General comments

The authors have used a numerical model to explore changing CPO orientation and strength in ice deforming in flow regimes intermediate between pure and simple shear and with varying degrees of vorticity. They are able to present detailed data representing ice fabrics at much higher strains and under a much wider range of conditions than it is possible to achieve with laboratory experiments, which is an important contribution to the field. Their examination of steady-state fabrics at high strains is particularly valuable.

The manuscript could be made more impactful by adding more thorough comparisons between model results and experimental results from the cited literature. Some more clarity on the model setup would also be useful.

We thank the reviewer for their helpful and positive review of our submission. We agree with the changes suggested here and below. In particular, we agree that more explanation of the model would be helpful, and this is discussed also in our response to referee 1, which we summarize here. The proposed additions include clarifying the assumptions in the model, in particular distinguishing that we model the evolution of the distribution function of c-axes as a continuum representation of microscale processes, as opposed to explicitly accounting for individual grains.

As noted by the referee below, reviewing the model assumptions will cover some similar ground to the validation/calibration which was the focus of Richards et al. 2021, but we now understand that a more detailed review of the model, the assumptions taken, and its validation is warranted and important to reinforce here also, and this can be straightforwardly implemented in revision.

Specific comments

l.30: This statement (that looking at the c-axis alone is sufficient) needs some justification. Chauve et al. [2017] have found that non-basal slip systems are very significant at high homologous temperatures. Are you assuming that at lower temperatures they are no longer significant? Please clarify.

Thank you for highlighting this. Indeed, we do not wish to imply that non-basal slip systems are unimportant, but that, for the aim of modelling the fabric, the c-axis provides a sufficient leading order approach to capture experimentally produced fabrics, as confirmed in Richards et al. 2021, for example. In other words, while a description of the model is reduced to tracking of the c-axis distribution, some amount of the effects of non-basal slip sliding for higher temperatures is still likely captured by the empirical calibration (Richards et al. 2021). We will modify the referenced statement to make this clear.

l.50: Jun et al. [1996], and Budd et al. [2013] also performed experiments with a combination of shear and compression.

Thank you, we will include these references also.

l.76, and Fig.1 caption: There are referenced experimental examples for all of these fabrics aside from pure shear. I suggest citing Kamb [1972], or a more modern reference if one exists (I'm not aware of any).

Thank you again, we agree and will add these references.

l.113: I'm unsure what $W > \infty$ means. What is more rotational than pure rotation? Is there a section of the Ross Ice Shelf that is continually spinning in circles? Please make this clearer for easily confused readers like me.

We apologise, this was a typo. It should have said $W>1$ rather than $W> \infty$, which we will be sure to correct.

Section 2.4: This is almost a rewording of your stated research questions in lines 18-25, but not in an obvious way. It would be clearer to refer more explicitly to which question you intend to answer in which section. As I've interpreted it, you've addressed your first question (which deformations are present in the natural world) in section 2.2.2, and the following two (how fabrics evolve, and how steady-state depends on temperature and deformation) will be addressed in the following sections. In that case, it would be clearer for the reader if you reiterate the second two questions here with similar wording as in the introduction, and do the same with the first question where you address it above.

We agree with the comments here and will make these changes.

Section 3: I am not entirely clear on what is being physically represented in the model. E.g., are non-basal slip systems being incorporated, even just by an empirical parameter? How are grain boundary interactions being represented, if at all? To find the answers, the reader must decipher a page of equations. I'm aware this has been covered in more detail in Richards et al. [2021], but a brief explanation at the beginning of your methods section referring back to section 2.1.1 and stating how the specific mechanisms you have described are represented in your model would be very useful.

We agree and plan to add more description of the model to cover this. Our initial intention was to defer to the validation in Richards et al. 2021, but we now appreciate that a thorough review of the model and a more detailed account of its physical representations is worthwhile to include here also,

As remarked above, the effect of non-basal slip systems on the c-axis distribution is captured empirically within the model by the experimentally calibrated temperature-dependent parameters iota(T), beta(T) and gamma(T). It was not our intention to imply that these slip mechanisms are not important at high temperatures, and we will be sure to clarify this in revision.

