1st review

We again thank the reviewer for her helpful and thorough review of the manuscript, especially her suggestion of a parameter sensitivity investigation. We have implemented all these changes, detailed below in purple, and believe the manuscript is much better now thanks to these changes. We have added these purple comments to our original (blue) "reply to reviewers comments" to show where we have made changes in the manuscript.

This paper presents some simulations of ice fabrics in conditions relevant for the Antarctic ice sheet. The simulations are made by means of a numerical model inspired by the work of Placidi et al. (2010), that simulates the rotation of individual ice crystals included in a orientation mixture that is submitted to a given strain field, and includes a parametrisation of the effect of dynamic recrystallization on this rotation. This model has been applied recently in Richards et al. 2020 (EPSL) to reproduce laboratory observations.

1. We wish to thank the reviewer for her thorough review of the manuscript and numerous helpful comments. We understand the rationale behind the comments raised by the referee and provide responses to each below. In a number of instances relating to the nature of the model, we acknowledge that more explanation in the present paper is warranted and should be incorporated in a revised version. We also argue here that certain concerns raised by the reviewer are not rendering the model as invalid or inappropriate for the first order analysis and prediction of ice fabrics that this contribution is focussed on. We also assure the reviewer that the model has been tested by direct comparison with experiments, propose more quantification of confidence in the model, and clarify the motivation for focusing here on 2D flow.

Please find our comment-by-comment responses below. To summarise our responses:

a. We agree that all assumptions underlying our model need to be clearly stated. This can be satisfactorily addressed through added discussion in the paper and through reference to the existing experimental validation in Richards et al. 2021. In particular, we emphasise the validity of the assumptions is demonstrated by reproduction of experimentally produced fabrics (Richards et al. 2021). Please see below for details. **Discussion of the assumptions has been added extensively in section 2.1.1 which is almost entirely new.**
b. We do not agree with the referee's comments that the model makes a Taylor type assumption. This has been discussed before in Faria (2008). The model does not attempt to simulate individual ice crystals, only the evolution of the orientation distribution function. However, we acknowledge that this discussion is important and easily addressed in revision. **Clarification of this has been added in new subsection 3.1**
c. We agree that a focus on 2D deformations with constant deformation history, limits direct application of the simulations provided to a complete interpretation of fabrics retrieved from ice cores, and this was not our intention to convey. Nonetheless the presented results provide a necessary stepping-stone towards such an application. In a revised manuscript we will emphasise this notion in the motivation for our work, highlighting the importance of analysing fabrics in more complex conditions in the future. The primary motivation of this paper is instead to use the already validated fabric model to take a first step away from the isolated conditions of pure and simple shear, to identify new properties of fabrics occurring continuously across linear space between them (as well as to rotational deformations, which lie on the same spectrum indexed by

the vorticity number). While the model can accommodate general 3D and changing deformation/temperature history, this is beyond the intended scope of the solutions we intend to present in the current paper, as the parameter space is too large to explore within the scope of a single paper, yet can be incorporated and explored in subsequent work. To clarify this, we propose changing the title to ‘Ice fabrics in **two-dimensional** flows: beyond pure and simple shear’ alongside more clarification in the text of the reasons for beginning with 2D deformation, and also the rationale for considering surface velocities of Antarctic for basic motivation (see below). **Alongside changing the title, we have added 2 new figures, showing the vorticity number from a 2D vertical cross section of ice at a divide (Fig 3) and over a bump (Fig 4) respectively. Based on these new figures, section 2.2.2 is almost entirely new to highlight that the investigation presented in our manuscript can be motivated by the range of 2D deformation regimes occuring through the depth of a vertical cross-section of ice. In addition, we are now explicit that we are limiting our analysis to 2D deformations (as shown by the title)**

This paper suffers from a lack of clear explanation of the strong assumptions that are included in the numerical simulations and the associated parametrisation.

Such assumptions, that I detail below, can have a significant impact on the results, and, since they are not clearly stated, they are not tested either, and this undermines the credibility of the study.

2. The model was presented, calibrated and tested in Richards et al. (2021) (EPSL) in direct comparison with laboratory experiments. This provides validation of the approach and exhibits predictions (such as secondary clusters observed in simple shear) that have not been successfully predicted even by previous detailed microstructural models. In a revised version of the manuscript, this fact will be emphasised more explicitly.

We have now emphasised this in section 3.1, around line 233.

- It would be first necessary to recall the way the strain and stress interactions between grains are dealt with in the model.

3. It should be noted that the explicit modelling of grains or grain-grain interactions is not applicable to the model used in this contribution. The rationale of the continuum model is mathematically similar to the Navier-Stokes equations, which do not attempt to represent the motion of fluid particles, but instead describe the spatial average of a bulk of quantities describing them at a larger scale; this is the basis for all continuum approaches, and we are applying here the same principles for fabric modelling – in this regard the model does not neglect the microstructural interactions per se (e.g. it does not make any assumption of uniform distributions of stresses at the microscale, see below) because their mean emergent bulk effects are encapsulated by the model parameters that we have rigorously constrained empirically through direct comparison with laboratory data. All parameterisations in the model are formulated to represent the change in the ODF (orientation density function representing the fabric), not specific individual grain behaviour.

