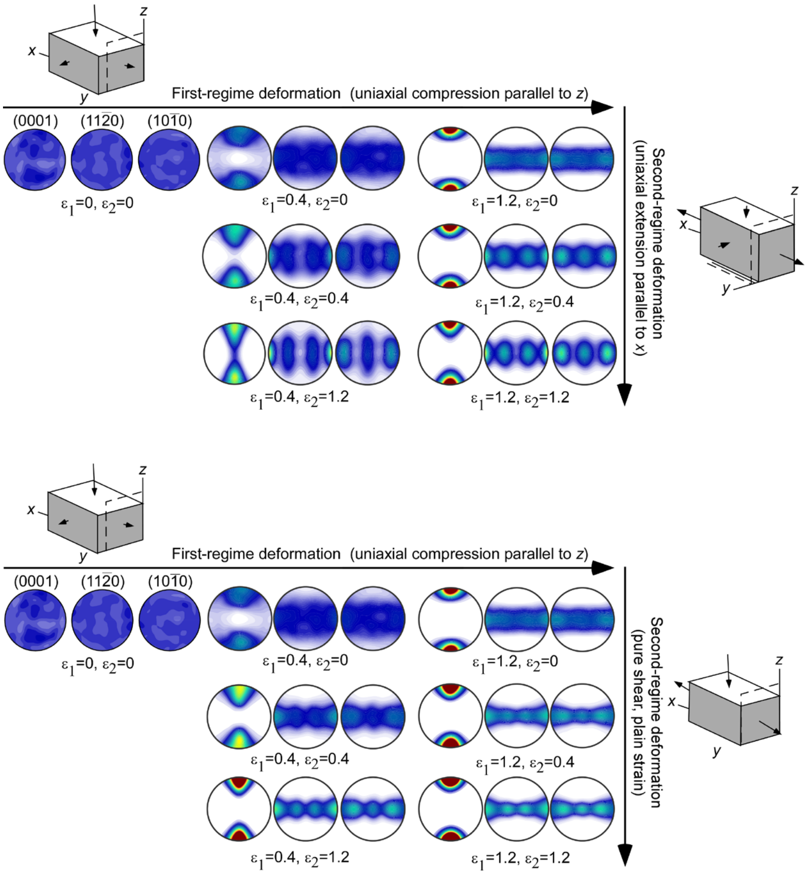
used in the manuscript will have a greater tendency to form a girdle since the extensional stress is equal in all directions in a vertical plane. Because this may affect the results, I would like to see series B, C, and D redone with more realistic conditions, or at least a sensitivity test with

$$\nabla u = \begin{pmatrix} a & 0 & 0 \\ 0 & -a & 0 \\ 0 & 0 & 0 \end{pmatrix}$$

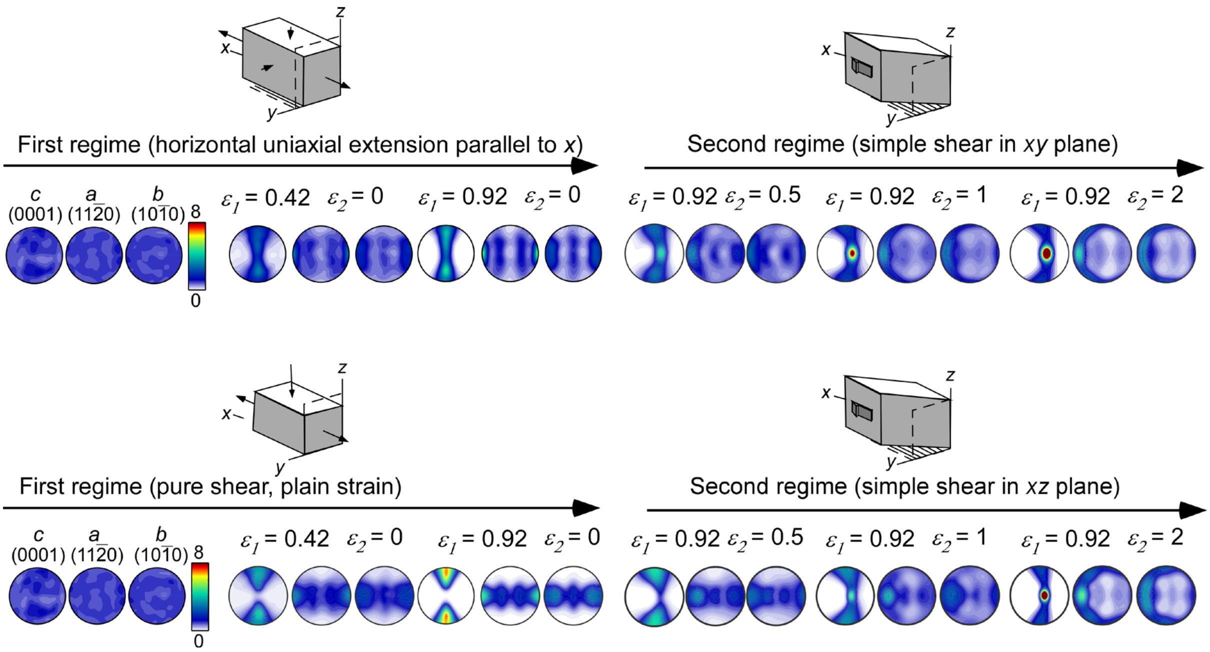
We have tested the B, C and D series, deformed by V'_3 in pure shear, as indicated by the referee (*supplementary figures 1, 2 and 3*). The description of the series is now indicated in the text, and the simulation results shown in the supplementary material compared with the current ones. This comment will be included in the text: *As observed in the supplementary figures1, 2 and 3, not so much influence on the final CPO is observed considering V'_3 or V'_3.*