Regarding the representation of grain boundary interactions, we remark that the model is a continuum model of the evolution of the distribution function of c-axes, so only the bulk effect of grain boundary interactions on the distribution function is modelled. The fact that, once calibrated, we can reproduce experimental results – without additional fitting parameters - demonstrates that the approach taken is sufficient to model this distribution function.

Figure 5: It would be comforting for an experimentalist reading this paper to see some real experimental results alongside the model results, to show the agreement between the two on the presence of double clusters, and their strength. Fig. 5 shows your results from a simulation of simple shear at -5°C, and this is a scenario which has been tested experimentally by Qi et al. [2019]. Perhaps you could take some results from Qi et al, where they have provided detailed c-axis plots and J-indices, and present them alongside your model data plotted in a similar fashion. Again, I know this comparison appears in some capacity in Richards et al. [2021], however it would make this current paper much more convincing to see specifically the agreement on double clusters.

Thank you for this suggestion. We are happy to provide this by adding a pole figure from Qi et al. 2019 into this figure and adjusting the strain so it matches.

Fig. 8 and Fig. 9: What is the resolution of the data used to make these contour plots (i.e., how many W and T values were tested)? This information would be useful for interpreting parts of the figure. E.g., in Fig. 9c, it would be good to know how many data points make up the "wiggles" between the single cluster and secondary cluster zones at high W.

The resolution of these diagrams is 100x100. The boundary between regimes exhibiting a single-maxima versus a secondary cluster is not exactly sharp and we plan to update this figure to broaden the current sharp boundaries to highlight the more gradual transitions.

Section 5.3: The observation made earlier in the text, that highly rotational fabrics can produce a CPO which would ap- pear isotropic when sampled at the resolution of most commonly used techniques, is important for ice core interpretation. It should be mentioned in this section.

Thank you for highlighting this, we will certainly take your advice and highlight this more explicitly.

l. 321 It is unclear what is meant by "other constraints". Other data on strain history and temperature in the area?

Other constraints could include, for example, knowledge that the deformation history was constant to good approximation over a certain distance, in which case the vorticity number and temperature could be estimated more precisely. We will be sure to list potential additional constraints in more detail.

l. 325: "We also assume an initial random orientation for the fabric". Although I assumed that was the case, this information would have been useful to know much earlier. If it was explicitly stated above, I missed it!

Thank you for highlighting this, we will update the manuscript to clarify this earlier.

l. 326: It is true that ice formed from accumulation will have an initially random fabric, but as you say the fabric can adjust to the stress conditions very quickly (within strain of 0.2). It would be interesting to know if a pre-existing fabric affects the evolution of a CPO once the stress conditions change. That's a completely separate paper of course, but worth pointing out as an area of future research...

Indeed, this is an interesting question, we agree with you that this would be a separate paper – in fact, this is one avenue we are planning to explore.

Section 6: I think there are two important findings mentioned in the text which have not found their way to the conclusions: firstly, that the W =0 case which is very commonly found in experiments results in a CPO which is different to that found if there is even a small amount of rotation, implying that the most common experimental scenario is not representative of most real scenarios (line 312). Secondly, that the double-cluster/cone CPO which we see so often in experiments is not present in your results in steady-state, and appears not to persist beyond the highest strains which we can reach with experiments (line 234). These deserve a brief mention here.

We thank the reviewer for highlighting the importance of these findings and will be sure to emphasise this point. We agree that our finding that slight rotation makes the double maxima fabric unstable at high strains, is interesting. . We remark also that although the steady-state fabrics produced for W=0.1 and W=0 are very different after sufficient strain and in steady state, the initial fabrics at low strains are similar in accordance with experimental observations.

Technical corrections

Fig. 5 caption: "which", not "witch"

l.274 should read "fabric patterns"

l.50 "strain" rate response

Apologies, thank you for highlighting these typos, we will update the manuscript to correct them.

References

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