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.

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.

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

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).

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.

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.

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.

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 $-5^\circ\,\mathrm{C}$, 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.

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.

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

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

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!

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...

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.

Technical corrections

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

l.274 should read "fabric patterns"

l.50 "strain" rate response

References

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