Ice fabrics in two-dimensional flows: beyond pure and simple shear

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Collated Response to Reviewers

Our responses are in blue throughout.

Review 1: Ed Waddington

I was invited to comment on this manuscript late in the review process. I have read version 3 of the manuscript, and the authors' reply to the previous reviews.

1.1 Author responses to prior reviews

Previous reviewers of this manuscript have identified several points that needed to be addressed.

One of those points for clarification was the concept that SpecCAF does not directly model crystal-level processes such as dislocation densities, slip on basal planes, recrystallization, or crystal-crystal interactions. The authors have clarified that SpecCAF is an empirical continuum model for the evolution of (x; t; n), the mass fraction of grains at position x at time t with c axes directed into a solid angle dn around direction n.

Slip on basal planes, rotational recrystallization, and grain-boundary rotation are all incorporated in principle as continuum processes based on gradients in the continuum description, i.e. SpecCAF is essentially an empirical model with what might be called a model shape set by equations (3), (4) and (5), and the coefficients and on the terms are set empirically by comparison with fabrics observed in samples whose deformation histories are known or understood.

To me, this seems to be the same in principle as choosing to fit an exponential shape to a data set, where the data determine the prefactor and the exponent. Choosing a good shape and a good training data set are key to establishing a good t over a wide range of circumstances.

Reviewers were concerned that SpecCAF used the Taylor assumption, in which all grains experienced the same strain rate. The authors have clarified that, following Faria et al. (2008), the SpecCAF model assumes only that the material holds together such that individual grains (if they were explicitly followed, which they are not) would merely retain their position relative the surrounding continuum. There is no restriction imposed on how individual grains strain, rotate, or recrystallize, relative to their neighbors; the only restrictions are on their species, defined as other grains with similar orientations.

Reviewers reminded the authors that not all previous lab tests were restricted to pure or simple shear; some previous lab experiments imposed stress and strain patterns that were combinations of pure and simple shear. The authors have incorporated the suggested references in new discussion in Section 2.1.2, and modifed their claim to be the first group to study this.

Reviewers asked how the evolving fabric was coupled to the applied strain-rate elds. The authors have clarified that they are not yet coupled; that is a goal for future work.

In my view, this paper is a commendable analysis of evolution of fabric (CPO) in 2-D under a wide range of temperature T and ow regime as characterized by a vorticity number W in Equation (1). In my view, the paper is suitable for publication in The Cryosphere, pending minor revisions that can be negotiated with the scientific editor.

We thank the reviewer for the detailed consideration of our manuscript and positive comments. Please find our description of minor revisions below.

1.2 My questions

The previous reviewers are all clearly experts in anisotropic fabric development and ice-sheet flow, and I think they have done a good job of identifying technical issues and concerns. While I have some familiarity with the field, I will address mainly the likelihood that the manuscript will speak effectively to colleagues and students who are not as well-versed in the topic as the reviewers. Since these points are less germane to the scientific integrity of the manuscript, and more germane to the readability and potential readership, I expect that you can discuss with the editor the rigor with which you should follow them.

Page 1, Line 24: What is meant by a uniform spectrum?

We mean to highlight by this that we are exploring the continuous space of deformation regimes as vorticity number increases, rather than the isolated cases of pure and simple shear. In light of this comment we have reformulated this part to be clearer:

“The objective of the present paper is to use the fabric evolution model SpecCAF (Richards et al. 2021) to take a step away from the isolated conditions of irrotational deformation and simple shear where the model has been validated, and explore the continuous space of deformation regimes lying between these cases. We also extrapolate to deformation regimes more rotational than simple shear.

Effective strain rate is introduced in Equation (8), where it is defined in terms of the strain-rate tensor, which is derived in turn from the velocity-gradient tensor. However, strain itself just appears without an explanation in the caption for Figure 7. This may be a concern, because the manuscript deals with some large finite strains that may not be simply related to the history of strain rate. It is not obvious how (or if) the strain rate is integrated over time to get the strain. I assume Lagranian or Eulerian finite-strain tensors are involved?

Are the finite strains in SpecCAF calculated in a way that is compatible with the calculated finite strains from lab tests, and inferred from Antarctic data sets such as Figure 8?

We calculate the strain by computing the integral of the strain-rate with time, with the strain-rate defined as in eq (8). Different measurements are used by different groups doing laboratory experiments, but they can be converted to an effective strain for comparison with the work here by using a scaling factor. We have added a paragraph on this after eq (12) (where we define the strain as explained here)

For example, in lab tests to large finite strains, the shape of the sample changes significantly, and even if the applied force or the applied stress is held constant, the strain rate is time-dependent. Is the strain history inferred directly from snapshots of the shape, rather than from integrating the strain rate?

Within the context of SpecCAF, the strain can always be evaluated as the integral of the strain rate over time. Within the context of experiments, the strain is achieved by fixed movement of pistons. For example, during uniaxial compression, the sample shape changes and therefore the strain rate will not be constant. However, the finite strain for each deformation step ("snapshot") can always be correctly retrieved from experiments. As such results are compatible.

In the Antarctic Ice Sheet, the vertical strain rate is inferred from the horizontal velocity divergence through continuity, and then is assumed to be uniform through the upper 25% of the depth. How is the strain profile then calculated for ice as it moves downward?

Figure 5 was computed to illustrate the variability of vorticity number at first order across the ice sheet based on simple assumptions. For Fig 5 of the Antarctic ice sheet, we are calculating the vorticity number of ice flow using the surface deformation field. this calculation a conservative estimate as we assume no slip at the base. We are not using this figure to calculate any strain profiles. We agree it would be interesting to use this model to track ice parcels through an ice sheet.
We have now emphasised the purpose and assumption of the figure in the text and figure captions.