Supplementary figure 1. Comparison between series B deformed by V3 (horizontal uniaxial extension) and V'3 (pure shear)



Supplementary figure 2. Comparison between series C deformed by V3 (horizontal uniaxial extension) and V'3 (pure shear)



Supplementary figure 3. Comparison between series D deformed by V_3 (horizontal uniaxial extension) and V'_3 (pure shear)

Along these lines, I am a bit skeptical of the total strains experienced with V4 as the second condition. Is there anywhere where ice spends long enough in a shear margin to reach these total strains? On a particle path, I would expect the particle to enter the ice stream or shelf before such high strains are reached. I do not see this as key to the results overall, so just a sentence mentioning whether it is realistic may be sufficient.

We assume that the reviewer is referring to our series D, in which a volume of ice experiences a change to simple-shear deformation, which can happen in the shear margin of an ice stream or in shelves.

Along flow shear zones are described in Drews (2015) for the Antarctic Koning Boudwijn (aka Roi Bouduoin) shelf. Here GPS velocities show a differ laterally by about 25 m/yr over a distance of ca. 500 m, giving a simple shear strain rate of 0.05 /yr. This is at a general velocity of about 300 m/yr and a shelf extent of ca 90 km, meaning the ice takes 90/0.3=300 years to reach the calving front. The total strain accumulated is then 300 x 0.05 = 15. We calculated that the shear strain in in the shear margin of the Northeast Greenland Ice Stream (NEGIS) near the location of the EGRIP drill site is between 15 and 20. Unfortunately, we cannot provide more details in a publicly available comment, as these data are still in review. However, we claim that a shear strain well above 10 in a volume of ice experiencing simple shear is very plausible. We did not change the manuscript text.

4. Although strain is the classical scale for fabric development, most glaciologists do not think in terms of total strain when working on problems other than fabric. To make the work more accessible, it would therefore be nice to give numbers as approximate timescales as well (I assume this is easy since the strain rates are known). It would also be nice to say something along the lines of “under realistic conditions, CPO can be preserved for XX years, and a flow change YY years ago could be detected.”

Taking natural strain rates for different scenarios, the preservation of the fabrics would be:

reference	conditions	strain rate	strain in our simulations	time (sec)	time (years)
Jougin et al., 2018	Upstream Ice stream	1.056E-11	2.4	2.27E+11	7201
Jougin et al., 2018	margins of NEGreenland ice stre	4.00E-10	2.4	6.00E+09	190
Bons et al., 2018	Base simple shear from dome	5.00E-09	6	1.20E+09	38
	Base simple shear from ridge	5.00E-09	8	1.60E+09	51
Weikusat et al., 2017	Base EDML	9.00E-11	8	8.89E+10	2817
Millstein et al., 2021	Ice shelf	5E-11	4	8.00E+10	2535

According to these calculations, the manuscript now includes the following sentences at different parts of the discussion chapter*:*

