This paper present an application of specCAF, a numerical model of fabric development based on a continuum theory by Faria and Placidi, and described in Richards et al. (2021). Compared to previous works on ice fabric evolution, this paper discuss the fabric patterns obtained for a wide range of vorticity numbers, including highly rotational flows, using synthetical 2D experiments. To justify this approach, the authors have computed the vorticity number from observed horizontal surface velocities in Antarctica.
They obtain big (>1) vorticity numbers in large portions of the ice-shelves with curved stream lines, and a conclusion of the paper is that in such regime the fabric should remain nearly isotropic.

My main comment, is that I remain very sceptical about this conclusion and the interpretations of the results for fabrics in natural flow. The authors claim that most previous studies have focused on pure and simple shear, this is true, but they forgot to mention that the justification is that something between pure shear and uni-axial compression in the **« vertical «** direction is supposed to dominate in the upper ice layers while simple shear (**parallel to the bed**) is supposed to dominate in the lowest layers, at least in the central parts of the ice sheets where ice cores have been drilled and direct fabric observations are available. It's not clear from section 2.2 how the spin and strain – rate tensors are computed for the observed Antarctic horizontal surface velocities ? It is assuming plane strain in the horizontal plane ? I don't think that an horizontal 2D plane strain would be a good approximation of the natural conditions in ice shelves. I still would expect to have a compression component in the vertical direction, so the interpretation of the results presented here in term of fabrics in natural conditions need better justifications.

I read the comments from the other reviewers and the author responses. The debate between Gagliardini and Faria has not really been clarified and I think that this papers could be a good opportunity to clarify the assumptions behind the continuum approach and how it compares with homogenisation models. Two points seems to require clarifications.
First, the classical approach in ice flows model is to solve the Stokes equations (or some shallow approximations) for a given flow law, i.e. a relation between the macroscopic strain-rates and stresses, that are then solution of the problem. It is not clear here how such a relation could be obtained from specCAF. Faria (2006a,b) gives some homogenisation rules to compute the macroscopic stresses, but it seems that is has never really been used. Instead Seddik and others (2008, 2011), using the CAFFE model, parameterized an « enhencement » factor as a function of the polycrystal deformability that depends on the fabric. Using the same argument as for the strain rates, i.e. the volume contains an infinitely large number of grains, Seddik and others (2008) claim that the stress tensor do not depend on the orientation. So it is not clear, (i) how both the stresses and strain-rates at the level of the species (i.e. using Faria's terminology in is reply to Gagliardini) can be equal to the macroscopic equivalent, but still with a viscosity tensor that would depend on the orientation, and (ii) if the macroscopic stresses computed this way would be solution of the continuum model, i.e. the balance equations that are derived in Faria's papers?
Second, an anisotropic model must be able to describe how the fabric evolves. Here, the model includes several processes, including rotation of the ice crystals due to basal-slip deformation. The equation used to take into account this effect (Eq. 5) at the scale of the species in the continuum approach, is based on equations that have been derived for single crystals. According to the description of their model (Richards et al., 2021) : *« If this equation is applied to an individual grain, it describes the c-axis rotation rate (Gödert and Hutter, 1998; Svendsen and Hutter, 1996) under the Taylor hypothesis (neglecting grain-grain interactions). However, since we are using a continuum model that assumes a large number of grains within any solid angle of orientation, any grain-grain interactions are smeared-out (Faria et al., 2008). In this continuum model, we do not therefore require the Taylor hypothesis. »,* From that I understand that the continuum approach would give a fabric evolution similar to an homogenisation model that uses the Taylor hypothesis ? So maybe, strictly speaking the continuum model do not use the Taylor hypothesis because it does not have grains, but at the end the equations that are

used for the species (i.e. the orientations) come from single crystals models ? As the model has been calibrated against experiments, this could potentially affect the interpretation of the relative contributions of the different recrystallisation mechanisms that are included in the model ?

I have few other detailed comments listed below :

- Sec. 2.2 : see my main comment, the procedure to compute the vorticity number needs to be better explained and justified especially if it's only done in 2D. Ice is incompressible, so tr(D) must be zero is this enforced? Also it's not clear of me on which length scale the velocity gradients are computed, directly using a finite difference from the original grid resolution?
- Sec 2.3 : *« At the other end of the scale, models such as presented by Gillet-Chaulet et al. (2005) track the evolution of tensorial descriptions of the fabric, without including migration recrystallization. These cannot accurately reproduce detailed fabric patterns but are computationally cheap enough for integration into large-scale models (Gagliardini et al., 2013). »*. Gillet-Chaulet et al. (2005) only present the flow relation, i.e. the anisotropic tensorial relation between the macroscopic stresses and strain-rates, so there is no fabric evolution at all. The equations for the fabric evolutions are presented in Gillet-Chaulet et al. (2006). The fact that it do not includes migration recrystallisation is not a limitation of the procedure itself. Seddik et al. (2011) also derive an equation for the evolution of the orientation tensors from the CAFFE model ; so in principle migration recrystallisation, as it is represented here, could be included within the same framework.
- Sec. 3.2 : explain what is *y* here and in Fig. 5 and what are the deformation principal axes with respect to this reference frame for the pole figures.
- Sec. 3.2 : give the expression for the computation of the strain (\gamma) from the strain-rates.
- Page 9, last line : *« Furthermore the pre-factor »,* I'm not such which pre-factor ?
- Fig. 5 : Maybe the schema for the single maxima is a bit misleading at it shows a single maxima in the vertical direction, while it is directed at 45 degrees.
- Line 209 : give the definition of the J-index before using it.
- Sec. 5.4 : *« The model SpecCAF used in our paper can be coupled with an anisotropic viscosity formulation to include directional variation in viscosity »*. Provide more details on the exact procedure, i.e. how the stresses are computed with SpecCAF, and the assumptions that would be required for this step.
- Sec. 5.4 : *« This has been done with simplified fabric evolution models which do not include recrystallization and temperature dependence (Martin et al., 2009). »* This gives the impression that Martin et al. use the continuum model while they are using an homogenisation model with the static (uniform stresses) assumption. Also, from the CAFFE model, Seddik et al. (2008,2011) derive an anisotropic flow law where stresses and strain-rates remain colinear. So if the same method is used here (depending oon the previous comment), it is not so clear that this model would also produce the syncline patterns in the isochrones that are mentioned few lines latter.

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