Equation (2): At first reading, it was unclear to me whether the mass fraction rhostar(x; t; n) was a (dimensional) mass, or a (nondimensional) fraction. I figured out that it must be a mass, because it integrates to (x; t), which appears to be a dimensional mass, rather than integrating to unity over the sphere; however, perhaps that could be made clearer to help your readers avoid an interruption in smooth reading.

We agree that a clarification of this is helpful. We do introduce it as a mass fraction in (2), and then non-dimensionalise it in section 3.3, so that, for the rest of the paper, it is a nondimensional fraction. To better clarify the distinction between the mass fraction and its non-dimensional version, we have introduced a new symbol $f^* = \rho^*/\rho$ to denote the non-dimensional orientation distribution function and use this throughout (previously we had continued to use the same symbol following a non-dimensionalisation).

In order to help me read the paper more efficiently, I made a table of variables with definitions and notes about where they first appear. I expect that such a table of variables would be helpful for other readers, and could increase the readership of the paper.

We have added this as Table 1.

Exploring the full range of two-dimensional responses to two-dimensional loading is an important step, and I think the authors are making a useful contribution. However, I also expect that minor perturbations in that two-dimensional flow may create fabrics that generate instabilities causing growing nonzero strain rates and ow in the third dimension. This is a question that could also motivate further work.

Yes, this is an interesting comment

1.3 Copy editorial points and clarity

Line 44: The author's name is Takeo Hondoh, so the reference should be simply Hondoh, 2000.

In the References section, at line 643, the citation should be Hondah, T., Nature and behavior . . . i.e. only the initial, to be consistent with all the other references.

Thank you for pointing this out, we have corrected this.

The manuscript uses vector notation, indicial notation, and the summation convention, but does not explain these concepts from continuum-mechanics to readers who may be unfamiliar with them. While a couple of dozen or so people in the community will understand what you are doing, this oversight is liable to dissuade other readers (such as new graduate students) from reading beyond equation (1). A couple of sentences could rectify this.

We have added a paragraph after eq (1) to explain this:

"As a note for people unfamiliar, in Eq (1) we have used both summation notation W_{ij} and vector notation $\boldsymbol{W}$. W_{ij} is a 2nd-rank tensor (shown by the number of indices) and the operation $W_{ij}W_{ij}$, indicating summation over the repeated indices, is the tensor inner product $\boldsymbol{W}:\boldsymbol{W}$.

There appears to be some oversight or misunderstanding about the difference between maximum and maxima. Maxima is a plural word meaning (if we were to purge the latin forms), maximums. Just as it makes no sense to talk about a single maximums, it makes no sense to talk about a single maxima, or a single-maxima fabric.

The expression a double maxima is also problematic, because it could be interpreted to mean four or more peaks. A double maximum more clearly indicates two peaks.

The top row in Figure 1 has it right - single-maximum fabric, and double-maximum fabric.

This has been corrected throughout the document

Line 240: What is meant by fully resolved experiments?

We agree, this was unclear and replaced with "laboratory experiments for which parameters such strain, deformation rate, and temperature are known".

Figure 5: To my eyes, there appears to be a slight change in the character of the vorticity number inside a ghost circle at 80 degrees South. Is this a relic of the Pole hole caused by polar orbits that turn at 80 South? Does this affect the quality of the data shown?

This is correct, Mouginot et al., 2019 use a different method to derive the surface velocities within this circle. This figure has been updated based on comments by other reviewers and the circle is no longer visible.

With regard to data quality effects: all the data is coloured by the relative error estimated from averaging over a 10x10 block, so any low-quality data should be coloured as white (I.e. not visible)

I will spare you a complete line-by-line list of other grammatical suggestions; however, I hope you will see the merit of checking throughout the text for other examples of these points. Making your text easier to read can only enhance your readership numbers.

We have gone through the document to check over the grammar and spelling throughout

The English language is fraught with many rules that often don't appear to make a lot of sense, and there are differences of opinion among groups who have differing communication aims, such as journalists, popular-media editors, poets, novelists, and scientists; however, some rules can eliminate ambiguities and make scientific text easier to read. The following points address recommendations on using hyphens and strings of ideas, in order to make your text more accessible to readers, and therefore helping you to create a more easily understandable, and ultimately more memorable and important paper.

Hyphenation:

A hyphen should be used in a compound adjective (an adjective and a noun) that modifies another noun, e.g. line 15 and elsewhere – ice-flow dynamics.

A hyphen should not be used between a stand-alone noun (subject or object) and an adjective that modifies es the noun, e.g. line: 10 and elsewhere - no hyphen in strain scales. e.g. Figure 1 caption and elsewhere - no hyphen in (d) shows a single maximum with . . .

Generally there should be no hyphen after an adjective or adverb that ends in 'y' . e.g. line 9 and elsewhere - highly rotational

We appreciate the reviewer taking the time to make these suggestions and have gone through the document to correct this.

Lists:

When comparing a string of ideas in text, the ideas are easier for readers to grasp quickly when they have equivalent and parallel grammatical structures.

For example, in the Abstract,

The use of our model in large-scale ice flow models as well as for interpreting fabrics observed in ice cores and seismic anisotropy, introduces two ideas, but the first is written as the phrase *in ice flow models,* while the second is written as the clause *for interpreting fabrics observed in ice cores and seismic anisotropy*.

Can you rewrite both ideas as phrases, or both ideas as clauses, i.e. neither or both should contain a verb form?

We thank the reviewer for this suggestion. We have corrected this to "The use of our model for addition to large-scale ice flow models and for interpreting fabrics observed in ice cores and seismic anisotropy will provide new tools supporting the community in predicting ice flow in a changing climate." We have also gone through the document to improve the use of language throughout.

