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.

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.

   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?).

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 V_2 and V_4 (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.

3. I do not think that V_3 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

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

Along these lines, I am a bit skeptical of the total strains experienced with V_4 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.

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."

Technical corrections:

L62: I do not think this is the intended Alley paper—perhaps (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.
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.
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 V_2 shear "on" the horizontal plane (I think this is a typo, but the terminology leaves me unsure).
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.
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.
~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.
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.
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.
Figure 8: "with vertical dashed line."
L532: FSE was previously defined
L478: This seems to be a conclusion of previous studies rather than the present one

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