The success of our validation against experiments, including its ability to reproduce fabric structures that have not been predicted even by complex discrete models, shows clearly that the general continuum modelling approach taken is indeed justified, though we do understand that a more thorough discussion of the nature of the model and its assumptions, is helpful to include.

Again, we wish to reiterate that we do not wish to refrain from a clear explanation of the nature of the model, and we will therefore endeavour to clarify the model validation and the rational of an continuum model better in a revised version of the paper, and more clearly, alongside references to additional details and intended scope (see below).

As mentioned above, we have now clarified the assumptions of the model in the new section 2.1.1. We have also clarified SpecCAF's place in the hierarchy of fabric models in the new section 2.3

Unlike stated in Richards et al. 2020, the model, that derives from previous works of Faria et al. (2006-I,II,III), assumes an homogeneous strain rate, meaning that each crystal is submitted to the same strain rate. This hypothesis, apparently not clearly stated in any of those works, has been shown by Gagliardini (2008) in its response to Faria et al. (2006) to correspond to a Taylor-type of approximation, meaning uniform strain.

4. Faria (2008), in his reply to Gagliardini's comment, showed that the theory does *not* make a Taylor-type approximation. In essence, Gagliardini draws a false equivalence between averaging over grain-to-grain interactions up to polycrystal quantities in Lebensohn et al. 2004, and averaging operators over the abstract orientation space. The theory proposed in Faria et al. (2006-I,II,III) does not impose any constraint whatsoever on the deformation of individual grains. Please see Faria (2008) for a more detailed discussion of this point.

We have now added an explicit clarification of the discussion between Gagliardini and Faria in section 3.1, paragraph 3.

Such an approximation can be clearly recognised as such, and then it is possible to evaluate its impact on the simulation of the mechanical response of the polycrystal, as done by Castelnau et al. (1996). In particular, Castelnau et al. 1996 showed that this approximation was not satisfactory for a highly anisotropic material such as ice since it requires the activation of non-basal slip systems at a non realistic level. By doing so, it strongly reduces the level of strain heterogeneities between crystals, the latter being the main driving force for dynamic recrystallisation. We can expect this approximation to impact the modelling of this mechanism.

Since Castelnau et al. work, it appeared clear that in situation where the full stress and strain field heterogeneities can not be taken into account, an homogeneous stress approximation is more adapted to simulating the mechanical response of ice (see maybe, for instance, the work of Pettit and co-authors).

In Richards et al. 2020, it is mentioned that the fact that the model considers a large number of grains for each orientation specie, reduces (or annihilates) the dependency on the grain orientation on the mechanical state and response (strain and stress). Gagliardini (2008) showed, based on Lebensohn et al. 2004 work, that this is not true and that only the dependency on the neighbourhood is reduced by considering many grains for each orientation.

5. We appreciate the comments here as they have highlighted that we need to extend the explanation of the model and include more details from Faria (2008).

We have now added an extensive clarification of the discussion between Gagliardini and Faria in section 3.1, paragraph 3.

- The way the dynamic recrystallization is simulated is also based on important assumptions, not always in agreement with laboratory or field observations. It would be necessary to explicitly mention these approximations, and justify their use.

6. Thank you, we agree that the explanation of assumptions should be elaborated on in revision. Justification is provided by validation against existing laboratory experiments.

As mentioned above, we have now clarified the assumptions of the model and the nature of the continuum approach in the new section 2.1.1

First, in the main part of ice sheets, where temperature and strain rates are low, the main recrystallization mechanisms is continuous (or rotation) recrystallization, characterized by a low driving force for grain boundary migration (see for instance De la Chapelle et al. 1998). In such a regime, the fabric is supposed to evolve only slightly owing to recrystallization, and to remain mainly dominated by deformation (see also Montagnat et al. 2012, for the Talos Dome core).

It would therefore be important to evaluate, in some appropriate locations, the relative influence of the simulated rotation recrystallization versus migration recrystallization in the obtained fabrics. If migration recrystallization, the way it is simulated here, has too much weight on the resulting fabric in location where rotation recrystallization is expected to dominate, the model can be questioned.

7. We agree, this analysis of the contribution of different recrystallization mechanisms would be useful. At low temperatures our model also predicts a fabric mainly produced through deformation, in agreement with the referee's comment. The relative importance of the recrystallization mechanisms and its implementation in the model using dimensionless parameters is explained extensively in Richards et al. 2021. In a revised version a summary will be provided.

We have added a new parameter sensitivity analysis in the supplementary materials (discussed in more detail below) which examines the sensitivity of the results to changes in the parameters. We haven't added a specific investigation into the effect of rotation recrystallization vs migration recrystallization. Such an investigation is not in the scope of the current contribution. In addition, the effect of rotation recrystallization is to diffuse i.e. weaken the modelled fabric but is not to change the actual pattern generated.