Page 1, line 4

. . . in both compression and simple shear, . . . is unclear. Do you mean . . . in both pure shear and simple shear, . . . ?

We have corrected this to unconfined compression and simple shear – as the experiments we compared against were performed at these conditions. We hope this clarifies the precise deformational regime. We didn't say pure shear as this is understood to be a 2D deformation.

You are describing deformational regimes in terms of strain rather than stress. While there can be compressive stress in all directions (pressure), there can be no volumetric compressive strain for incompressible ice (neglecting elasticity). Perhaps as a community we are sloppy in our terminology, by calling it a compression test when we set a weight on top of an ice slab, because that slab experiences compressive deviatoric stress on one axis, but extensile deviatoric stress on other axes. (While we can't change the world, we can each make our own writing clearer.)

It would be better to choose one wording, then stick with that throughout the manuscript. (I think the text gets it right later at line 21.)

We have gone through the document and have removed any reference to 'confined compression' or '2D compression' instead only referring to pure shear. However, we keep the term uniaxial compression to refer to compression along a single axis with the other 2 axes then experiencing extension, as this term is used as such in the literature.

Review 2: Sergio Faria

Dear Editor and Authors,

The manuscript uses the numerical model SpecCAF to simulate and classify crystallographic preferred orientations (CPOs) generated by a wide range of two-dimensional deformation regimes. It is a follow-up of [22]. The text is well written and self-contained. The work has good scientific quality and presents interesting results. I enjoyed reading it. There are however, several clarity issues that require careful revision. None of these issues affect the main results and conclusions of the work, which I recommend for publication after revision. **Specific comments:**

Lines 20–21: To be fair, the most studied deformation regime to date in relation to ice fabrics has been uniaxial (vertical) compression, probably as much or even more than pure and simple shear.

We have added this.

Line 24: It would be nice to explain why "ice flow is commonly modelled in the two dimensional $x - z$ plane", and which $x - z$ plane is chosen.

Added: One way to model ice flow is to simplify it to the two-dimensional x-z plane (along the flow direction and the vertical, e.g Martin el al. 2009). This is done to understand vertical variation and compare to ice core profiles.

Line 26: Delete the spurious "below".

Done.

Lines 28–32: Concerning the four open questions to be answered: The first two have been considered by [15] through the combination of theory with experimental extrapolation. The third question is unclear: which "steady state" do you mean? Strain rate steady state? Stationary CPO? Some other kind of steady state?

Regarding Kamb, we have added a paragraph in the introduction reviewing this paper. We have clarified the third question (and combined it with the fourth): "Third, how do fabrics evolve at very high strains which have remained inaccessible to laboratory experiments, and at what strain does the fabric reach a steady state?"

Lines 42–43: Personally, I find the term "crystal slip" a bit misleading and recommend replacing it with "intracrystalline slip", or even better "dislocation glide" (and climb, if you wish to be general; [10]). If you want to keep term "crystal slip", then please make clear what it means (as it stands, I can only guess). Also "rigid-body rotation" sounds slightly misleading, since the material under consideration is not a rigid body. Better would be simply "rigid rotation".

We have referred to dislocation glide instead of crystal slip. While we see the point of the reviewer that rigid body rotation may be misleading, it is the term used in the microstructural community and therefore we would like to keep it.

Lines 42–43: Assuming the established definition of recrystallization as "the formation and migration of high-angle grain boundaries driven by the stored strain energy" [2, 7, 9, 13], it follows that migration and rotation recrystallization are not deformation mechanisms, but rather annealing phenomena. Admittedly, recrystallization of any kind is closely related to strain, being driven by the stored strain energy and affecting the mechanical response of the material. Nevertheless, recrystallization is not a deformation mechanism per se, since it cannot produce strain (change in shape) or rigid rotation in a stressed body [10, 18, 19, 24, 26]. Migration recrystallization describes the motion of grain boundaries *through* the material (i.e., without material movement). Rotation recrystallization describes the formation of a new grain boundary. In this respect, it is worth mentioning that some authors confuse cause and effect by erroneously attributing a material rotation to "rotation recrystallization": Actually, the material rotates by a deformation mechanism like dislocation glide and climb, and the strain energy stored in the material by this rotation triggers rotation recrystallization, which is the formation of a new grain boundary. The fallacy that recrystallization phenomena were deformation mechanisms is an epidemic pseudodoxy perpetrated by unreliable sources.

We have reworded this to "As ice deforms, the fabric evolves both through dislocation glide along the basal plane, which causes c-axes to rotate, rigid-body rotation which simply rotates grains around the rotation axis, and recrystallization processes which rearrange the grain boundary network.

Line 43: Insert "in ice" after "slip".

We have reworded this section, however we have taken care to clarify that basal-slip deformation dominates in ice only.

Lines 60–63: The references cited here are not the most suitable. For instance, Piazolo et al. [20] is a very interesting work, but it refers only to transient creep in laboratory and simulations, and it would be reasonable to argue that stress and strain heterogeneities may disappear after the transient phase. As it turns out, that is actually not the case in practice, rather the contrary. Kipfstuhl et al. [16, 17] have observed strong strain heterogeneities in shallow and deep polar ice, while Faria et al. [6] explained those stress/strain heterogeneities through the concept of "a highly strained mantle and a less strained core within a grain." As for the diffusion/dispersion of c-axes by rotation recrystallization, Godert [12] presents a model that simulates the concepts and observations made by previous researchers, while the original concept can actually be traced back to Poirier [21], which was popularized in ice by Alley [1].

