“Ice fabrics in natural flows: beyond pure and simple shear”

by Richards et al.

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.
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.
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).

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.

- 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).

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.

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).

- 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.

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.

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 revisions.

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.

- 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.

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.

- 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.

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.

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.

“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”.

- 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.

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.

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.

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.

- 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.

- 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.

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.

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.

- 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.

- 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

- 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.

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.

- 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).

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.