In areas where migration recrystallization dominates (high temperature / high strain rate), the grain boundary migration kinematic dominates the softening process, so that the fabric and microstructure end up resulting from the stress state, and loose track of the deformation history (see what happens at the bottom of the GRIP, NEEM, Dome C ice cores for instance, or also in high shear conditions, Hudleston 1977 for instance, or even Hudleston 2015, see also Alley 1992). Can we expect, in such conditions, an evolution of fabric with strain?

8. Laboratory experiments (Qi et al. 2019, Journaux et al. 2018, Craw et al. 2018, Piazolo et al. 2013), performed at high temperatures (>-10C) and very high strain-rates, clearly show an evolution of fabric with increasing strain. See Fig. 5 of Richards et al. 2021 (EPSL) for a collation of these experiments plotted against strain.

- Second, concerning the physical mechanisms. Migration recrystallisation is supposed, in the presented model, to be governed by a "deformability" related to the total deformation accumulated in the grain.

Dynamic recrystallization mechanisms (nucleation and GBM) are related to the local accumulated dislocations in the form of geometrically necessary dislocations (responsible for local misorientations), and GNDs are not correlated with the total amount of strain experienced by the grains. It has been recently shown by Harte et al. 2020 for Ni-based alloy by coupled EBSD observations and Digital Image Correlation strain measurements (stored energy is different from cumulated strain).

In various experiments performed on ice, or full-field modeling, it was shown that there is no relationship between the amount of deformation (measured by Digital Image Correlation for instance) and the Schmid factor of a grain. There is therefore no "hard grains", or "soft grains", since the local behavior is much more controlled by the grain interactions and the resulting stress redistribution. The uniform strain assumption neglects this aspect too.

9. While in the continuum model approach taken the modelling of migration recrystallization includes assumptions and simplifications, the model (as noted above) is not aiming to simulate each grain or grain to grain interactions, but rather the mean effect of migration recrystallization on the orientation distribution function. Therefore, the representation of processes in the model should not be expected to correspond to grain behaviour, but rather their bulk mesoscopic representation (as represented by the dependent variable evolved by the model, the ODF). We again highlight the fact that in Richards et al. (2021, EPSL) the model was shown to predict the distribution function of fabrics from experimental results, indicating that these assumptions are justified. The model also predicts detailed features such as secondary clusters in simple shear, which even full-field approaches such as Llorens (2016) have struggled to reproduce. This is evidence that these assumptions are justified in terms of capturing the essential effect on the distribution function. We also note that, as stated above, the uniform strain assumption on grains does not apply, and we will make sure to clarify this better in revision

We have incorporated the above in the new sections 2.1.1 and 2.3

My point of view concerning these approximations made relatively to dynamic recrystallization is that they can be useful and justified in the simplified numerical modeling approach used in this work. Nevertheless, it has to be clearly mentioned that they ARE approximations, and their effects should be tested.

10. In light of the referee's comments, we realise that we need to be more explicit in this paper about the assumptions of the model. Testing of the model was already conducted in the previous paper Richards et al. 2021 (EPSL), but we agree that a more thorough discussion of assumptions is warranted here and can be easily incorporated in revision.

This is added in sections 2.1.1 and 2.3

- The way the boundary conditions are selected is very unclear to me. Considering that fabric is being formed during deformation in depth of the ice sheet, how can a surface velocity map be representative of the in-depth flow conditions? Can the authors be clearer about that?

11. We do not aim to match deformations to the conditions of flow in ice sheets exactly, but rather to use the surface velocity data as motivation for investigation of a range of vorticity numbers away from pure and simple shear. We thank the referee for highlighting that this was unclear in the paper, and can be addressed in revision. Please see below our response to comment No. 24 for details.

We have added 2 new figures, showing the vorticity number from a 2D vertical cross section of ice at a divide and over a bump respectively. Based on these new figures, section 2.2.2 is almost entirely new to highlight the results can be motivated by considering a 2D cross-section of ice, and that we are limiting our analysis to 2D deformations. We have also updated the surface vorticity number plot to include estimates for du/dz, dv/dz from the SIA approximation 25% into the ice-sheet, so the vorticity number shown in (now Fig 5) can now be said to be estimating the fully 3D vorticity number near the surface of the ice-sheet. Nevertheless, we have updated the manuscript to be explicit that our analysis is primarily motivated to help with the analysis of 2D cross sections through ice (as shown on Figures 3 and 4).

The 2D approximation is also strong. It was shown by the Elmer-Ice community to be OK in the case of specific types of flow, like divides (where there is little divergence or convergence). Can it holds for more complex situations such as fast ice streams? What effect could it produce on the fabric evolution? This should be justified and tested.