We thank the reviewer for suggesting these helpful references. We have changed this section to:

The second recrystallization process is *rotational recrystallization*. This occurs when dislocations recover into subgrain boundaries which, with increasing strain, will develop into grains (Drury et al., 1985). These dislocations tend to be concentrated closer to grain boundaries due to stress heterogeneity, observed in shallow and deep polar ice (Kipfstuhl et al., 2006, 2009) and which can be thought of as an ice grain having a stressed outer `mantle' and a less stressed inner `core' (Faria et al., 2009). Therefore, new grains developing from subgrains, dominantly occur near grain boundaries. The orientation of these new grains is similar to, but slightly different to, the parent grain. With increasing strain, the difference in orientation tends to increase (Halfpenny et al. 2006). This randomisation of orientation acts to diffuse any concentrations in the fabric (Alley, 1992).

Lines 66–67: Radar should be mentioned here as well (it is mentioned only later, on Line 92).

We have added this

Line 75: The correct citation is "Li et al., 1996". The surname is "Li", the given name is "Jun".

We have corrected this.

Line 78 and elsewhere: The plural expression "single maxima" is repeatedly misused in singular contexts in many points of the text. The singular is "single maximum" and its plural is "single maxima". Please do not mix them up.

This was mentioned by another reviewer as well, and we have corrected this

Figure 1: Please indicate the principal directions of compression and simple shear. The pole figure (d) is incorrect. The primary cluster should be closer to the centre and the secondary cluster closer to the border of the diagram, at approx. 70° from the primary cluster [1, 15].

We have plotted (d) this way so that the principal directions of deformation do not change across the subfigures. It is rotated 45 degrees from 'normal' simple shear. We have clarified the principal directions in the caption

Lines 104–105: The 45° is a theoretical estimate, because observed angles are less than 45° due to the continual rotation of c-axes towards the main compression axis.

We have changed this to: “This process acts to consume grains orientated towards the compression axis and, on its own, grows grains orientated in a ring 45° away from the compression axis (the orientation easiest for basal slip and hence likely to be with the least dislocations). Therefore, the balance of basal-slip deformation and migration recrystallization produces a girdle pattern, with an angle always 45° due to the interaction between the two processes”

Lines 110–114: The explanation for the imbalance in cluster strengths seems a bit confusing. The main reason for the imbalance is neither the “vorticity” in simple shear (i.e. gradual rotation of the principal strain axes), nor recrystallization. Rather, the imbalance is mainly derived from the fact that, for simple shear, the secondary cluster is unstable, whereas the primary cluster is stable. In other words, c-axes in the primary cluster stay there, while c-axes in the secondary cluster quickly rotate away from it by usual strain-induced lattice rotation. If migration recrystallization were causing the imbalance, more recrystallization would imply a weaker secondary cluster, which is contrary to observation (the secondary cluster actually gets weaker when there is less recrystallization). The function of migration recrystallization is to make the secondary cluster more defined, by consuming the grains with c-axes that rotate away from it and move towards the principal axis of compression (“hard-glide orientations”). The “vorticity” of simple shear generally plays a very minor role, since it is much slower than the effects of c-axis rotation and recrystallization.

The c-axes of the secondary cluster do rotate away from the orientation preferred by migration recrystallization. But this is caused by a combination of vorticity and basal-slip deformation. If you consider the velocity gradient we use:

$$\nabla u = \begin{pmatrix} 1 & W \\ -W & -1 \end{pmatrix}$$

We are keeping the deformation tensor constant and adding vorticity to move from pure to simple shear. So for the case of pure shear, there is a balance between basal-slip deformation rotating c-axes towards the compression axis, and migration recrystallization consuming crystallites towards the compression axis and producing them at 45 degrees.

With the addition of vorticity using the velocity gradient above, there is no rotation of the deformation axes. So the only change is the addition of vorticity: here the primary cluster remains stable as vorticity acts to move c-axes away from the compression axis, so it balances basal-slip deformation. For the secondary cluster the vorticity acts in the same direction as the basal-slip deformation

While indeed migration recrystallization makes the secondary cluster stronger, in our view it is important to isolate as much as possible the effect of increasing vorticity from any rotation of the deformation axes. To make this clear we have re-worded this part to:

“This pattern is similar to a double-maximum but the presence of vorticity in simple shear causes an in-balance in cluster strengths. For the stronger, primary cluster the vorticity acts to move c-axes in the opposite direction to the basal-slip deformation, resulting in a stable position. For the weaker, secondary cluster the vorticity and basal-slip deformation both rotate c-axes towards the compression axis."

Line 118: It could be mentioned here that Kamb [15] related ice fabrics to deformation regimes using a somewhat related measure, which he called the "stress character".

This has been added.

Figure 2: Please be consistent and use either "rigid rotation" or "pure rotation", but not both.

In this paper we are using these to refer to separate concepts: rigid (-body) rotation refers to the effect of vorticity on the fabric, pure rotation refers to a deformation condition which is solely rotational. However we have updated the caption to clarify, replacing pure rotation with "a purely rotational deformation"

Figure 4: Why have you amplified that much the Gaussian bump? I am afraid that the high vorticity numbers reported in this figure may be derived from such an extreme amplification of the bump.

We have modified this figure so that the height of the gaussian bump is unchanged (a tenth of the flow domain height) compared to the ISMIP benchmark. We have only reduced the width of the gaussian. Upon reflection, the height of the bump in the previous figure was unrealistic but we believe that the presence of sharp bumps or features is likely common at the base of ice-sheets.

Figure 4: Why have you chosen $n = 1$ instead of $n = 3$ in this example? Intuitively, one would expect a realistic modelling of ice flow with high vorticity numbers to use the non-Newtonian description with $n = 3$. What would be the effect of $n = 3$ on the vorticity numbers in this simulation?

We have updated this figure to use n=3, as the reviewer points out this is more representative.

