Author’s response letter to Peter Hudleston

Dear Mr. Hudleston,

Thank you very much for your positive feedback. We considered your suggestions to further improve the manuscript. Please refer to our answers below for details.

Kind regards,

Sebastian Hellmann and the Co-authors

Regarding your comment about the model. This is actually a good point that needs to be clarified. We introduced a short discussion at the beginning of our interpretation:

We also calculate the strain rates ε_{xz} from the stress tensor by using Glen's flow law, i.e. Eq. (1) (Table 3). Since the model does not consider anisotropy, the directions of stress and strain are parallel. This is a crucial limitation to be considered in the following interpretation. Especially for shear stress, the model-derived and the actual strain rate directions may differ significantly. In addition, the quantitative numbers deviate from isotropic ice as the aforementioned basal sliding affects the strain rates that an ice grain experiences. For more typical fabrics (e.g. single maximum, girdle fabric), enhancement factors could be introduced (Thorsteinsson et al. 2001, Pettit et al. 2007), but this is too complex for the multi-maxima patterns. However, we regard the modelling output as auxiliary values for our interpretation.

Specific points keyed to line numbers in the text

Line 1. The centroid does not align with the modeled maximum compressive stress for the deepest sample. Perhaps this exception should be noted in the abstract, and the adverb “approximately” should be placed before “align.”

Thank you very much, we changed this accordingly.

Line 100. τ is the second invariant of the stress tensor, not τ^{n-1}

Indeed the exponent should not be in the accompanying sentence. We corrected this error.

Fig. 5. The labels λ1, λ2 and λ3 should also be added to each of this projections, not just the first one.

We adjusted the final figure.

Line 166. I really don’t like the term “fissure” for these features, since it implies an opening, which is not there.

We removed the word here and only keep “fracture traces”.

Line 216. σ_{xz} is the shear stress component, it does not represent simple shear unless the only deformation component in this reference frame is one of base parallel shear strain σ_{xz}.

We used the term simple shear here to distinguish it is not a pure shear. However, this is not fully correct and therefore we followed your recommendation and renamed it to shear strain.

Table 3. The borehole data on strain rate do not provide much useful information. Clearly the errors are large compared with the signal over the limited time between survey measurements. On a short time frame velocities can vary significantly (as the authors note), and this may explain why the model strain rates are poorly constrained by strain rates derived from the boreholes (especially the shear strain rates). The shear strain appears to be most strongly developed in the deepest ice, with less of a gradient in the top part of the profile than the model predicts. This may be due to the fabric related anisotropy and its effect on the flow law.

We agree that these components do not contribute much. Therefore, we followed your suggestions and removed them from Table 3 and the references in the following text.

Line 235. Kinematics – the pattern of movement - is indirectly related to stresses, but in this case there is a close association. The word "causes" here may not be the best one, since a direct casual relationship is not established. "is associated with" might be better.

We rephrased this sentence accordingly.

Fig. 7. The principal stress directions should be labeled $\sigma 1$, $\sigma 2$, $\sigma 3$ on these plots.

We adjusted the final figure.

Line 240. Since this is a new paragraph, it's not clear which observation this refers to. If it's the relationship between fabric and σ_1 at 79m, this is not what Budd and Jacka (1989) show for multi maxima fabrics. The centroid of the fabric is vertical in both Budd and Jacka's examples and the ice at 79m, but σ_1 is vertical in Budd and Jacka, but at a strongly inclined angle to vertical for the ice at 79m.

This was a writing mistake. We considered the general alignment of the two azimuths (eigenvector and principal stress) and therefore Budd and Jacka (1989) are a good source to cite. We corrected the sentence:
The observed azimuthal alignment of the COF with the glacier flow (with limitations for 79 m) is in accordance with results from laboratory experiments […].

Line 250. The ice c-axes tend to become oriented such that the basal planes are aligned for easy glide (have high Schmid factors), which may not be in the ice flow direction. It depends of course on σ_1, which in this case lies in the vertical plane following the flow. In the ablation zone, σ_1 would be close to vertical near the surface and the relationship between flow direction and crystals oriented for easy glide would be different.

The statement should refer to our data and not c-axes in general. We rephrased the beginning of our sentence:
The ice crystal c-axes in our samples generally orient themselves parallel to the ice flow […]

Line 254. Again, this is not the "simple shear" component, just the shear component.