12. We are using the 2D approximation only as a stepping-stone to explore new fabric patterns and features beyond and intermediate to 'pure shear' and 'simple shear' (and to rotational deformations, which lie on the same spectrum). This is a deliberate choice for the scope of the present paper as a focus on a well-defined continuous space of fabrics indexed by a single parameter *W* (the vorticity number) and temperature *T*. In principle the model could be extended to more general deformations, but this is not the aim of this contribution, and would require more parameters to classify (e.g. an extra parameter representing the relative importance of vertical shear would be a natural next step). In this regard, two-dimensionality is not an approximation or limitation, but a focus to allow systematic and controlled exploration of a new research question as an initial step in the exploration of ice fabric evolutions. We appreciate that the title of the paper and the abstract may have suggested otherwise. Considering the comments by the reviewer we propose softening these statements, and to incorporate the words "two-dimensional" into the title.

We have updated the title to Ice fabrics in *two-dimensional* flows: beyond pure and simple shear. We have also included 2 new figures showing the vorticity number through a 2D vertical cross section of ice at a divide and over a bump respectively, showing how a continuous variety of vorticity numbers appears through the depth of an ice sheet. Based on these new figures, section 2.2.2 is entirely new to highlight the results can be motivated by considering a 2D cross-section of ice, and that we are limiting our analysis to 2D deformations.

- What "highly-rotational" conditions represent "in reality"? Does that correspond to area where a block of ice rotates freely on itself? Can that happen in the depth of ice sheets? If yes, where?

13. An ice block rotating freely on itself corresponds to a vorticity number of infinity. Vorticity numbers greater than 1 are in general common, and the surface velocity data is a way of illustrating that. As an example, for flow around a cylinder the vorticity number will be greater than 1 in the region directly above and below the cylinder, and this situation is typical for flows involving obstructions and junctions. The identification of the essential form of fabric arising in this limit is a novel result of the present work.

- About the capacity of the model to predict steady-state fabrics. Steady-state fabrics depend strongly on the mechanical state the ice is experiencing, and the flow history. I therefore don't understand how could the model be realistically predictive considering the strong assumptions made (1) on the mechanical state (Taylor-type of approximation) and (2) on the recrystallization mechanisms.

14. As explained in our responses no. 3 and 4., the Taylor type approximation does not apply. Furthermore, the model is validated against experimental results.
See the response in purple just below.
In order to test the predictability of the model, it would be necessary to test how robust it is to variations in the parameters, and to the 2D approximation, and to the use of surface velocity vorticity. Such a robustness test was already missing in Richards et al. 2020.

15. We agree that this would be a useful addition to this paper, and we are currently working on this and will add this into the discussion soon, with a view towards adding this as a supplement. It is worth noting that with the results, especially the steady-state analysis, we are not seeking to draw conclusions based on precises values but on the general pattern and change with vorticity number and temperature, and we would not expect this to change with variations in the parameters.

We have added this parameter sensitivity as a supplement. To do this we have taken the parameter fit from the inversion from both compression and simple shear in Richards et al. 2020 (rather than just simple shear as before) and calculated the 80% and 95% confidence intervals around this – shown in a new Fig 6. We have then reproduced the figures with the strongest possible fabric (maximum basal-slip deformation and migration recrystallization parameters, minimum rotation recrystallization parameter) and weakest possible fabric. This is then used to reproduce all the main figures in the results. We thank the reviewer for this suggestion.

In light of the parameter sensitivity investigation we decided to slightly modify our the focus of our results relating to the strain to reach steady-states. Rather than plotting steady-state time based on 90% of the convergence, which we found to be sensitive to parameter variations due to their effect on the tight criterion at which a steady state is reached, we instead report the halfway-strain to reach steady-state. This can be thought of as a half-life for fabric evolution, as explained in section 4.3.

Specific comments:

- Abstract: "a definitive classification of all fabric patterns". This sentence lacks humility... in particular owing to the lack of clarity of the text regarding the assumptions made (see my comments above), and their effects on the obtained simulation results. On top of that, the 2D simulations highly limits the ability to provide this full classification, and also the fact that strain states were deduced from surface observations, very likely not relevant for flow in the depth of the ice sheet.

16. As mentioned in our introductory statement, based on the reviewers comments we appreciate that the limitation to 2D deformations limits the applicability to general ice cores. Therefore, we propose changing the title to 'Ice fabrics in two-dimensional flows: beyond pure and simple shear', rewording this sentence, and clarifying the immediate caveats towards any direct application to interpretation of ice cores in our discussion.

We have updated the abstract to remove this sentence and highlight our focus on 2d deformations.

“Highly-rotational fabrics... produce a weak fabric”. Can we expect a fabric to produce a fabric? Not clear to me.

17. We apologise for this typo. It should read “Highly-rotational deformations... produce a weak fabric”.

We have corrected this

- Part 2.1: The presentation of the processes made in this part is simplistic regarding the many other observations and analyses that exist in the literature (see my comments above). It is OK if it is clearly presented as assumptions made to simplify the processes and better introduce them into the modeling approach. It is a very classical approach to simplify the physics in order to be able to take it into account in a modeling approach. But it needs therefore to be clearly stated, justified, and tested when the results are presented.

18. We agree with the reviewer that more explanation of the model, and the underlying theory, would be helpful, and we are happy to address this in detail in revision.

We have added this in section 2.1.1

What is the “real situation” responsible for some “rigid-body rotation”?