- *Considering strain rates as at modelled the base of the EDML (~ $9x10^{-11}s^{-1}$; Weikusat et al., 2017) the CPO would be preserved for a most of ~ 2,8 kyr.).*
- *Assuming strain rates calculated in ice shelves (~ $5x10^{-11}s^{-1}$; Millstein et al., 2021) the inherited fabric would be preserved for at most ~ 2,5 kyr.*
- *Our results suggest that, under natural conditions, as for example the NEGIS onset, where the velocity increases by 40 m/yr over a distance of 120 km (i.e., strain rate of ~$1x10^{-11}$ s^{-1}) (Joughin et al., 2018) an inherited fabric would be preserved for at most ~ 7 kyr. However, in the NEGIS margins, where the flow is considerably faster (i.e., strain rate of ~ $4x10^{-10}$ s^{-1}) (Joughin et al., 2018), an inherited fabric would be destroyed in a mere ~ 200 years.*

Technical corrections:

L62: I do not think this is the intended Alley paper—perhaps (Alley, 1988)?
Corrected to Alley, 1988

L95: There are two other studies that model fabric changing in new deformation regimes. (Thorsteinsson et al., 2003) looked at some examples with overprinting. This exact question is addressed by (Lilien et al., 2021). These studies do not negate the novelty of the present work, since they both used a different type of model, but this is not the first study to consider such a question.
Many thanks for pointing us to these works. We have included references to them in the text: *"To date only a few modelling studies have addressed the issue of the evolution of polycrystalline ice that has previously experienced flow. Examples are Thorsteinsson et al. (2003), which evaluate to what extent an observed CPO tracks the local stress within an ice sheet, and Jansen et al. (2016), who study the viscoplastic response of ice polycrystals with a starting CPO. More recently, Lilien et al. (2021) analyse whether the effect of changes in ice-stream flow can be recorded in ice-crystal fabrics."*

L116: This quite circuitous—we have direct measurements of velocity that show extensional flow at ridges (or, really, we only call them ridges because flow is extensional), so there is no need to use CPO to conclude it.
Following the reviewer's suggestion we have removed the following sentence from the text: *"Evidence for extensional flow transverse to the ridge has been found in ridges (Fig. 1b), as in the NorthGRIP (Wang et al., 2002; Faria et al., 2014) and EDML ridges (Weikusat et al., 2017)."*

L137: I strongly suggest altering the notation. *z* as the vertical coordinate in 3D is such a widespread convention that using *y* vertical leads to unnecessary confusion, and I see no benefit. This is compounded by the terminology for shear; the authors refer to the shear plane rather than the plane in which the shear happens (e.g. "horizontal simple shear" for shear in their xy), which I have heard called "vertical simple shear" since motion differs vertically. I suggest calling it "simple shear in the vertical plane" to avoid all ambiguity. I am particularly confused by things like line 225, where the authors call V2 shear "on" the horizontal plane (I think this is a typo, but the terminology leaves me unsure).

We have modified the coordinate notation, according to the reviewer's suggestion. Now *z* is the vertical coordinate. We have also modified the terminology for the two shear conditions applied to avoid ambiguity: *simple shear in the vertical plane (xz)* and *simple shear in the horizontal plane (xy)*.

L173: Perhaps I misunderstand how the full-field model works, but why is the bulk exponent discussed here? The model captures individual grains, so should we not care about the grain exponent, which need not be the same as the bulk exponent (e.g., Rathmann et al., 2021)? Experimental evidence for slip on individual monocrystal slip systems indicates that the exponent is in fact closer to 2 for basal glide (Duval et al., 1983 Figure 2), and I do not think this is evaluated in Bons et al. To be clear, I do not think that anything in the simulations needs to change, but it would be good if this discussion clarified the grain/bulk distinction and did not introduce the bulk n=4 discussion unless needed.

This is an interesting point raised by the reviewer. In the model, we only consider deformation by dislocation glide with the same stress exponent for each slip system. Simple scaling arguments then give the same stress exponent for the bulk aggregate. Simply, because there is no mechanism with any other stress sensitivity at play. we are aware that Duval et al. proposed a lower exponent for basal glide. One must bear in mind that the easy-glide system may not be rate controlling. Fig. 2 of Duval already indicates that the bulk stress exponent is closer to the non-basal glide. Our simulations also show that although basal slip is by far the easiest, the bulk behaviour is rate controlled more by the other slip systems (see Llorens et al.2017 and de Riese et al. 2019.