We changed it to shear stress component.

Line 260-263. This is not strictly simple shear, but it does approach simple shear towards the base. You might say "dominated by the shear component, which approaches simple shear." The last sentence of this paragraph is good.

We considered your recommendation to enhance this paragraph:
This implies that the COF for the deepest sample is dominated by the shear component, which approaches simple shear.

Line 266-268. I may disagree here with what you appear to be saying. I believe the single maximum pattern seen in the deep parts of the Antarctic and Greenland ice cores is related to subhorizontal shear strain (close to simple shear) dominating the flow, as first suggested by Gow and Williamson. That is, they are related to shear strain, with the σ_1 direction inclined at some angle to the shear plane by an amount related to the degree of anisotropy of the fabric. I suspect the multimaxima fabrics with clusters arranged about a vertical line in these deep cores have a similar relationship to shear strain and stress that is rather like the situation in your deepest sample. Perhaps this is not in disagreement with you.

We agree with your description, but we do not want to focus on a particular sample (e.g. 79 m) but rather on the general multi-maxima clustering in all depths. The text does not make this fact clear. Therefore, we slightly rephrased it:

If the c-axis orientations would be governed solely by the orientation of the major principal stress and strain direction (σ_1) (mainly a result of compressional and simple shear stress), we would rather expect a single maximum in the stereo plots as in deeper parts of other ice cores (Faria et al., 2014a). As observed in Figures 5 and 7, there is no single maximum. Instead, the individual c-axes in our samples deviate on average about 30° from the principal stress or strain (for 79 m) direction (indicated by black small circle girdles in Fig. 7) and group in several maxima.

Fig. 8. A very nice addition!

Thank you. This was a recommendation of the second reviewer.

Line 296. Not everyone thought that the multiple maxima were artifacts of limited sample size and sampling single grains several times, although this is something Monz et al. suggest may account for many of the early measured multimaxima fabrics. Kamb, in particular, was very careful to overcome this problem by the way he took samples. I'm not sure what you mean here by "method-immanent."

Indeed, your paper Monz et al (2020) with the nice description of the branched grains encouraged us to be more strict. We added an "often debated to be". Method immanent: as (again shown in Monz) the thin sections cannot handle branched grains very well. However, we removed this detail as it is not relevant to discuss here.

Line 299-300. “High strain rate” is a relative term. Strain rates in valley glaciers are typically orders of magnitude smaller than those in most experiments. The key thing that appears to control whether or not multimaxima fabrics develop is temperature. Typical multimaxima fabrics are restricted to “warm” ice, above about -10o. As for strain rate, they appear to form under a wide range of strain rates. Russell Head and Budd (1979), for example, considered they might develop in nearly stagnant ice.

We adjusted this sentence:
The conditions for a “diamond-shape” pattern seem to be suitable in large glaciers like the Rhonegletscher with its high temperatures and large ice flow velocities compared to other valley glaciers.

However, as the Glacier Tsanfleuron in Tisson&Hubbard is also a temperate glacier, but with less obvoius MM-patterns, there need to be another difference than just temperature and we would like to point that out here. As you said in the first review, their glacier is much smaller and has a lower ice flow velocity. Therefore, we refer to this instead of “higher” strain rates.

Line 305. Kamb did not produce typical multimaxima fabrics in his 1972 experiments. They were either double maxima, in simple shear, or a small circle girdle in shear plus compression. Also, Kamb noted that fabric development was mostly related to strain and only weakly to stress. Thus, Kamb may not be the best citation here to support your argument.

According to the second reviewers recommendation, we shortened the discussion and in particular this paragraph was excluded.

Sebastian Hellmann and the Co-Authors

Author’s response letter to Erin Pettit

Dear Ms. Pettit,

Thank you very much for your critical feedback. We considered your suggestions to further improve the manuscript. Please refer to the point-to-point answers for details.

