Review 2

> This paper offers a comprehensive and detailed study of fabric evolution models, both grain size and macroscopic scale, and apply the model to both an ice stream and an ice divide with a range of different anisotropic rheologies. It is quite interesting to see the gaps between these models and how they fit into the realistic world. Especially, the authors relate grain rotation and anisotropic rheology to their models and reproduce fabric observations at an ice stream, which is of great significance to current ice-stream studies, where the development of ice fabric and its influence on ice streams are often overlooked. Overall it is a well executed study, and I look forward to seeing it published. I believe this paper will be a useful reference for ice modellers and also draw wider attention from the ice community to the importance of ice fabric and anisotropy regarding ice flow.

We thank the reviewer for their positive comments and their taking the time to review the paper

> I feel this is already solid work. I just have a few minor questions that I'd like to mention here.
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> 1) Is there a reference for 'a factor of 8' in Line 34 for the viscosity enhancement factor.

We will clarify this by changing it to *the effect of fabric is usually only represented by adding to the viscosity an enhancement factor, for which values between 3 to 8 have been suggested [Graham et al., 2018]*

> Maybe it would also be good to move the statement like '(Line 75) For ice, slip along the basal plane - with unit normal denoted by the c-axis - is approximately 70 times easier than other slip systems (Duval et al., 1983).' earlier in the Introduction.

We would rather keep this statement here as although there is large enhancement observed for a single crystal, for a polycrystal the maximum enhancement observed is around 10 [Pimienta and Duval, 1987], so placing this factor of 70 near Line 34 would be drawing a false equivalence in our opinion.

> 2) Line 100 'all large-scale models have so far been limited to linear grains'. As for some large-scale models (e.g. Zhang et al., 2024), the problem is actually not due to linear grains but there are no real grains. Instead, ice particles simulate grain aggregates and each particle is defined with a power-law rheology.

We thank the reviewer for pointing this out and will update the text to highlight it is most, not all, models.

> 3) It seems the authors mainly consider the ice inside the ice stream but not at the ice stream's shear margins (such as Sections 5.2, 6.3). It would be good to mention this in the paper because the ice fabric at shear margins is different. And the power-law stress exponent n would also be important regarding strong shearing areas. I understand normally it is fine when n is considered as 3 in ice models (Line 451), but recent studies also suggest a higher $n = 4$ (Bons et al., 2018; Ranganathan and Minchew, 2024).

We agree with reviewer about the different fabrics in shear margins and will update the text to clarify we are referring to the flow inside an ice stream. Regarding $n = 4$, we agree it would be interesting to investigate but we believe is beyond the scope of this work.

> 4) The authors assume the fabric undergoes unconfined compression as it moves vertically down at the ice divide, and the deformation is steady over time and the observed fabric is simple - a single maximum. I was just wondering if the ice fabric would also rotate when considering Raymond bump effect (Martín et al., 2009) that ice layers are not flat in an extensional environment (e.g. Figure 3)?

Theoretically, exactly at the ice divide the deformation should be solely unconfined compression. In reality, an ice core will never be drilled exactly at the divide and the divide moves over time. Nevertheless, this deformation condition dominates and modelling ice divides this way is well established in the literature [Castelnau et al., 1996, Montagnat et al., 2012]

Or I feel this question is also similar to point(3). At the beginning part of this paper (such as Abstract and Introduction), the authors say they can reproduce observations at both an ice divide and at an ice stream, but the locations and specific cases of GRIP and EGRIP are not mentioned. Perhaps it would be helpful to add a few words at least in the Introduction to specify the areas the authors want to focus on?

We agree with this point and are happy to make this clear in the introduction

5) As for the last paragraph in Section 7.4 (Lines 662-666), I would stand that the ice stream at EGRIP is a recent formation and may not be as old as 16,000 years (e.g. Fig. 1 in Tabone et al., 2024). But as for the age of around 3000 years, I think it would not be a problem for the authors' results. Because the shear margins were established 2000 years ago from Jansen et al (2024), and before this stage, there could be convergent ice flow or proto ice stream without well-developed shear margins.

We thank the reviewer for pointing this out and will add this point into the discussion

The writing is good but some words or statements are repetitive and could be removed, such as,

Line 14: 'approximately **the** the same stress'. One of 'the' can be deleted.

Line 223: ' If we only have **have** macroscopic quantities'; ' in terms of these **these** macroscopic'

Line 414: 'that the **the** final age '.

We thank the reviewer for noticing these repetitions and will correct these, alongside further proof reading the manuscript

Line 642-645: ' In this contribution, we have constrained a general model for fabric evolution against an ice divide and an ice stream... where previous models could not provide accurate predictions.' This part could be removed, as the authors have clearly stated this in other sections several times. There is no need to repeat it in the 'Limitations of this Analysis' section.

We agree with the reviewer and will update the manuscript accordingly

References

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