One could ask the question whether we should have included grain-boundary sliding (GBS) as well. For example Behn et al. (2021) argue that the stress exponent is <4 because of GBS, especially in warm ice. Cold ice (245 k) is according to their model mostly in the dislocation creep regime at natural strain rates. Only at low strain rates would GBS dominate, but at low strain rates volumes of ice would hardly move and therefore also no move into different flow regimes. As such, non-dislocation-mediated creep dominated flow would not be relevant for this study. Although a stress exponent of 3 or 4 is highly relevant for the flow of ice sheets, as argued in Bons et al., 2018 our comparisons showed that it makes little difference for CPO's.

Eq 1: This equation should be re-written to conform with standard typesetting conventions, and the explanation should be expanded. What is the summation variable *S* (I assume it is the slip system)? I am guessing that *sgn* is the sign, but by convention (and ISO standard) that should be non-italic (indeed, I spent a while wondering why gravity, n and s were being multiplied). I also suggest dropping the "x" for multiplication, as with tensorial quantities it is often confused with the cross product; ISO standards allow skipping the symbol entirely.

We have modified the notation of the equation according to these comments:

$$\dot{\varepsilon}_{ij}(u) = \sum_{s=1}^{N} m_{ij}^{s}(u)\dot{\gamma}^{s}(u) = \dot{\gamma}_0 \sum_{s=1}^{N} m_{ij}^{s}(u)\left(\frac{\left|m_{ij}^{s}(u)\sigma'_{ij}(u)\right|}{\tau^{s}(u)}\right)^{n} \times \operatorname{sgn}\left(m_{ij}^{s}(u)\sigma'_{ij}(u)\right)$$

(1)

Where $\dot{\varepsilon}_{ij}$ is the strain rate, σ'_{ij} is the deviatoric stress, m^s_{ij} is the symmetric Schmid tensor, $\dot{\gamma}^s$ the shear strain rate, τ^s is the critical resolved shear stress defined for the all active slip systems (s): , $\dot{\gamma}_0$ is a reference strain rate and n is the rate sensitivity exponent."

L205: There is no mention of the single-regime simulations that I can see—it would be good to mention these in the first paragraph.

Single-regime simulations are now described in section 3.2, where the boundary conditions are indicated: "*For comparison, results from simulations of microstructures deformed under a single-deformation event (**V_2**, V_3 and V_4) are shown together with all series results*."

L455: This does not seem like a fair characterization of Smith et al., 2017; I do not think they claimed anything that contradicts the results here. They note that large flow changes have occurred recently in ice streams and argue that this may be evident in the fabric. As pointed out in the specific comments, this it would be useful to put a timescale on the results here as well as a natural strain so that results can be compared to other studies. My sense is that there is no conflict, but regardless to dismiss their consideration of the possibility by calling it an "assumption" is inaccurate.

According to the reviewer's suggestion, we have included a timescale in the results. This part of the discussion does not include the reference to Smith et al. (2017) now: *"Our results suggest that, under natural conditions, as for example the NEGIS onset, where the velocity increases by 40 m/yr over a distance of 120 km (i.e. strain rate of ~$1x10^{-11}$ s^{-1}; Joughin et al., 2018) an inherited fabric would be preserved for at most ~ 7 kyr. However, in the NEGIS margins where the flow is considerably faster (i.e. strain rate of ~ $4x10^{-10}$ s^{-1}; Joughin et al., 2018), an inherited fabric would be destroyed in only ~ 200 years."*

L464: This makes it sound like the CPO does not change, when I think the point is intended to be that it changes much more slowly.

Changed to "*and therefore the CPO is slowly modified…*"

Figure 8: "with vertical dashed line."

Included in the figure caption "*Transition of deformation regimes are marked with a vertical dashed line*"

L532: FSE was previously defined

We have removed the FSE definition from this paragraph.

L478: This seems to be a conclusion of previous studies rather than the present one

All simulations presented in this work start with a random distribution of c-axes. Therefore, the conclusion indicated in L478 comes from the description of the CPO evolution during the first deformation regime (starting with a random fabric), that in all cases develop a CPO with *c*-axes mostly oriented parallel to the compression axis, and a-axes oriented parallel to the elongation axis of the finite strain ellipsoid.

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