Review 1

This paper presents a unified framework in which most recent, large-scale ice-crystal fabric modeling can be considered. The challenge in constructing the framework was mainly in the non-collinearity between stress and strain rate with anisotropic rheologies, which bleeds into the fabric evolution. The unified framework clarifies this effect of anisotropy and allows for using quite a few different rheologies in a unified model of fabric evolution. This unified fabric model is used to try to reproduce data from the GRIP and EGRIP ice cores, with 7 different rheologies, and parameters tuned for each rheology to fit observations. Several of the rheologies allow the model to do a good job reproducing the fabric observations. From these results, the authors recommend one of these rheologies for use going forward.

General comments

This is an interesting study, and I enjoyed reading it. The writing is good though occasionally repetitive, and the figures are well made. While this framework is not a huge change from the existing descriptions of lattice rotation, the use of the normalized stress to support this general framework is a nice addition, and the framework provides clarity on how parameters relate between models. Overall, I think it is valuable to be able to compare these rheologies in a common framework for fabric evolution, and I am quite impressed by the number of rheologies compared.

We thank the reviewer for their positive and detailed comments, and we believe that by addressing the points they mention the manuscript will be a stronger paper. Please find our detailed responses below

However, I think the authors should temper and reframe the latter part of work; this is a convoluted way to test rheologies, and I am not convinced that it should be used for this purpose. Contrary to the claim in the introduction, it has in fact been possible to test rheologies—we have laboratory deformation tests against which to compare rheologies, and this has been done by one of the studies considered extensively within the present manuscript (Rathmann and Lilien, 2022b). Comparing to deformation tests is a much, much more direct test of the rheologies than feeding them through a fabric evolution with tuned parameters, uncertain flow history, and simple assumptions about flow (shallow shelf or Dansgaard-Johnsen). As the authors point out late in this work, the Sach's hypothesis cannot produce the extent of enhancement observed with real fabrics by about an order of magnitude—that is a lot more straightforward than sending it through a fabric model! This work tuned multiple parameters, one of which carries physical meaning (lambda), to make these rheologies reproduce observed fabrics—without knowing that the value of lambda is reasonable, how do we know that a rheology gets the right fabric for the right reason, rather than due to this tuning? I do find the comparison of fabric predicted using Taylor, Estar, CAFFE illuminating, since I would have expected them to do better, but I am not convinced that this is a meaningful way to distinguish between the other rheologies considering the tuning and uncertainty. As a result, I would like to see the conclusions tempered. In terms of rheologies, I think all I conclude is that most of them can be tuned to do a pretty good job reproducing observations of fabric—not that one of Sachs, Rathmann, or GOLF is superior based on the present work. I think the work is publishable in The Cryosphere with relatively minor changes if this rheology recommendation is tempered/removed.

We are happy to temper the conclusions as suggested, indeed the modelling in this paper is only looking at two single locations in the whole ice sheet. We also acknowledge that data from laboratory experiments exists. However, based on our previous work (Richards et al. 2023), we found that the fabric evolution model that worked well in laboratory experiments (Richards et al. 2021) did not work when applied to the natural world. This result led us to the hypothesis that, at the lower strain rates of the natural world (about 5 orders of magnitude), different physical processes are dominant at the micro-scale. This hypothesis motivated this work; in which we try to compare rheologies at the natural strain-rate scale. We will emphasise this idea more in the introduction and highlight the advantages and disadvantages of this method compared to using laboratory data, as well as tempering the rheology recommendation as suggested.

Specific comments

First, it is Dr. Pettit, not Petit.

We thank the reviewer for pointing out this mistake and will correct it.

