Responses to Reviewer Fabien Gillet-Chaulet

We thank the reviewer for their constructive review. We agree with the majority of the suggestions and appreciate all the comments raised. We believe the comments can be addressed in revision by including:

a. A complete clarification and review of the numerical model used and its underlying assumptions, including its place in the hierarchy of complexity for fabric evolution models. We agree with the reviewer and comments by reviewer Montagnat that the discussion between Faria (2008) and Gagliardini (2008) can be clarified here. Finally, we will be more explicit in the text to clarify that the model presented here is a fabric evolution model only; consequently, it is not set-up, nor are we seeking within the scope of the present work, to solve a coupled full-Stokes system. We do agree that such a model including a coupled Stokes system would be a good next step, hence we are happy to add in the discussion a future perspective of how this can be done.
b. Clarification that the discussion of vorticity numbers derived from two-dimensional strain rates in Fig. 1 was primarily intended to motivate the basic need to look beyond the limiting endmembers of simple shear and compression. We are not suggesting there is a direct link between the predicted 2D fabrics and these regions, and appreciate that the motivation to show Fig. 1 was not made sufficiently clear in the original submission. As correctly noted by the referee, there is the potential for significant three-dimensional deformations. We admit that the previous version did not clarify this sufficiently. As stated in our reply to reviewer Montagnat, we aim to use this figure primarily as a motivation for exploring deformations away from pure and simple shear, and 2D deformations are a logical first step away from this. Furthermore, following a comment here we realise that a clearer motivation for our 2D analysis arises from considering a vertical cross-section (in the (x,z) plane) of an ice sheet, for which simulations are often performed. This would encompass regions involving pure shear and simple shear and all intermediate cases between these endmembers (as noted by the reviewer), which will arise through the depth of the ice sheet. Again, not all regions of the ice sheet will conform to this regime precisely due to the presence of horizontal deformations, but the case of 2D deformations we consider provides a necessary first step for the systematic documentation of fabrics, which we would like to emphasise in revision.

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.

We remark that the surface vorticity number from Antarctica is merely intended as an illustrative example. For any complex flow field ice will experience deformations away from pure shear and simple shear. The presented work acts as a first and currently unexplored step towards deformations away from these endmember flow regimes, and as a first step we limit the analysis to 2D. In revisions, the later will be made explicit.

As mentioned in our reply to Reviewer Montagnat, we seek to correct the statement that curved streamlines necessarily lead to vorticity numbers >1. However, according to our analysis vorticity numbers >1 should lead to a weak fabric.

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.

We agree and, on reading this (and a similar comment received from reviewer Montagnat), appreciate that we – in the initial submission – failed to be sufficiently clear about the purpose of Fig. 1. It is used for motivating the analysis from first order observations. Indeed, vertical strain likely applies widely due to thickness variation of the ice sheet, and shear will indeed apply (particularly the lower 50%) of central parts of the ice sheet (with no slip at the base), and hence the direct application of fabric predictions in 2D cannot be attributed directly to these regions, at least without further quantification of the role of three-dimensional deformations.

Our intention with Fig. 1 was only to provide a basic quantitative indication of the diversity of deformation styles in natural ice flows beyond the idealised situations of simple shear and compression (whether two-dimensional or three-dimensional) on which experimental analysis has focused to date. It was not our primary intention to attribute the fabrics arising from two-dimensional deformations directly to these regions. The essential indication of the diversity of deformation styles is nonetheless helpful to motivate the study, particularly, we believe, for the benefit of highlighting the limitations of current experiments. We will address this in revision. As remarked above, our results here provide a first step towards documenting the full range of fabrics that can apply, since (for example) pure shear in the vertical can be included with just one additional parameter alongside the horizontal vorticity number W. Given that the exploration of 2D fabrics is already highly rich, it is sensible to retain the scope of 2D deformation alone for one paper before this additional complication is added.

That said, it is still interesting to discuss where two-dimensional deformations may apply to good approximation in the case of natural ice flows. As highlighted by the referee, we would expect, for example, that in approximately horizontally one-dimensional ice sheet flow, the *vertical* cross-section of the flow will experience a spectrum bridging simple shear at the base and pure shear near the surface. Indeed, this spectrum corresponds directly to the range of deformations we explore in this paper. In this case, horizontal deformations would affect this profile, and more work would be needed to address these more complex situations. Nonetheless, the motivation based on deformations experienced in the vertical plane is straightforward, and we would like to include it in revision with appropriate explanation of caveats; we are grateful to the reviewer for highlighting it here.