Figure 5: This figure intrigues me. Maybe I misunderstood it? I have doubts about the use of shallow ice approximation at the transition from grounded ice to ice shelf...Besides, we know from detailed modelling and ice-core observations that the dominant deformation regime for ice shelves is non-rotational, asymmetric horizontal extension; not simple shear as indicated in the map.

The shallow ice approximation is just used to introduce some vertical shear and makes the vorticity number lower, giving a conservative estimate. Therefore, we say that for regions of grounded ice the vorticity number shown is valid to 25% depth, for regions with greater basal sliding it should be valid to a greater depth. We have added this explanation in the text.

We agree that extension is present in these locations, however this has been seen in combination with a simple shear component (with the shear plane perpendicular to the flow direction) e.g. (Lutz et al., 2020) for the Ross ice shelf.

However, based on your comments we double-checked this figure and saw that it disagreed with measurements in some locations as you say, e.g. the Amery ice shelf. We have updated it by recalculating the velocity gradients based on differentiating the averaged values rather than the local ones, and this gives results more in line with what is reported and would be expected. Consequently, we have also updated the text in referenced to this.

Lines 238–239: That is correct indeed. At this point I have to digress to do something that I very rarely do—because it causes me great displeasure—which is to correct erroneous statements by another reviewer. In this particular case I feel obliged to do so, to rectify harmful and unfair criticism to the work under review. The unfair claims by the Reviewer are:

> *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.*

There are several errors in that statement. First, the Reviewer cites a comment by Gagliardini [11], but fails to cite the subsequent response [5] that proved the falsity of all Gagliardini's comments.

Second, it is true that the SpecCAF model is ultimately based upon the theory of Continuous Diversity developed by Faria et al. [3, 4, 8], but the Reviewer's claim that the theory of Continuous Diversity assumes a homogeneous strain rate for each grain (so-called "Taylor-type approximation" or "uniform strain") is clearly fallacious: it represents a complete disregard for the fundamental principles of continuum mechanics.

The theory of Continuous Diversity (CD) describes the large-scale ("macroscopic") flow of a glacier or ice sheet. As any other continuum theory, all fields and gradients in the CD theory are spatially defined on that large scale, which is many orders of magnitude larger than the grain scale. Therefore, just as the strain rate in fluid dynamics does not impose any constraint, hypothesis or approximation on the motion of individual molecules, the strain rate in the theory of Continuous Diversity does not impose any constraint, hypothesis or approximation on the deformation of individual grains: every grain is free to deform as inhomogeneously as needed. In plain mathematical terms, if $\mathrm{d}x$ defines an infinitesimal distance in the continuum (upon which all spatial gradients, including the strain rate, are defined) and D is the average grain size, then $\mathrm{d}x \gg D$.

We thank the reviewer for supporting our line of reasoning here.

Figure 7: Please explain the grey arrow in the figure caption.

We have added to the caption: "The grey arrow shows the secondary cluster transposed from the pole figure to the γ - θ diagram."

Figure 7: In the caption, please replace "principal axes of deformation" with "principal strain axes". The former expression does not make much sense when there is rigid rotation.

Thank you for spotting this, we have corrected it.

Line 304: Wrong figure reference. It should be "Fig. 7b", not "Fig. 8b".

Corrected

Equation 10: I am confused here. The non-dimensional velocity gradient defined in (10) does not seem compatible with the non-dimensional velocity gradient derived from the definitions (8) below, for its symmetric and skew-symmetric parts. If they are compatible, please show that. If not, which one are you using in your simulations?

The velocity gradient is compatible. However from this question it is clear it was not sufficiently well explained. Therefore, we have modified the text to define a dimensional velocity gradient first which we then non-dimensionalise. We have also included the symmetric and skew-symmetric parts for clarity. We hope this clears up any confusion that may arise for a reader.

Line 321: I guess you mean −5C, not −10C, right?

Thank you, we have corrected this.

Lines 325–326: The positions of the clusters for W= 1 (simple shear) seem way off from the observed positions in the real world...Why? The primary cluster should be close to vertical (centre of the diagram,$\vartheta = 0$) and the secondary cluster close to horizontal≈-70(at around 70 from the primary cluster, that is, $\vartheta= 70$). Are you rotating the fabric backwards to remove the vorticity and transform the simple shear into pure shear? Please clarify.

Yes, we have rotated the fabric such that the principal strain axes remain constant as we increase the vorticity number. We have clarified this after eq (10).

Line 327: The secondary cluster is consumed by "c-axis rotation", not "migration recrystallization".

Indeed, c-axis rotation rotates these grains to an 'unfavourable' orientation, and at this orientation these grains are consumed by migration recrystallization to grow grains more favourably orientated.

We realize that the wording is misleading. Hence we now write: "At high temperatures with a double-maximum, as W increases the clusters are moved by the rotational component of the deformation. In combination with basal-slip deformation, this results in one stable and one unstable cluster (see section 2.1.3). The c-axes of the unstable cluster rotate under basal-slip deformation and vorticity such that they are at an orientation where they are consumed by migration recrystallization as strain increases, leading to a single-maximum at high strains…"

Lines 329–330: This statement may need revision, depending on the reactions to the comments to Figures 4 and 5 mentioned above. In any case, "prevalent" is a too strong word.

We have changed this to "We also show in Fig. 8 analysis of fabrics produced in highly rotational (W > 1) deformation regimes, which we have shown to occur (Figs. 4,5)."

Lines 335–336: The *J*-index as a stand-alone measure of anisotropy has several problems and is considered unreliable [23, 25]. The former reference proposes the use of an *M*-index based on misorientations. Within the framework of a continuum theory with continuous diversity of the type presented here, the definitions and combinations of various anisotropy indices commonly used in ice-core fabric studies are discussed in [4].