I think there are implicit assumptions about collinearity of stress and strain that might confuse the reader. At a minimum, the assumptions should be stated. Equation 6 does not appear to be a result of Equations 2 and 5, and I think it involves more assumptions that the authors are crediting. Clearly plugging equation 2 into 5 does not straightforwardly lead to 6—perhaps I am missing a complicated derivation demonstrating this, but I do not think that I am. Rather, I think that this is essentially an assumption about the collinearity of the strain rate and stress. If I understand, 6 is essentially an alternative to 5, and then by combining them we get something like the fabric evolution equation in 44? Considering the conclusions of the paper, this does not really make sense—the reader needs a full accounting of whether Eq. 6 is alternative to the strain-rate based model (as I read it) or an equivalent formulation as currently implied. Relatedly, I think the introduction of normalized stress is muddled, which might confuse the reader about what is colinear under that assumption. In essence, I think Eq. 28 is essentially a property of 29 that emerges for isotropy, but it took me a long time to see this. If we had a clear definition of normalized stress early, that was then consistent across rheologies, I would have an easier time conceptualizing the results.

Eq (6) does in fact come from directly plugging Eq (2) into Eq(5). We will include this derivation in the supplement as it is not straightforward. In index notation, Eq (2) and Eq (5) are:

$$\dot{\varepsilon}'_{ij} = \eta^{-1}\left(\tau'_{ij} + \frac{3(E_{cc}-1)-4(E_{ca}-1)}{2}\tau'_{kl}c_kc_lc_ic_j + (E_{ca}-1)(\tau'_{ik}c_kc_j + c_ic_k\tau'_{kj}) - \frac{E_{cc}-1}{2}\tau'_{kl}c_kc_l\delta_{ij}\right) \tag{R1}$$

$$\frac{dc_i}{dt} = \omega'_{ij}c_j - (\dot{\varepsilon}'_{ij}c_j - \dot{\varepsilon}'_{kj}c_kc_jc_i), \tag{R2}$$

Taking advantage of the fact that $c_ic_i = 1$, and defining $\tau'_c = \tau'_{ij}c_jc_i = \tau'_{ji}c_jc_i = \tau'_{kl}c_kc_l$ etc.

$$\dot{\varepsilon}'_{ij}c_j = \eta^{-1}\left(\tau'_{ij}c_j + \frac{3(E_{cc}-1)-4(E_{ca}-1)}{2}\tau'_cc_i + (E_{ca}-1)(\tau'_{ik}c_k + c_i\tau'_c) - \frac{E_{cc}-1}{2}\tau'_cc_i\right) \tag{R3}$$

and

$$\dot{\varepsilon}'_{ij}c_ic_j = \eta^{-1}\left(\tau'_c + \frac{3(E_{cc}-1)-4(E_{ca}-1)}{2}\tau'_c + (E_{ca}-1)(\tau'_c + \tau'_c) - \frac{E_{cc}-1}{2}\tau'_c\right) \tag{R4}$$

hence

$$\dot{\varepsilon}'_{kj}c_kc_jc_i = \eta^{-1}\left(\tau'_cc_i + \frac{3(E_{cc}-1)-4(E_{ca}-1)}{2}\tau'_cc_i + (E_{ca}-1)(\tau'_cc_i + \tau'_cc_i) - \frac{E_{cc}-1}{2}\tau'_cc_i\right) \tag{R5}$$

Then, subtracting Eq. (R5) from (R3), and renaming indices in Eq. (R3) so that $\tau'_{ik}c_k \to \tau'_{ij}c_j$:

$$\dot{\varepsilon}'_{ij}c_j - \dot{\varepsilon}'_{kj}c_kc_jc_i = \eta^{-1}\left((\tau'_{ij}c_j - \tau'_cc_i) + (E_{ca}-1)(\tau'_{ij}c_j - \tau'_cc_i)\right) \tag{R6}$$

finally giving:

$$\dot{\varepsilon}'_{ij}c_j - \dot{\varepsilon}'_{kj}c_kc_jc_i = \eta^{-1}E_{ca}(\tau'_{ij}c_j - \tau'_{kj}c_kc_jc_i) \tag{R7}$$

Consequently, this does not require any assumptions about co-linearity of the stress and strain-rate.

Regarding the definition of the normalised stress, the reviewer is correct that Eq (28) is a property of Eq (29) under isotropy, and we agree that the normalised stress could be defined in a clearer manner, and we will define it earlier as the reviewer suggests.

