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## Comment on tc-2021-118

Maurine Montagnat (Referee)

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Referee comment on "Ice fabrics in two-dimensional flows: beyond pure and simple shear"  
by Daniel H. M. Richards et al., The Cryosphere Discuss.,  
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"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.

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.

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

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.

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.

- 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.

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.

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?

- 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.

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.

- 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?

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 hold for more complex situations such as fast ice streams? What effect could it produce on the fabric evolution? This should be justified and tested.

- 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?

- 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.

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.

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.

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

- 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.

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

- 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 than 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.

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).

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

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

- 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?

- 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).

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.

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).

- 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.

- 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?

- 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.

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.

- 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...

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.

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