19. See our response no. 13, in general this arises from any vorticity in the flow-field.

- Part 2.2: Various studies were done in the past that include torsion and compression, or shear and compression, and therefore that consider a more complex scheme that pure or simple shear. None of them are mentioned in part 2. I can suggest Budd et al. (2013), Duval 1981 for instance, but others are mentioned in Hudleston 2015.

20. We thank the reviewer for mentioning these papers which we will include in the literature review (in the introductory part as well in appropriate locations in the discussion section.

We have added reference to these papers (also Jun et al. 1996) in section 2.1.2, including reviewing their results (paragraph 3 of this section). We have also compared the fabric patterns of Budd et al. 2013 to those predicted by Fig 11 in section 5.1 paragraph 5, with general agreement.

At domes, in fact close to domes since deep ice cores are never exactly at the dome location, if girdle is observed it is that not only compression occurs, but also lateral extension. This can signify that the core was cored slightly on the flank, or that dome has moved with time (see for instance NEEM, Vostok, EDML, NorthGRIP). For nearly every deep ice core drilled close to a dome, a shear component was observed close to the bedrock, that participated to strengthen the single-max fabric (see for instance Talos Dome).

21. Thank you for this insight which can be used as an alternative motivation for exploring conditions away from pure and simple shear. We aim to use this paper to provide a clear exploration of these conditions, like laboratory experiments provide for compression/simple shear.

Interestingly, this can be clearly seen in our new Fig 3. where the region where W=0 is only seen very close to the divide.

Can we consider ice deep in the ice sheet to be fully unconfined?

22. Do you mean fully confined? As we have limited our analysis to this. As stated in 21. and 12. we are not aiming to fully represent ice sheet deformations but provide a systematic look at deformations away from pure and simple shear.

Please cite Gusmeroli et al. 2012 for sonic measurements of fabrics.

23. We will add this. **Now added in Section 2.1.2, paragraph 4**

- Part 2.2.2: How do you extrapolate surface velocity measurements to get access to in-depth flow history? What are the limitations? Where can it be used, and where it can't, and why?

24. Ice shelves form near-plug flows in which the surface velocity represents the velocity throughout the depth of the ice flow. Ice streams also form near-plug flows and will likely be two-dimensional in their flow properties except for flow close to the base, where basal conditions may generate localised three-dimensional flow. We have done some quick calculations with the shallow ice approximation with no-slip at the base, for ice flowing down an incline and n=3. These suggest the ice will remain at greater than 90% of the surface velocity to a depth of around 56% into the ice sheet. In all situations, the surface velocity provides the leading-order *direction* through the depth of the flow in accordance with all standard thin-layer regimes of ice flow (shallow ice, stream, or shelf), but can be subject to vertical shear. Therefore, while our motivation (which we will better clarify in revision) is nonetheless primarily to indicate deviation from pure and simple shear *per se*, it is in fact reasonable to expect surface velocity to imprint on the depth of an ice sheet flow to a certain depth (in many cases, such as ice shelves and ice streams, almost the full depth); this point was left largely implicit and can be detailed more explicitly in revision. Exploring the effects of vertical shear on the fabrics we have determined in this paper would make for an interesting extension that would encompass a broader range of ice-sheet flow deformations, with the analysis here a necessary first step.

We have updated the figure showing the vorticity number of Antarctica to include estimates for the vertical derivatives. This is done by assuming the vertical profile is following the shallow-ice approximation. We then estimate the derivative down to a depth of 25% into the ice-sheet. We also calculate dw/dz by continuity. Dw/dx, dw/dy can are small by order of magnitude analysis so this figure now represents and estimate for the 3D vorticity number up to 25% into the ice-sheet. Compared to the previous figure, including the vertical derviatives makes vorticity numbers closer to 1 and reduces the calculated error. In regions where there is basal slip, the shown vorticity number will be valid deeper into the ice sheet.

- Part 3: See my comment above, please provide here the main assumptions that are made in this model, from a mechanical point of view (how are the mechanical strain and stress field distributed in the microstructure, what is the flow law considered, how are the interactions taken into account, what are the boundary conditions, etc...), and from a physical point of view (what are the assumptions made to formulate the recrystallisation mechanisms, and why).

25. Please see above for a discussion of the model assumptions. It is worth noting again that the idea of strain/stress in a microstructure, and explicitly describing flow laws and grain-grain interactions do not apply. We are happy to provide more discussion of the assumptions in the model.

In relation to this comment, section 2.3 clarifies the position of SpecCAF in the hierarchy of modelling approaches, and we clarify that the model is a fabric evolution model only.

Some assumptions made, like the parametrisation with the deformability for instance, or the one for the temperature effect, are very strong and very likely control the results. It would be clearer to emphasise them and test their relative impact.

26. As stated in our response no. 15 above we will test the variability of the temperature parameterisation in a supplement.

See the supplement where we test the results against variations in the parameters as functions of temperature. We also hope we have but more emphasis on the assumptions implicit in the model in sections 2.1.1 and 3.1.