As an incidental point, we also remark that in a vertical cross-section of a horizontally one-dimensional flow, the relevant endmember for pure shear is the two-dimensional (confined) version that we report here, not uniaxial (radially symmetric) compression, the latter being the focus of experiments of compressed or extended cylindrical samples of ice. In fact, the fabrics produced in confined (two-dimensional) compression differ significantly from those in uniaxial compression, and it is the two-dimensional form included in our analysis here that is the one which is the most relevant endmember to discuss in the context above. Uniaxial compression by contrast requires radial spreading of a compressed cylinder of ice. This situation does not readily correspond to anything in natural ice flows (perhaps flow at the centre of an ice dome would be one, very rare, instance of this). We will clarify this important point in a revised manuscript, giving yet further motivation for our work.

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.

We certainly agree with the referee. In this section it was not our intention to assert that the surface velocities represent the full deformation field, but merely to illustrate that two-dimensional vorticity numbers away from 0 and 1 (including >1) derived from horizontal velocity fields alone motivates analysis of fabrics beyond those that have been analysed from existing laboratory configurations. As noted above, a one-dimensionally flowing ice shelf would indeed involve a pure shear flow in the vertical along-flow (flow line) cross-section (x,z). In such a case the *vertical* compression is equal in magnitude to the horizontal extension (by incompressibility). Although we had not mentioned it previously, this range of two-dimensional deformations arising in the vertical cross-section of a horizontally one-dimensional ice flow (in both central and floating regions of the ice sheet) will generally involve pure shear in the vertical cross-section, simple shear near the base, and a mixture of pure shear and simple shear elsewhere; these are precisely the regimes and spectrum of deformations we have studied. As noted above, this is a further and potentially clearer and more straightforward motivation for our analysis of two-dimensional fabrics than the illustration of horizontal surface deformations alone. Hence, we aim to include the aforementioned arguments/reasoning in the revisions.

In the late 1990 and early 2000 it was recognized in the geological community that flow in rocks cannot be approximated by endmember plane strain flow models alone. There is now an extensive literature within structural geology which developed conceptual models and analytical techniques to predict and recognize natural flow with vorticities between 0 and 1. In contrast, in the ice flow community, such analysis is not yet common place – here pure shear and simple shear has dominated discussions for both flow and fabric development models/interpretations. This may be mainly due to the fact that such endmember scenarios are a) experimentally straightforward to achieve and are the only experiments in the literature so far and, b) the two endmembers can – as a first approximation - be associated with different "ice flow scenarios". In the revisions, we suggest to include a short review of the geological vorticity literature.

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.

We agree with the reviewer that our paper provides a nice opportunity to clarify the assumptions behind continuum modelling of fabrics, particularly the relationship between the model and those for single crystals, and its relationship to the process of homogenisation (we elaborate below).

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?

The model considered here is for fabric evolution only for given deformations, which (for this purpose) does not require coupling to a flow model. While not the focus of the present paper, we nonetheless agree with the reviewer that methods for coupling SpecCAF with an anisotropic viscosity, to simulate the coupled fabric/full Stokes flow, are worth discussing, and we would like to do this in revision. We nonetheless emphasise that we are concerned here only with fabric evolution, and the details of this discussion, while worth discussing, do not concern the results of the present paper where the focus is on predicting fabric evolution under different specified strain fields per se, not its coupling to ice flow.

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?

We agree the fabric evolution due to basal-slip deformation derived from the continuum is similar to that produced by the Taylor hypothesis, and this could have some effect on the values of the parameters (ι, β, λ). The equation comes from assuming a linear dependence on D (the strain-rate tensor) as Placidi (2010) does. The term [Dijnj – Djknknj] is then valid for any plastic spin induced by deformation and is not necessarily linked to the Taylor bound but appears in other fields, such as fibres rotating in a flow (Dafalias, 2001).

The continuum framework also allows us to include the effect of migration recrystallization on the fabric. We note that no other fabric evolution model has been able to reproduce the detailed features seen in experiments (which also occur in the natural world), even full-field models which are much more computationally expensive.