It does not really make sense to try to fit these rheologies to data when they are misconstrained at the surface. That is to say, it appears that the models struggle at EGRIP in part because the shallowest available data are not that close to isotropy. I suggest assuming that reorientation processes differ in the firn, justifying the use of the shallowest measurement of fabric as the initial state for the model. In this way, the misfit at the top of the ice sheet, which I do not think is due to the models per se, can be avoided, and they can be compared over more depths.

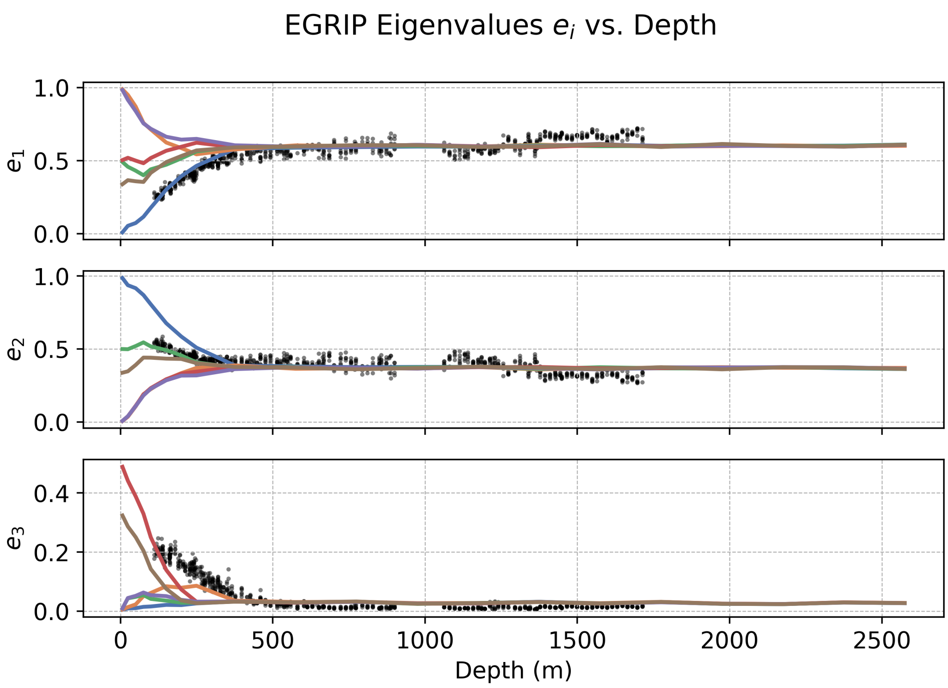

Figure 1: Evolution of eigenvalues at EGRIP with depth with Sachs model, for different initial conditions

We agree with the reviewer that the fabrics at the shallowest depth are due to firn processes which we are not modelling. However, we find that the fabric is insensitive to the initial condition below a depth of 400, as shown in Fig. . In the manuscript, when fitting the parameters, we only include data below this 400 m as part of the inversion.

> I do not see why the authors exclude migration recrystallization under the Taylor hypothesis—including it by using the strain rate to calculate s' seems like a reasonable alternative to allow better comparison, and presumably would be very easy to do. Particularly since it doesn't need to be coded up, as beta is taken to be zero, this is no work to include.

Based on our third comment, where we establish that we are not making any assumptions in Eq. (6) but rather this follows from the above derivation, we are not making a Sachs or Taylor assumption in our derivation of migration recrystallization in section 2.3.1. We then set all migration recrystallization to zero for the rest of the paper.

> On the topic of migration recrystallization, there should be discussion on how the possibility of low temperature, high stress/strain migration recrystallization might affect the results. There is some work that suggests such a possibility (see the Faria 2014 reviews), so its effect on the results here should be considered. I do not necessarily see a need to tune Beta, but discussion is warranted.

We are happy to add this into the discussion, we thank the reviewer for highlighting this.

> This paper would benefit from a table of symbols/notation, or at a minimum more clear definitions of key notation. I can make sense of it based on familiarity with related literature, but I do not think the equations stand on their own. For example, I can guess about implicit outer products in Equation 2, but considering that cc was used two lines above to refer to compression in the c axis direction rather than an outer product, it is really confusing here. In another, inconsistency in use of h and f in equations 7, 11, 12 leaves me confused—I cannot tell if this is two names for the same thing or some notation that I do not understand. I did not understand what n is in Equation 21 and 22 until much later. A table would help all of these.