As it is presented, it appears to me as if the model was a parametrisation of the rotation of crystals, under homogeneous imposed strain, and not a mechanical modeling (such as Elmer-Ice or VPSC) able to provide interactions between the stress and strain field and the fabric evolution (see Martin et al. 2009 for instance).

27. Please see our response no. 3 & 4 noting the statement 'a parametrisation of the rotation of crystals, under homogeneous imposed strain' does not apply. The referee is correct in stating that the model presented here does not involve coupling between fabric evolution and the flow field, rather the fabric is solved from an imposed velocity gradient and temperature. A full coupling is not the intention of the present paper and is not required to address questions of what kinds of fabrics arise from given deformations. It should be noted in comparison to Martin et al. (2009), where second-order orientation tensor representations of the fabric are used, the model presented here is able to model much more detailed features in the distribution function than is possible than in this previous approach.

We have clarified the model does not represent 'homogenous imposed strain' in our discussion of the Taylor type approximation (section 3.1).

- Part 4: the limitations associated with the 2D formulation are not mentioned. Can it be applied in every stress and strain configurations considered? See my comment above.

28. See our response no. 12. We agree more explanation would be useful and we are happy to provide this.

In line 28 we mention that the 2D formulation is a first step away from pure and simple shear. At the start of the discussion we have added a paragraph discussing the limitations

- Part 4.3 and discussion: to my point of view, in order to test the robustness of the results presented, the authors should provide results within which the parametrisation is modified, and the effect of the assumptions made tested. In particular, the steady-state obtained is highly dependent on the way the recrystallisation is modeled, on the parameters that control the effect of temperature. By changing them slightly, are the steady-state still reached in the same conditions?

29. See our response no. 15

We have added a parameter sensitivity study in the supplement. We have revised the steady state figure (and consequently the discussion) to show the halfway time to steady-state, rather than the steady state time which the reviewer rightly predicted would be sensitive to parameter variations. As

can be seen in the supplement the halfway time is not sensitive to changes in the parameters. Because of this we have changed the discussion in section 5.2

- Part 5.2: I don't think that the model can be, as it is, predictive in terms of relation between finite strains and steady-state fabric owing to the fact that it neglects the complexity of the deformation history along flow lines, that it considers a homogeneous state of strain. Taylor-type of approximation, by neglecting the strong anisotropy of ice, very likely underestimate the fabric development rate (see Castelnau et al. 1996). It therefore seems to me hardly transferable to ice core interpretation.

30. We again highlight the fact that the model has been shown to successfully predict the fabric development of experimentally produced fabrics in both endmembers of pure and simple shear. However, we agree that assuming a constant deformation history restricts direct application of the simulations we show in this paper to ice core interpretation. Within the scope of the present paper, we do not wish to model a real ice flow history, just to show the evolution from an isotropic fabric towards final steady states, and to document the properties of the final steady states, as a demonstration and first order analysis.

We have added a discussion of the Taylor-assumption and clarified the model does not require this assumption in section 3.1. We have also added an experimental pole figure to Fig 7. (from Qi et al. 2019), to highlight the good agreement between the model and experimental fabrics.

Instead of citing Faria et al. 2014, please refer to some of the original work that deserve the credit, since Faria et al. 2014 is a review.

31. Thank you, we will be sure to change to the earlier papers. **We have replaced this with a reference to Schytt, 1958 on line 437**

- Part 5.4:

Before Minchew et al 2018, you could refer to Russell-Head and Budd, 1979, Alley 1988, Van der Veen and Whillans 1990, etc...

32. Thank you for highlighting these highly relevant additional references (to be incorporated in a revised version of the manuscript). **This has been replaced by a reference to Alley 1988 (line 542)**

By the way, the work of Minchew et al. 2018 seems to contradict the hypothesis of an evolutive effect of migration recrystallization, and go in favor of the fact that the fabric, in conditions where this recrystallization regime is dominant, is dominated by the state of stress (also mentioned by Alley 1992). Indeed, it shows that in shear zones, the fabric is very rapidly steady.

33. It should be noted that Minchew et al. 2018 does not model the fabric, but rather infers the enhancement factor by making a series of assumptions. They find values of between 6 and 10 for the fabric enhancement fit their model. This variation of 40% of the enhancement factor leaves plenty of room for the development of the fabric to continue to take place. We also note simple shear experiments at high temperature (Qi et al. 2019, Journaux et al. 2019) show fabric development with strain at such high temperatures. These laboratory results require far fewer assumptions to reach this conclusion.

2nd review

We again thank the reviewer for their helpful and positive review. Based on the discussion below, we have added much more explanation of the model and discussion of the assumptions required for it. This is in section 2.1.1, 2.3, and 3.1. As mentioned below, we have emphasised the fact the model agrees well with laboratory experiments by including an experimental pole figure into Fig 7 (previously Fig 5). We have added below in purple comments to our original "reply to reviewers comments" to highlight where and what changes have been made in the revised manuscript.

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.

