Dear Peter Hudleston,

We appreciate your constructive and valuable comments to our manuscript tc-2020-133 entitled "Crystallographic analysis of temperate ice on Rhonegletscher, Swiss Alps".
We have considered your typographic recommendations and have provided a point-by-point response to your review comments.

If there are further questions, we are happy to answer them and look forward to hearing back from you regarding your decision.

Kind regards,

Sebastian Hellmann and the co-authors.

<u>General comments</u>

This paper provides a detailed description and analysis of the crystallographic fabric of ice taken from a core from the surface to bedrock in the central part of the ablation zone of a temperate valley glacier. It finds that multimaxima fabrics of the type commonly found in most valley glaciers, usually just from near-surface samples, occur at all depths within the glacier, with some systematic changes with depth in orientation of the clusters that constitute the fabric. This is a new finding and deserves to be published on this basis alone. The paper then, importantly, relates the fabric to the stress field derived from numerical modeling and finds a direct relationship between the orientation of the fabric and orientation of the modeled principal stresses. This leads to a possible explanation of these four maxima fabrics. I question parts of the interpretation and don't believe these fabrics are yet fully explained, as discussed in the specific comments below, keyed to lines in the text. I have also corrected a few typographical errors and made some suggestions for language usage.

We considered the typographic recommendations in the most recent version.
Based on the reviewer comments, we revised the modelling part and recalculate the values from the model. The new results slightly change our interpretation and also fit better to your explanations. We are going to add the actually derived values for the stress components to the interpretation part to improve the argumentation. Furthermore, we will remove Fig. 9 as it may not fit to the improved results anymore.
The recommendations about grammar and language, especially in the first sections are already included (comments like "changed")

<u>Specific suggestions:</u>

Line 9-10. The language here doesn't clearly describe the observed relations, since there are four azimuths and colatitudes that define the fabric and three principal stress directions. It is the centroid of the fabric and the maximum principal stress direction that nearly coincide in orientation.

We changed this sentence:

The centroid of the four-maxima patterns of the individual core samples and the coinciding maximum eigenvector align with the compressive stress directions obtained from numerical modelling.

Line 31. The stress and kinematic conditions in valley glaciers are more complex than just combinations of simple and pure shear.

Changed to:

In contrast, for ice samples from temperate glaciers, the deformation is dominated by a series of interfering and variable compressional, extensional, and shear stress conditions along the flow in the valley.

Line 94. Although the details of the numerical model need not be given here, the basic form of the flow law should be given, since the value of the flow law parameter A is defined. The value of the power law exponent, n, in the flow law should also be noted.

We revised the complete paragraph as a new section and added some more details according to your and the second reviewer's comments. Furthermore, we incorporated most recent data about bedrock sliding and tuned the model accordingly.

Line 117. It is not clear what is meant by fractures here, since there are no actual fractures in this *core. This needs clarification. What are the physical manifestation of the 'fractures?" They must be defined by some combination and bubble or grain size distribution.*

We changed it to "fracture traces" as recommended in your comment for Line 151. However, in other literature we found the term "fissures".

Line 135. Surely this is mm2 not µm2

Indeed, this must be mm2, changed.

Line 151. Here is some information about the fractures. Presumably these patterns are in the form of linear traces in thin section. Following Hambrey I like the term 'fracture traces' for these likely healed fractures.

We changed it to "fracture traces" as a much better name for these features that could be observed in some core depths.

Line 157. You use the term centroid here for the maximum eigenvector on these plots, and state that these are equivalent in the caption to Table 1. Yet in Figure 7 the two are represented and plotted as separate entities. The usage needs to be consistent. In this case how is centroid defined?

We revised the usage of centroid and centre (i.e. midpoint) between the clusters. The midpoint (red dots in Fig. 7) is defined as geometric point between the four maxima (independent of number of grains per cluster). The centroid is affected by the particular distribution of grains and those maxima with a larger grain number attract the centroid. Therefore, midpoint and centroid differ slightly. When calculating the opening angle we considered the midpoint as symmetry point of the multi-maxima pattern.