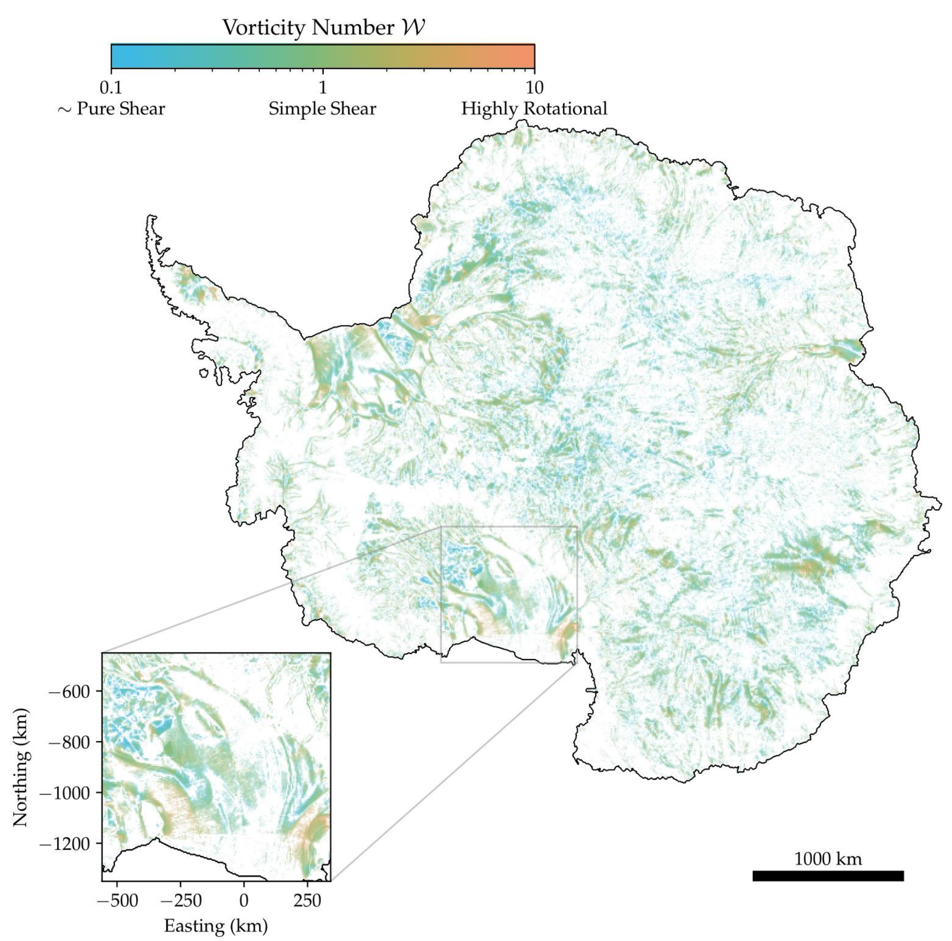
We agree that care should be taken on interpretating the contribution of different recrystallization mechanisms on the grain-scale from the model parameters, as the parameters represent the contribution to the change in the distribution function and are do not directly correspond to grain behaviour.

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?

Thank you for highlighting this. The vorticity number is calculated based on the 2D horizontal velocity gradients derived from the observations of surface velocity, even though dw/dz can also be calculated from the surface velocities due to incompressibility as you say. As other derivatives (du/dz, dv/dz, dw/dx, dw/dy) cannot be estimated but are likely to be non-zero, hence we decided not to include dw/dz as it would underestimate the vorticity number. The derivatives are found using second order accurate central differences on the original grid resolution and then averaged over a 10x10 block as described in section 2.2.2.

The figure below shows the surface vorticity number including the calculated contribution from dw/dz, which makes very little difference.



- 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.

Thank you, we will correct this reference to 2006. Migration recrystallization as represented here is a 4th order process, so cannot be represented by frameworks solving for the 2nd order orientation tensor. If an evolution equation for the 2nd order orientation tensor is derived by taking the 2nd moment from the CAFFE fabric evolution equation, the term for migration recrystallization depends on the 6th order orientation tensor. Furthermore, the 2nd order orientation tensor does not contain sufficient information to distinguish between ODFs produced by migration recrystallization (such as cone shapes or secondary clusters) and simpler fabrics such as single maxima, due to the limited information.

We note further that migration recrystallisation requires the temperature-dependent pre-factor β to have been defined to be used in simulating fabrics. A new development in SpecCAF (Richards et al. 2021) was to provide this through a regression analysis with laboratory data. This, in addition to the solution providing the full ODF field, allows the important process of migration recrystallisation to be implemented.

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.

Thank you for highlighting this, we will clarify the strain γ here. The deformation is the same as defined in eq (10) in section 4.1. The principal axes are orientated at 45 degrees relative to the to the x and z (out of the page) directions of the pole figure. We will define the strain and grad u earlier to avoid confusion.

- Sec. 3.2 : give the expression for the computation of the strain (\gamma) from the strain-rates.

We define the strain-rate in Section 4.1, as above to avoid confusion we will define it earlier.

- Page 9, last line : *« Furthermore the pre-factor »,* I'm not such which pre-factor ?

We mean the factor of sqrt(2)/2, we will clarify this in the text.

- 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.

We are happy to change this.

- Line 209 : give the definition of the J-index before using it.

Thank you for highlighting this.

- 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.

We believe the reviewer has slightly overestimated the scope of SpecCAF. SpecCAF is limited to fabric evolution, and we make no attempt here to compute the stresses (through a viscosity formulation). As it is purely a fabric evolution equation, it can in principle be combined with a variety of viscosity formulations should one wish (see below).

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.

SpecCAF could be combined with either the Static viscosity formulation or the viscosity formulation from the CAFFE model. When we comment on Martin et al. (2009), we refer only to the fabric evolution part of the model and not the viscosity formulation. We will be sure to clarify this in the text.

We agree it is an interesting open question whether a co-linear (or other alternative viscosity formulations/homogenisations) could produce the syncline patterns seen in Martin et al. (2009).

References:

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