We have added more comparisons to experiments in Fig 7 (previously Fig 5), by adding an experimental pole figure. We have also compared the fabric patterns of Budd et al. 2013 to those predicted by Fig 11 in section 5.1 lines 406-411, showing general agreement.

We have also clarified the model setup by clarifying the assumptions in section 2.1.1, clarifying the models place in the hierarchy of ice models in 2.3, and adding more discussing of the model theory in 3.1.

We have also included a parameter sensitivity supplement, exploring the sensitivity of the results to changes in the parameters based on 80% confidence intervals (Fig 6.).

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.

Reworded to: Basal-slip is generally dominant in ice (Duval et al., 1983), although non-basal slip systems are active, especially at high temperatures (Chauve et al., 2017).

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.

We have added reference to Budd and Jun in section 2.1.2, including reviewing their results (lines 71-80). We have also compared the fabric patterns of Budd et al. 2013 to those predicted by Fig 11 in section 5.1 lines 406-411, with general agreement.

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.

We have referenced Kamb 1972 (line 99 and Fig 1. caption). We have also referenced Budd et al. 2013 as it contains an example of pure shear.

l.113: I'm unsure what W >∞ 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> ∞, which we will be sure to correct.

This part has been removed.

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.

We have changed this section to reference the sections in which we answer the open questions, as suggested 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.

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.

In section 3.1, before the equations we have added a discussion of how the processes in section 2.1.1 are represented in the model, along with other discussion on what assumptions are implicit in the model

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.

We have added an experimental pole figure from Qi et al. 2019 to this figure (now fig. 7)

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.

It should be noted that the resolution in the updated figures is 50x50, we have decided to keep the boundaries as they are. We have added this information to the figure captions.

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.

We have added this sentence to section 5.3: Furthermore, we have shown that the presence of a fabric which appears to be isotropic could be indicative not only of no deformation, but also of highly rotational deformation regimes.

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.

We have added an extra clarification in the text (section 5.3) : "other constraints such as knowledge the deformation regime history or temperature has been constant to good approximation"

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. **Added to the start of the results, section 4.1**

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.

We have this into the conclusions highlighting that double-maxima only persist for W=0: "We show that for two-dimensional deformations, the double-maxima fabric is not present at high strains when only a small amount of vorticity is present in the deformation regime W>0.1. This is important as many laboratory experiments are performed for W=0. Future work could investigate whether this conclusion extends to three-dimensional cone-shape fabrics."

Based on the comments by the first reviewer, we introduced a parameter sensitivity study (figure 6). The time/strain to full steady-state was found to be sensitive to variations in the parameters so we have instead plotted the halfway time to steady-state (a kind of fabric half-life). Because of this, we have reduced our discussion of steady-state fabric strains.

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. **These have all been corrected**

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3rd review

We again thank the reviewer for the helpful and thorough review of the manuscript, especially the suggestion of a parameter sensitivity investigation. We have implemented all suggested changes, detailed below in purple, and believe the manuscript is much better now thanks to these changes. We have added in purple comments to our original "reply to reviewers comments" to show where we have made changes in the manuscript.

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. **To address this, we have included a new section 2.3 that provides discussion of the model's place in the hierarchy of ice modelling approaches. Clarification of the discussion between Faria (2008) and Gagliardini (2008) has been included in Section 3.1. We have also clarified that the model is for fabric evolution only in 2.3 (last paragraph).**

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. **We have now changed the title to "Ice fabrics in two-dimensional deformations: beyond pure and simple shear" in order to clarify the focus. We have also added 2 new figures (figures 3 and 4), showing the vorticity number from a 2D vertical cross section of ice at a divide and over a bump respectively. Based on these new figures, we have included a new discussion in section 2.2.2 to highlight that the results can be motivated simply by the diversity of deformation regimes occurring through the depth of a 2D cross-section of ice alone, and that we are limiting our analysis to 2D deformations.**

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.

We have changed our background motivation, and introduced new figures 3 and 4, showing the vorticity number through a 2D vertical cross sections from an ice divide and flow over a bump, to highlight that we are motivating our analysis of two deformations primarily by the 2D vertical cross section (see summary point b above). We have also updated our analysis of the surface vorticity number to include the vertical derivatives (discussed in more detail below) so now it can be said to represent the 3D vorticity number (rather than just the surface) down to a depth of 25% into the ice-sheet.

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.

We have removed the statement about curved streamlines.

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.

We again thank the reviewer for pointing this out. Based on this comment we have adjusted our motivation to primarily focus on the 2d vertical cross section (summary point b). We present fig 3 showing the range of vorticity numbers at an ice divide, going between 0 and 1. We also show ice flowing over a bump (a modification of Exp F from Pattyn et al. 2008) to highlight vorticity numbers >1 (figure 4).

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.

Considering this helpful comment, we have updated the plot of vorticity numbers in Antarctica to include dw/dz (calculated using div.u =0) and the du/dz and dv/dz (calculated by assuming a shallow-ice approximation velocity profile with n=3 and calculating the derivative in the top 25%). Accordingly, the vorticity number shown in this plot is three-dimensional and is valid for the top 25% of the ice-sheet. This is now explicitly stated/clarified in the figure caption and text.