Line 173-174. It should be noted that Kamb, Hooke and others have discussed the issue of accounting for complex and branching shapes of large grains when making c-axis plots.

We added the recommended references and furthermore two papers that also show images for a better visualisation:

Therefore, two-dimensional cuts through large, branched grains may let them appear as several individual grains within the same section. Kamb (1959) and Hooke (1969) have already discussed the statistical relevance of these branched grains. The sketches in Hooke (1980), Fig. 6 and more recently in Monz (2020), Fig. 3, further illustrate this issue that could result in over-represented clusters in the superimposed stereo plots from the different sub-samples.

Fig. 6. The caption could be shortened by stating that the annotation is as in Fig. 5

Changed.

Line 192. The c-axis fabric has orthorhombic (and perhaps close to axial) symmetry, but is this also true of the stresses? What about the other two principal stresses. Are they consistent with plane strain or plane stress, as appears to be assumed in Fig. 9? Are the principal stresses and strain rates in this section of the glacier near the surface parallel to the flow direction (σ1), vertical (σ 3) and horizontal (σ2), with the lateral strain rate close to zero, as one would expect for a valley of constant width. One would expect the maximum principal stress to become inclined deeper into the ice as shear stress parallel to the base increases, which the modeled stress shows a tendency to do.

We will add the other two principal components in Fig. 7. Although the lateral strain rate is small, it is not zero, neither as result of the model nor from our borehole displacements at the surface. The modelled principal stresses are not perfectly aligned with the calculated eigenvectors from our COF analysis. They show a slight offset of 25°. However, the eigenvectors (blue dots in Fig 9) are the actually calculated directions of the principal axes derived from our measured COF distribution and not only an assumption. In all other depths, the distribution of the eigenvectors is similar.

Line 200-204. This is a possible explanation, but I prefer the misorientation of the sample as the explanation, which as you state, fits very nicely when a 60o azimuthal 'correction' is made. A preserved fabric from earlier in the flow path is less likely at high temperatures when rapid recrystallization is expected.

We removed this immature hypothesis, especially as we cannot see a smaller deviatoric stress in the uppermost parts.

Lines 210-213. With this explanation you would expect σ1 to be vertical to explain the fabric at 79m depth and not as given by the numerical model. Although the vertical normal stress increases with depth, it is the deviatoric stress that controls deformation, not absolute stress values, and this likely does not change greatly with depth. I think the main thing that changes with depth is not the vertical effective compression (σ1 – σmean) but the increasing addition of base parallel shear stress, that in general terms increases linearly with depth.

Our mistake was to consider a (hydrostatic) overburden pressure. However, this hydrostatic pressure does not contribute to the deviatoric stress that drives the c-axis orientation (via strain rates). We revised this part accordingly. As you say, the c-axis orientation (i.e. the centroid) in the deepest part is in alignment with shear stress: Base-parallel shear stress lead to a base-parallel orientation of the basal planes and thus a more vertical c-axis. The model shows that the shear stress component σ^{d}_{xz} (which we will add to the results section) is the most dominant stress and the in-flow compressional component σ^{d}_{xx} is much smaller in this depth (similar to σ^{d}_{yy}).

Line 217. There is almost certainly some dependence of fabric on strain, which may not be great with fast recrystallization.

Here you suggest to also consider the strain (and not strain rates)? Indeed, recrystallization may appear only if the strain load is high enough. However, we need to analyse, if an immediate (and complete, i.e. all grains recrystallize) recrystallization is necessary or if we could also have a recrystallization of some grains that allow a strain reduction for the whole polycrystal and then other grains with less suitable orientations can still exist until the strain becomes too large and they recrystallize too.

Line 213-214. In simple shear the directions of principal stress are only aligned with those of principal strain for infinitesimal strains. The divergence grows as strain increases.