Kind regards,

Sebastian Hellmann and the Co-authors

Specific points:

I still don't see a need for the 3D full stokes model when there is so little data to validate the model output and the model input and results aren't presented in the paper. Without seeing more of the model details and knowing that the model has been validated, I don't trust the subtle variations in stress presented in table 2 without some understanding of the uncertainties in the model. And the estimates of strain rate in table 3 are based on isotropic ice, so they are highly uncertain.
I would highly recommend starting with a simple estimate of stresses. If you would like some examples Pettit et al 2014 which uses a simple flow band model. Or Pettit and others 2007, which combines anisotropic effects with simple flow band. Or go back to Ny 1957 or the basic stress descriptions outlined in Cuffey and Paterson or Hooke or similar textbooks. It seems to me the overall surface flow pattern from the GPS is longitudinal compression along with weaker transverse extension (although without the numbers I can't estimate from the GPS). This should then be accompanied by vertical extension in order to conserve mass. The borehole shows very little shearing, which is expected since it is only the top 75% of the ice thickness. The fact that the maximum eigenvector gets more vertical with depth should be expected because the upper part of the ice column is dominated by normal stresses - with the longitudinal stress being the largest compressive stress and near the bottom, the shear stresses should increase, causing a rotation of the maximum eigenvector. None of that understanding requires the model. It would simplify the paper and minimize the tendency to believe details in a model that is not fully validated.

We understand your point of view. However, we have to insist that the model was not set up from scratch only for our study. This model already existed, and we only used a subversion without time evolution and updated bedrock and ice thickness data (for 2017, the year of ice drilling). For a flow band model or any other model, our investigations in the vicinity of the drilling location are too uncertain (only a few weeks of observations) for setting up a completely new model.
Indeed, we could remove the model and just argue as you propose – surface data show that we have a compression and then increasing shear stress below 60 m from borehole investigations. However, we thought, it might be good to underlay these assumptions with a model available for this glacier. Furthermore, ElmerIce models are state of the art and were used for many glaciers in the last years (e.g. Aletschgletscher, Gauligletscher amongst others). Such a model (even the isotropic version) allows to investigate/consider the large scale ice flow effects that may have an influence on the microstructure.
As a last point, we did not only qualitatively argue, which stresses and strain rates are expected, but want to frame the argumentation/interpretation with quantitative (but auxiliary) values.

According to the second reviews comments, we added some limitations/concerns for such an isotropic model at the beginning of our interpretation section.

The best result/conclusion is that the fabric indeed is pointing in the direction of the most compressive stress and it is not a simple single maximum - that observation alone suggests that recrystallization is happening. I really liked the thin sections showing the bubbles, as I thought that helped frame the story of migration recrystallization. Because these observations don't provide a direct cause-effect story for the multi-maximum - just an association - I am not sure that a page of explanation is necessary and the statement in the abstract claiming that the paper provides "an explanation" In fact the abstract is confusing, as it says there is significant shear stress - but there isn't significant shear stress until near the bed, below the level of borehole.

We added the LASM image. We agree with your argumentation – It shows examples for migration recrystallization. However, we selected a slightly different part of the zoomed section showing some more distinct features related to SIBM-N.
We also reformulated the abstract and exchanged the “an explanation” by “provide indications and suggestions”.
Indeed, the shear stress part is confusing and a leftover from our first version. This part is removed.

The overall citations have been improved, I'd suggest including Pettit et al 2007 and Pettit et al 2011 in the section on how fabric relates to deformation history - there are many others that could be in there too, but the use of sonic measurements allows for a different perspective on the fabric deformation relationship.

We added these papers to our introduction.

Mostly the text has been improved for readability and typographical errors. I still see a few typos (capes instead of caps). The abstract still contains some general phrases that don't provide much information and I'd suggest making being more specific "key information" (which information?) "specific parameters" (which parameters?) "horizontal" should be longitudinal. "good agreement" still needs to be defined in a few places.

We revised the abstract and rephrased these imprecise sentences. Furthermore, we added some numbers to explain the “good agreement” (variations of up to 26° for the colatitude angle ($0° <= \varphi <= 180°$)).

To summarize - I'd suggest cutting out the model (adding in a simple stress description/analysis) and cut text from the long discussion at the end and then maybe add back in the images showing the bubbles and grain boundaries - which I thought was useful for the discussion of the migration recrystallization.

We removed some less relevant details from the discussion to shorten it. In return, we added the LASM images again (although a slightly different part of the scan showing some more distinct features as the one in our first version). However, as said above, we would like to keep the model as it is also a powerful modelling code and followed the recommendations of Reviewer 1 for improving the modelling details although we also understand your concerns.