We will add a table of symbols and mathematical operations to aid the reader. We have aimed to be consistent throughout the paper but agree with the the reviewer that the examples highlighted are confusing, and will change them. In particular, we can update Eq. (1) to not result in cc having two different meanings.

> There are times that the paper is insufficiently specific about the category of models considered, which leads to statements that are incorrect at the generality in which they are presented.
>
> Microstructural models do some things that this paper suggests are impossible (e.g., model dislocation density, L136). In general, microstructural models like ELLE (Llorens et al., 2022) deserve mention in the introduction, as potentially the best available tool to consider fabric development in 0D. A close read by the authors, making sure that all categorical statements apply to all models, or that statements are qualified, is needed.

We thank the reviewer for highlighting this and will add this into the introduction and update the paper to ensure all these statements are correct.

> The paper is a bit under-referenced. It gets better as it goes on, so this is most evident in the introduction; several claims need citation, and there are several places where additional references were missed. Each of the first three sentences in the introduction deserve citations. The list of studies at lines 32 to 33 misses several recent references that used coupled flow/fabric modeling: (Gillet-Chaulet et al., 2006) considered an idealized divide/flank; (Rathmann and Lilien, 2022a) considered an idealized ice stream. (Lilien et al., 2023) was coupled used real geometry on a flowline at a divide and (Gerber et al., 2023) was also coupled used real geometry in an ice stream. The claim that coupled modeling studies can be counted on one hand needs to be amended. Similarly in lines 30-36, it is not really fair to skip the ESTAR line of work (Graham et al., 2018; McCormack et al., 2022), which lies intermediate to the two approaches mentioned and has been used for relatively large scale simulation of Thwaites. At line 43, the authors are missing a number of direct measurements in faster-flowing areas, such as (Jackson and Kamb, 1997; Voigt, 2017) among a handful of others. I think the current reference for the EGRIP fabric is (Stoll et al., 2024), though this may not have been out at the time that this preprint was submitted. At line 54, (Lilien et al., 2023) has a similar conclusion to (Richards et al., 2023). At line 57, I would argue that (Martín et al., 2009; Pettit et al., 2007) are both anisotropic rheologies in their own right. Certainly both (Rathmann and Lilien, 2022a, b) have different nonlinear rheologies that they use, so the list is an undercount of proliferation. The temperature in line 396 needs a citation.

We thank the reviewer for highlighting this and suggesting these references and will add these into the paper and make sure the introduction is sufficiently referenced.

> Title: This study does not really compare fabric evolution models, but rather unites them.

We are happy to change this to something like "Unifying and testing fabric evolution models and anisotropic rheologies"

> L56: This claim is not really correct. We have long had laboratory deformation tests, so it seems untrue that it has not been possible to test anisotropic rheologies.

We are happy to update this to "Secondly, there has been no way to test whether an anisotropic rheology *at strain-rates seen in ice sheet*", as laboratory experiments are at much higher strain rates. Laboratory experiments also cannot measure all of the the different components of the anisotropic stress tensor which is needed to characterise anisotropic rheologies.

> L125: Only appears true at depth in ice sheets, whereas near the surface NGG is normally conceptualized as something like a part of firnification.

We thank the reviewer for this clarification and will update the text with references

> L140: is c_i really scalar?

This section of the text has not been updated from a previous iteration using index notation, and we thank the reviwer for spotting this and will update it to $\boldsymbol{c}$

> L226: There are infinite possible assumptions, and indeed others have been used (like the linear combination of these)

We will re-word the text to say end members rather than possible assumptions

L291: from not form

We thank the reviewer for spotting this

L429: Why not include the Martin approximation from Rathmann and Lilien, too?

We are happy to add this. We have tested it and it gives very similar results to the unapproximated version

L437: Latex error in min

L438: extra "two"

L467: Need subscript on the strain rate

We thank the reviewer for finding these errors and will correct them