We thank the reviewer for suggesting this, we agree that the M-index is a more useful measure. It could be possible to calculate this directly from the odf, though to our knowledge it has not been done before. However, our attempts to do so were very numerically expensive as the calculation involves integrating numerically over the orientation space twice. This is particularly a problem as to plot the steady state figure in this paper, we need to calculate the J or M index for each strain value and each vorticity number and temperature, so approximately 50x50x1000 times. Because of this we think it is best to remain with the simplicity and numerical efficiency of calculating the J-index. However, we have added some comments on the reliability of the J-index after its definition:

"Although the M-index can also be used to measure fabric strength and may be more reliable (Skemer et al., 2005) the J-index can be calculated very efficiently, enabling exploration of the parameter space used in this paper."

Figure 8: I recommend adding contour lines or colour steps, as in Fig. 9 or 12, because the smooth colour gradations vary on screen and particularly on print, making it difficult to see the oscillations in the fabric patterns.

Done

Figure 10: Same question as before in Lines 325–326. I see an angle close to 30° for the primary cluster for strain = 1 (c) in simple shear (W = 1). Why? Are you rotating the fabric backwards to remove the vorticity and transform the simple shear into pure shear? In real observations (experiment or ice cores) this angle is close to zero. Please clarify.

We have kept the deformation constant, such that it is rotated 45 degrees from 'normal' simple shear.

For a velocity gradient of $\begin{bmatrix} 0 & 1 \\ 0 & 0 \end{bmatrix}$ the deformation tensor is $\begin{bmatrix} 0 & 1/2 \\ 1/2 & 0 \end{bmatrix}$ hence the deformation axes are orientated 45 degrees from the x and y axes.

Figure 11: Why not plotting W=0? Should not double maxima occur close to 0? They already appear at W==0.1 in Fig. 8!

We appreciate the reviewer's suggestion and considered changing the figures but we have decided to keep the scale as it is (0.1 to 10). We have not plotted W=0 as we are using a log-scale, and we do not want to break the scale as this may make interpretation of the figure more difficult. A double-maxima will occur for W=0, however in the limit of very high strains the double-maxima is only stable for W=0 exactly.

Figure 12: This figure is very useful and it should come before Fig. 11.

Done

Lines 392–394: In my opinion, the halfway strain is not very intuitive as a measure of fabric development, because it is normalized by the fabric intensity at steady state. That is, if the steady state fabric is strong, the halfway strain will be larger, giving the impression that it takes longer for the fabric to develop, which is not true, because it may actually develop fast, but it has a long way to reach the "fabric steady state". Therefore, a much more useful measure of fabric development is in my opinion the strain to reach a definite fabric strength. This will tell us how fast fabric develops, which is the information we really need for interpreting ice cores and simulations.

We appreciate the reviewer's suggestion, we tried to plot this but, as there are such different fabric intensities across the space, the plotted picture changes with changing the value of fabric strength chosen. However, based on this we have updated the figure to include a plot of the fabric strength at a given strain of 0.5. This allows comparison between this value and the steady-state.

Line 431: I am not sure what you mean by "cone-shaped fabric"...Do you mean a single maximum or a girdle?

We mean a girdle, and we have changed this throughout to be consistent

Lines 434–435: This conclusion has already been presented by Kamb [15].

We have added this referenced here.

Lines 446–447: That is not stated in the cited work by Jacka and Li [14]. In fact, their results indicate that the mechanical steady state depends on stress and temperature.

This reference has been removed and replaced with (Fan et al., 2020) as an example, where they refer to a steady-state stress at a strain of 0.2.

Line 650: Please correct this reference. The authors' list is wrong and the reference data are incomplete.

Thank you, we have corrected this.

I hope the Authors and the Editor find these comments useful.
Best regards,
S´ergio Henrique Faria

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Review 3: Maurine Montagnat

Please find below the review of the new version of the paper. Since a lot of the comments I gave for the first review were not taken into account, I put at the end of the document the previous review I did, for the editor and the authors to do comparisons. The comments in concerned are underlined in yellow.

One of my concern on this paper is the feeling it gives me of a lack of clarity. Hypotheses are done with the model used here, and this is very fine for me, but many of them are not clearly stated. For instance:

- by assuming the rotation rate of the orientation distribution with equation 5, the authors are doing the assumption of a Taylor-type of mechanical interactions between grains. Indeed, the only terms that act on the rotation rate is the strain-rate (and the vorticity that rotate the full distribution). Another way of doing, that would still be a rather crude parameterisation, would be to follow GilletChaulet et al. 2006 paper (eq 13) and put a stress component into the rotation rate. In this case, the mechanical hypothesis behind the orientation distribution rotation is in between a Taylor and a Sachs hypothesis.

We respectfully disagree with the reviewer that this model constitutes a Taylor type assumption.

We would like to remark here that just adding the macroscopic stress into the macroscopic fabric evolution equation does not entail any constraints on the stress or strain-rate experienced by individual grains (i.e. Taylor or Sachs). In fact, if Glens flow law is used, for the typical parameters used this parametrisation is equivalent to just multiplying the strain-rate tensor by a factor of 1.54. The point being that it is important to be mathematically rigorous with what assumptions are used, and not just say: strain-rate tensor in the equation means Taylor, stress tensor in the equation means Sachs.

We note that this is supported by the third reviewer of this article, quoted below in green.

The discussion about that in part 3.1, lines 230-245, is very deceiving... What is said lines 238-239 appears just wrong to me in a mechanical point of view. And the justification lines 240-245 is really astonishing! One can always parameterise any model to provide the result expected, it does not mean that the good "physics" is in the model!!! Please remove this sentence.

