The Cryosphere: review report of *"Ice fabrics in two-dimensional flows: beyond pure and simple shear"* by Richards et al. (tc-2021-118)

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.

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.

Line 26: Delete the spurious "below".

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?

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".

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.

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

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, Gödert [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].

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

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

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.

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].

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.

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.

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".

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

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.

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?

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.

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 $\mathrm{d}x \gg D$.

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

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.

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

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?

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

Lines 325–326: The positions of the clusters for $\mathcal{W} = 1$ (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, $\theta = 0°$) and the secondary cluster close to horizontal (at around 70° from the primary cluster, that is, $\theta \approx -70°$). Are you rotating the fabric backwards to remove the vorticity and transform the simple shear into pure shear? Please clarify.

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

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.

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].

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.

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 ($\mathcal{W} = 1$). 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.

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

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

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.

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

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

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.

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

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

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