We assume that strain rate (and the strain) and stress direction form an angle of ~45°. Therefore, the principal stress direction (governed by the simple shear component) in the deepest part of ~50° and the actual centroid (~0°) would fit under such an assumption. The MM cluster is aligned with the strain rate direction in that depth and not with stress as stress and strain diverge for simple shear.

Line 238 I don't believe Cuffey and Paterson really explain why there should be four maxima when the stress deviates from unconfined compression. This is more of an observation than an explanation.

No, they only provide a description and mention different stresses are required for multi-maxima. We considered this in the revised manuscript.

Lines 244-245. This is unclear. A change in direction of glacier flow could be associated with either an increase or decrease in strain rate and thus decrease or increase in recrystallized grain size. Why just a decrease?

This is correct, it could be an increase as well. However, here we want to highlight the effect of strain-induced boundary migration with new nuclei (SIBM-N). In this particular type of recrystallization, grain growth counteracts against recrystallization with new and usually smaller grains. We need to rethink how we can make this clear.

Line 251 and Table 2. Table 2 does not really give the grain size distribution, only average numbers of grains and average and maximum size in each sample. It would be useful to know the number of grains in each size category. Also interesting to know if there is any difference between the large and small grains in COF.

We added a supplementary figure (according to the comments of the second reviewer) and median and 6 different grain size classes to Table 2.

Line 255-266. I'm not sure how much information is given by the air bubbles, except they do provide excellent evidence of active recrystallization by grain boundary migration. Bubbles are found both within grains and along grain boundaries both in temperate ice and in cold ice experiencing dynamic recrystallization, although the recrystallization mechanism may differ.

As you said, we want to provide an example for recrystallization here. However, we expect to find more air bubbles along grain boundaries if the recrystallization takes place much slower. Air bubbles could be seen as obstacles and thus more energy (from strain rate or temperature) is required to fully incorporate these obstacles in the grain matrix rather than having an accumulation along dislocations.

Line 269. Hooke and Hudleston were concerned with polar, not temperate ice. The study was made on the Barnes Ice Cap.

Thank you, again a valuable hint. We will change it:
They were observed in early studies on temperate glaciers (e.g., Rigsby, 1951; Kamb, 1959; Rigsby, 1960), ice capes with ice temperatures above -10°C (Hooke and Hudleston, 1980), and also in the bottom ice of Byrd Station and Cape Folger in Antarctica (Gow and Williamson, 1976; Thwaites et al., 1984). They are often referred as "diamond-shape" pattern or fabrics.

Line 276. Whether the multimaxima fabrics are a result of unrepresentative sampling is still arguable in some circumstances, although the case you have here for these being true multimaxima fabrics is a strong one.

Here, the additional analysis according to your comment in Line 251/Table 2 may be useful to show that there are multi-maxima, but especially for the smallest grains, which may result from recrystallization, the analysis shows a preference for selected clusters.

Line 290-291. I think more data is needed to support this conclusion. The cores taken by Tison and Hubbard were in a different regime within the glacier – accumulation zone where perhaps there is *longitudinal extension rather than compression, and close to the lateral margin of the glacier rather than in the center. This must lead to a more complex stress regime.*

Actually, some cores were drilled in the ablation zone and in these cores they found multi-maxima at the bottom. However, it is true that they are drilled at the margin and therefore they could significantly differ from our core close to the centre flow line of the glacier. This could complicate a direct comparison.

Line 298-299. The combination of compression plus simple shear as applied in these experiments makes sense for much of your core, but not for the highest one where the shear component is minimal, nor for the lowest one, where the σ1 direction lies well outside the small-circle girdle of maxima. Some other explanation must hold in these places.

In case of a misorientation in the upper part, our explanation would fit as well. However, even after recalculating the deviatoric stress, we cannot find a proper solution for the depth of 79 m. The diamond pattern seems to cluster around the direction of the strain rate induced by shear stress only (if assuming that shear stress and horizontal compression are the two dominant deviatoric stresses).