We refer here to the third reviewer's comments, in support of this section (in green):

> **Lines 238–239:** That is correct indeed. At this point I have to digress to do something that I very rarely do—because it causes me great displeasure—which is to correct erroneous statements by another reviewer. In this particular case I feel obliged to do so, to rectify harmful and unfair criticism to the work under review. The unfair claims by the Reviewer are:
>
> > *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.*
>
> There are several errors in that statement. First, the Reviewer cites a comment by Gagliardini [11], but fails to cite the subsequent response [5] that proved the falsity of all Gagliardini's comments.
>
> Second, it is true that the SpecCAF model is ultimately based upon the theory of Continuous Diversity developed by Faria et al. [3, 4, 8], but the Reviewer's claim that the theory of Continuous Diversity assumes a homogeneous strain rate for each grain (so-called "Taylor-type approximation" or "uniform strain") is clearly fallacious: it represents a complete disregard for the fundamental principles of continuum mechanics.
>
> The theory of Continuous Diversity (CD) describes the large-scale ("macroscopic") flow of a glacier or ice sheet. As any other continuum theory, all fields and gradients in the CD theory are spatially defined on that large scale, which is many orders of magnitude larger than the grain scale. Therefore, just as the strain rate in fluid dynamics does not impose any constraint, hypothesis or approximation on the motion of individual molecules, the strain rate in the theory of Continuous Diversity does not impose any constraint, hypothesis or approximation on the deformation of individual grains: every grain is free to deform as inhomogeneously as needed. In plain mathematical terms, if dx defines an

infinitesimal distance in the continuum (upon which all spatial gradients, including the strain rate, are defined) and D is the average grain size, then $dx \gg D$.

[3] S. H. Faria. Creep and recrystallization of large polycrystalline masses. Part I: general continuum theory. *Proc. Roy. Soc. London A*, 462(2069):1493–1514, 2006.

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And please let's assume the choice of the parameterisation as an "OK" hypothesis in order to simplify, especially since there exist no better way of doing so far.

From this choice depends the value of the parameters that have been tuned in Richard et al. 2021 on compression and simple shear cases, and this is fine! Providing it is clearly stated...

- Considering the migration recrystallization mechanisms (in particular lines 264-265), the authors know that what drives them is more complicated that only the "cumulated shear strain"... What drives nucleation and grain boundary migration occurring during dynamic recrystallisation is related to the STORED strain energy, that is related to geometrically necessary dislocations, the ones that help compensating the strain incompatibilities between grains, and their density is not simply related to the cumulated shear strain (some areas that are deforming "easily" cumulate a lot of shear strain and very few geometrically necessary dislocations, so very little stored energy...). Once again, it makes sense, to my point of view, to use such a simplification in a model devoted to large scale flow modelling, but please mention it as an hypothesis and not as the truth!

Firstly, we would like to point out that we never refer to "accumulated shear strain" in the manuscript or in this location, we refer to the resolved shear strain-**rate** acting on the basal plane, such that this deformability acts to quantify "easy" and "hard" orientations. However, we appreciate that this may need further explanation. We have reworded this section to be clearer and to highlight that this is an approximation for the stored strain energy:

"For a given stretching tensor given by $\boldsymbol{D}$**,** for a basal plane with normal $\boldsymbol{n}$ this function represents the normalised strain-rate (or stretching) acting on the basal plane. Therefore, $\mathcal{D}*$ will be greater at orientations where it is easier to slip along the basal plane. Because ice deforms primarily by slip along the basal plane, this is a good approximation for the accumulation of deformation energy in a physical grain, which drives migration recrystallization."

Considering figure 2, for vorticity → infinity, there should be no fabric formed since the material experiences rigid body rotation only? Where could the girdle come from? What constrains the rotation within this girdle, under rigid-body rotation? What is the relative weight of the two parameterised recrystallisation regimes in this weak girdle? And how is it impacted by slight changes in the parameters? Is it robust?

We appreciate the reviewers' questions about the robustness and have added this figure to the parameter sensitivity supplement, and have referenced this in the text: Sensitivity analysis in the supplement reveals no change in the fabric pattern and only a small change in fabric strength, ranging between 1.11-1.23 for the min and max parameters.

Since we can only approximate infinity, exclusive rigid body rotation cannot be reached. Mathematically, the vorticity number is defined as $\mathcal{W} = \frac{O(W^2)}{O(1)}$ i.e. as we approach large vorticity numbers the magnitude of the deformation remains constant (it does not go to 0). Therefore there is always some deformation acting on the ice parcel, even if it may rotate many many times. We also remark in the text "The J-index of this fabric is 1.16, very close to completely isotropic (J=1). It is unlikely this weak girdle fabric would be distinguishable from an isotropic fabric in a physical sample, where the fabric is determined by sampling a limited number of grain orientations.

- Once again, this study lies on parameterisation performed in laboratory conditions, therefore very far from the "real world" it aims at representing. A sensibility study would therefore be necessary, to check, for instance, which of the rotation / migration recrystallization process is dominating and in which situation? Does that make sense with "real world" observations?

What happens when the parameters are shifted away from the linear fit? What is the impact on, for instance, the kinetic to steady-state?

This sensibility study is necessary to check the robustness of the modelling and therefore its ability to be predictive.

We introduced a supplement based on the reviewer similar comments in the previous review, which may have been missed by the reviewer. This contains 10 images exploring how all the figures in the article change based on the confidence intervals in the parameters used.

- Line 411-415: the 2D assumption is strong, and, as already mentioned in my previous review, was shown to give “correct” results only is some specific parts of the ice cores. This part is not an explanation of the limitations of the 2D assumption and the impact it could have on the results, it is more the expression of the authors’ opinion “is a good first step”... It may be, but please, explain us why and under which limitations.

We have added a sentence here explaining the limitations of the 2D assumption: “However, caution must be used when applying the conclusions of this paper to areas with highly three-dimensional deformations, such as curved ice streams or other areas where there is vertical and horizontal deformation.”