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.

(see description of revision above)

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).

We thank the reviewer for this suggestion, and have clarified this extensively in section 3.1, 3rd paragraph

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.

We have clarified that SpecCAF is designed to numerically model fabric evolution only in section 2.3, paragraph 3

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).

We have added the following clarification at the end of section 3.1: "Despite the model not including the Taylor hypothesis, the term for basal-slip deformation in the equation below is similar to that which would be derived from a Taylor homogenisation of ice under a simple basal-slip only model (Gagliardini et al.,2009), with the exception that the rate of viscoplastic deformation can vary relative to rigid-body rotation."

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.

We have also clarified this in section 3.1: "However, care should be taken in attributing our calibrated parameters as applying specifically to grain-grain interactions rather than the bulk interactions representing their net statistical effects in the model."

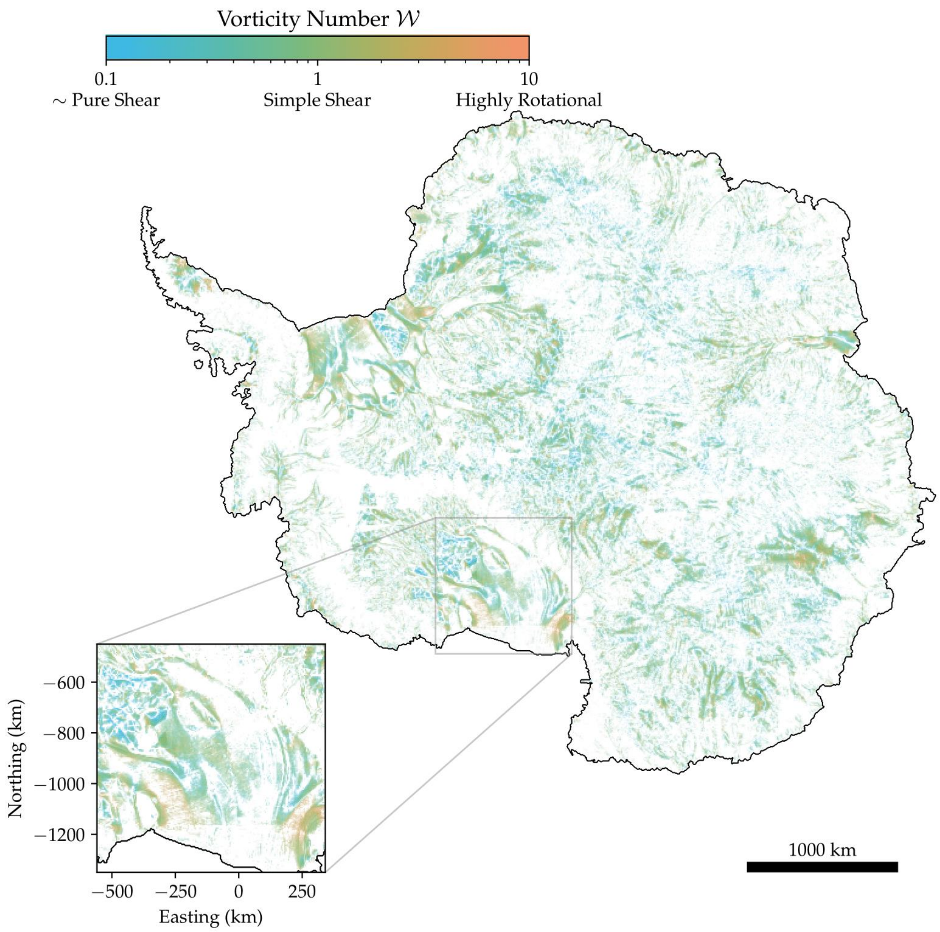
I have few other detailed comments listed below:

- 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?

As mentioned above, we have updated the plot of vorticity numbers in Antarctica to include dw/dz (calculated through div.u =0) and the du/dz and dv/dz (calculated by assuming a shallow-ice approximation velocity profile with n=3 and calculating the derivative in the top 25%). Accordingly, the vorticity number shown in this plot is three-dimensional and is valid for the top 25% of the ice-sheet.

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.

We have corrected this reference, and added the above points in blue into our almost entirely new section 2.3

Sec. 3.2 : explain what is *γ* 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.

We have clarified that the strain we use is the effective strain, and defined the principal axes in the caption.

- 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.

Added as eq. 8

- 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.

This has been removed as we have redefined the strain-rate earlier, so the pre-factor is not needed

- 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.

The pole figure single maxima is now aligned at the vertical direction as we have included the experimental pole figure alongside.

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

Thank you for highlighting this.

Done

- 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).

We have clarified that SpecCAF is for fabric evolution only in section 2.3, paragraph 3

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).

We have updated this sentence to hopefully avoid any confusion that Martin et al. Are using a continuum model: "Martín et al. (2009) has coupled a fabric model to an anisotropic viscosity, but the fabric evolution model used did not include recrystallization and temperature dependence.

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