Line 300-301. I'm not sure if I'm properly interpreting what you are saying here, but the planes of maximum shearing stress in Duval's combined compression-simple shear experiments are not vertical and horizontal in his experiments, but inclined by an amount that depends on the relative amounts of normal compression and simple shear.

This is exactly, what we wanted to cite here. His experiments show a multi-maximum pattern that is aligned with the compressional axis and two of these maxima are also aligned with the poles of the two shear planes as the angle between the maxima and the principal direction is roughly

Line 312-316. Both Llorens et al. and Qi et a. are dealing only with simple shear, not with combined compression plus simple shear as in the torsion plus compression experiments. The conditions in the Rhone glacier I imagine change from horizontal compression with minimal base-parallel shear near the surface to horizontal compression combined with increasing base-parallel shear near the base of the glacier. As theory shows, shear stress increases approximately linearly with depth, while longitudinal stress stays approximately constant.

The horizontal compression slightly decreases and the base-parallel shear stress significantly increases with depth. With these modelling results in Llorens et al and Qi et al, we wanted to discuss whether some of the maxima can be a result of the shear stress. The idea was to separate the maxima and explain how they develop as a result of different crystals that react as a network against different stress conditions. We have to admit that this was very speculative and needs to be revised. However, these localisation effects, i.e. some crystals recrystallize immediately to act against certain strain rates whereas other grains could still keep their orientation is a point that we reformulate in our improved version but with less emphasis as we tried to do in the first draft.

Fig. 9. The stress state shown in Fig. 9 is almost that of simple shear (no base-parallel longitudinal compression) with the shear plane (taken as the glacier bed) horizontal and σ1 inclined at 45o to the shear plane. If it is simple shear, there will be no horizontal compression and thus no shortening in the glacier flow direction, which is incompatible with your data. If horizontal glacier flow-parallel compression is added σ1 will move closer to horizontal than it would be for simple shear alone. This looks like being the case for much of the glacier from the stresses shown in Fig. 7. I would expect the inclination of σ1 to be near zero at the surface and something less than 45o close to bedrock, the amount depending on the amount of horizontal compression. Although not a smooth change, the σ1

directions in Fig. 7 are consistent with this. The plot in Fig. 9 does not correspond to any of the plots in Fig. 7, all of which have σ1 at a shallower inclination than 45o and thus have associated planes of maximum shearing stress that are neither vertical or horizontal, unlike the situation in Fig. 9. The one closest to horizontal thus cannot be considered a plane of simple shear. The 'shear plane' must always be the presumably sub-horizontal glacier bed.

Same as our comment for line 312-316, this figure was planned to support our assumption that particular clusters react against the different stresses.

Line 340. I disagree with the statement here (see comments for lines 210-213). Although the absolute value of the vertical normal stress increases with depth, the deviatoric vertical normal stress changes much less. It is the increase in base-parallel shear stress combined with the horizontal compressive stress (σxx if you like) that causes σ1 to rotate from near horizontal at the surface to inclined at some angle of less than 45° at the base.

Indeed, the vertical stress σ^d_{zz} is not responsible and actually decreases in our revised model with increasing depth. We will change this conclusion accordingly and agree with your suggestion that the orientation is driven by σ^d_{xz} and σ^d_{xx}

Line 342. The second part (ii) of the explanation for multimaxima fabrics given here makes no sense by itself. All states of stress that are non isotropic involve shear stresses. If the multimaxima fabric depended solely on the state of stress – that is with instantaneous adjustment of the c-axis fabric as the stress field changes – then there should be a constant relationship between the positions of the maxima and the principal stress directions. This clearly is not the case as the relationship in the deepest sample shows. There is, however, as you note, a consistent relationship between the fabric and the σ1 direction through most of the glacier and in all cases, with small deviations, the centroid of the COF fabric and the σ1 direction lie in the vertical plane that contains the flow direction. This is a key relationship that I believe you have only partly explained.

We are going to revise the interpretation and discussion. Then, we also put a larger emphasis on the fact that the general COF pattern is aligned with the flow direction (with an exception in 79 m).