<u>More specific comments:</u>

- Line 21: what does "validated" means? When can we consider a model to be fully validated? In particular, this model has not been validated in other deformation regime than simple shear and compression, while you are going to use it in very different conditions.

Thank you for this helpful comment, we have changed this to: “The objective of the present paper is to use the fabric evolution model SpecCAF (Richards et al. 2021) to take a step away from the isolated conditions of irrotational deformation and simple shear where the model has been validated, and explore the continuous space of deformation regimes lying between these cases, and extrapolate beyond to deformation regimes more rotational than simple shear.”

- Part 2.2.1: VERY IMPORTANT!!! This paragraph contains explanations that are contrary to what is known for ice and recrystallization mechanisms. It should be re-written and bibliography may be more correctly used. For instance: **Chauve et al. 2017 do not show that non-basal slip is active! They just show that under some specific conditions, geometrically necessary non-basal dislocations can be observed. It has already been mentioned in my previous review, and since I am co-author of Chauve et al. 2017, it is very important for me that the authors correct it!**

We thank the reviewer for noticing this and have removed this.

The mechanisms described lines 42 and 43 are not stricto-sensus deformation mechanisms. Only crystal plasticity in the list is a deformation mechanism. Recrystallization is a process of accommodation that facilitates the deformation, but does not produce a deformation per-se...

For instance, during post-dynamic or static recrystallization there are a lot of microstructure modifications without any deformation produced. Please modify.

We thank the reviewer for pointing this out and have corrected this:

“T Both the intensity and pattern of the fabric produced is dependent on the conditions of deformation, which will influence the relative activity of different mechanisms. As ice deforms, the fabric evolves through dislocation glide along the basal plane, which causes c-axes to rotate (Steinemann, 1958; Hondoh, 2000), rigid-body rotation, which simply rotates grains around the rotation axis, and recrystallization processes, which rearrange the grain boundary network.

Line 42: Migration Recrystallization does not refer to grain boundary migration. Migration Recrystallization refers to a recrystallization associated with nucleation AND grain boundary migration, on a regime where grain boundary migration is fast. But nucleation does take place also. Above all, migration recrystallization refers to a mechanism that is driven by stored strain energy while grain boundary migration can occur driven by the reduction in grain boundary surface energy. It would be very important to clearly make the distinction and not mis-explain the migration recrystallization regime... Please see Humphreys and Haterly 2004 if necessary.

The reviewer is correct that there is at least to some researchers indeed a distinction between migration recrystallization and grain boundary migration. Within the metallurgical and geological communities the terms are used slightly differently. We have updated the text here “The first is migration recrystallization, which can include a combination of strain-induced grain boundary migration and nucleation (Dorothy et al. 1997).”

Grain boundary migration may be driven to reduce the stored strain energy and grain boundary surface energy of the whole system. In a system that is deforming by intracrystalline slip the driving force for grain boundary migration driven by stored energy reduction 1-2 orders of magnitude higher than grain boundary migration driven by grain boundary surface reduction (Gottstein and Shvindlerman, 1999). As a result of migration recrystallization grains with high stored strain energy will statistically be reduced in number (e.g. Czaplinska et al. 2017).

Line 45: the paper by Chauve et al. 2017 does not allow to say that non-basal dislocation activity is restricted to high temperature regime... This is just that the experiments presented in this paper are at high temperature. And once again, it just observes some non basal dislocations and no non-basal slip activity. So this sentence should be removed.

We realize that this was misleading and hence have removed this.

Lines 51-55: I am really puzzled to read this part, especially when dealing with ice! Recent results have shown (and some done by one of the co-authors, S. Piazolo), that strain heterogeneities in ice can not be resumed relatively to the main deviatoric stress (see Grennerat et al. 2012 for instance), and that strain distribution is very heterogeneous, with strain high in area of low stress, and the contrary... (see Piazolo et al. 2015, Montagnat et al. 2015, Chauve et al. 2017 Phil Trans). Similar observations exist also in other materials. So to say that "strain energy stored in a grain is directly related to the imposed strain" is somehow too vague and may be wrong... The reference given here, Gottstein and Shvindlerman 1999 is a book that I can not access to to verify. It could be stated as a working hypothesis, and justified, but not as the truth.

We respectfully note that, for the second time, the reviewer misquotes the manuscript in their response. We actually wrote "the strain energy stored in a grain is directly related to the imposed ***stress***". However, we can see that this sentence may be misleading. The reviewer is worried about the distinction between far field stresses versus local stresses. In this section we refer to local stresses mainly, this has now been corrected in the section. We also add: "While the local stress axes are highly influenced by the environment around the respective grain and therefore stress and strain are heterogeneously distributed within a polycrystal (e.g. Piazolo et al. 2015, Grennerat et al. 2012), grains with c-axis oriented less favourably for slip relative to the far field stress axes will statistically have higher stored strain energy"

Regarding highlighted previous review comments, August 2021: (*we assume the reviewer felt these were not addressed in our new version*)

Reviewer: 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.

In response to this in the previous review, we added discussion of these assumptions in section 2.1.1 and 2.3. We hope that addressing the reviewers comments above, specifically regarding the formulation of $\mathcal{D}^*$, it is more clear what assumptions are used and what detail the model can represent.

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.

Our previous response:

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 believe that extending the analysis to three-dimensions to test the 2D approximation is beyond the scope of this paper. Starting with a 2D approximation is a well established first step in science.

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.

Our previous response:

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.

Please note: We believe the reviewer may have missed the parameter sensitivity supplement we included?

By including the parameters sensitivity supplement, we have tested how robust the results are to variations in parameters. As discussed above, we believe testing the robustness of the 2D approximation (I.e. extending analysis into 3D) is beyond the scope of this paper.