Dear Erin Pettit,

Thank you very much for your valuable comments. We appreciate your constructive feedback, which helped to enhance the quality of our manuscript tc-2020-133 entitled “Crystallographic analysis of temperate ice on Rhonegletscher, Swiss Alps”.
We have considered your suggestions for grammar and writing style 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.

General comments

This paper provides a careful analysis of the measured fabric in the central part of a Rhonegletscher, in the ablation area through ice cores (not quite to bedrock).
The authors did a really thorough and rigorous analysis of the fabric using multiple thin sections in 3 orientations. The most well analyzed core I have seen for a temperate glacier, I appreciated the thoroughness, as it was necessary because of the dominance by large grains and a grain size distribution that if far from normal. This paper in some form should be published because of the beautiful data set.

While the data analysis is done really well, the interpretation in terms of stress state is not as thorough and rigorous. Their qualitative interpretation of the stress state and its relation to fabric and recrystallization processes is confusing and in a few places incorrect. The paper would benefit from a summary of the key states of stress, key metamorphic processes, citing the original research (going beyond Cuffey and Paterson and the Faria reviews). As a reader, if I am to trust their interpretation of the fabric, I need to trust that they understand the underlying physics. At this point, the physics is description is still lacking. It is imperative that the interpretation of the fabric in terms of the stresses be written with the same care and rigor that that fabric was measured.

First, it would be helpful to clarify when deviatoric stress is being used versus total stress. Deviatoric stress control most of the deformation and pressure plays a minor, if any, role in deformation, therefore describe the deviatoric stress states rather than "absolute" stress and "overburden." For example, the authors suggest that there is less deformation in the surface layers because the "absolute" stress there is low, but this is not the case - the vertical compressive deviatoric stress is no necessarily smaller at the surface, it is typically about the same - it is only the pressure term in the total stress that is smaller at the surface and pressure does not drive fabric (only gradients in pressure or overburden can drive flow).
I would suggest that they re-write the description of the stress state in terms of more formal tensor components, and more specific (and correct) wording. And please be explicit about what is behavior linked to stress and what is behavior linked to strain rate (and discuss with respect to the statement that strain rate ultimately drives fabric development not stress).
In terms of writing, there are numerous run-on sentence, imprecise wording, and extensive use of passive voice, all of which slows down the reader. I provide examples of a few of these (but not all of them) below, I encourage the authors to edit carefully for these three writing issues.

We will check for any passive structures and run-on sentences and appreciate your particular recommendations below. In the revised version, we introduce a subsection about the physical details. We also show the deviatoric stresses for each depth in a separate table and use the particular values for an improved discussion. Of course, the overburden pressure is not responsible for deformation of the ice. We have rectified this blunder.

Specific suggestions:

Line 1 Abstract - the first line of the abstract should offer some kind of bigger picture motivation, something to entice readers beyond those already rheology and fabric "geeks" - this is a neat paper with respect to the unique measurements and it would nice for the broader glaciology community to read it.

We added a more general introductory sentence:
The crystal orientation fabrics (COF) provide key information about the mechanics of glacier flow as its development is driven by a combination of stresses, strain and recrystallisation. Detailed information of COF can be considered to improve specific parameters for glacier modelling.

Line 20: delete "to that" - not necessary

Removed

Line 23: delete "do"

Removed

Line 24: Faria offered great reviews in his 2014 papers, but be careful citing those papers when there are better papers that are more directly or more originally related to the statement. Here by citing Faria, it implies that that paper was the first to discover that COF evolves in a glacier. Provide more direct/original citations please (or be explicit that you are citing Faria as a review article).

We added the original work:
The stresses and strains occurring within the ice mass not only cause glacier flow, but also induce the development of a characteristic COF and microstructural anisotropy (Gow and Williamson, 1976; Herron and Langway, 1982; Alley et al., 1995, 1997) and summarised in Faria et al. (2014a).

Line 30: delete "quickly" unless you want to provide the timescale that quickly is indicative of (words like quickly, clearly, mostly, etc don't add any information and can lead to confusion).

Removed, we revised this sentence according to the first reviewer's comment.

Line 53 - run-on sentence, breakup into two or three.

We changed it as follows:

To date, ice core drilling and preparation of thin sections is still a time-consuming process. Only a few discrete measurements are possible within a reasonable amount of time. Nonetheless, the technique for analysing COF has developed extensively, for example, by using image analysis software and powerful computing resources (Wilson et al., 2003; Peternell et al., 2009; Wilson and Peternell, 2011; Eichler, 2013).

Lines 74-78 - lots of passive voice here, rewrite

We changed this to a more active style:

As the ice is just at the pressure melting point, we used a thermal drilling technique (Schwikowski et al., 2014). Although hot-water drillings, performed in the vicinity of the ice core location, showed a mean ice thickness of 110 m, we stopped drilling at 80 m, when hitting some gravel. This gravel blocked the cutter head. We retrieved an 80 m long ice core, with a gap between 46 and 50 m due to technical issues.

Line 78 - I'm still a little confused how you knew the orientation, did the drill head not spin on the cable as it was lowered or raised?

Indeed the core barrel and drill head could rotate, but a magnetometer was integrated into this core barrel. After each drilling we turned this core barrel until reaching the orientation at beginning. Then the core segment was retrieved and its orientation was marked with a knife. Afterwards we tried to attach the segment to the previous one. If this was possible we added a notch with a soldering iron on both segments. However, if this was not possible, we opened up the notch of the knife. Later, we could retrieve the orientation of the core barrel during the drilling process. Assuming that the core segment was not rotating within the core barrel (e.g. due to sudden shocks which we avoided by a decent winch speed), we could retrieve the actual core orientation.
The data also reveal that there is no 360°-spinning around the cable (just slight movements). The water-filled borehole damped any rotation of the core barrel.

Figure 2/3: I like the diagram in figure 3, but why not just calculate the bulk surface strain rate components from these measurements instead of the figure 3 plot.

We actually calculate the strain rates as constraint for our model. We will add this information and discuss whether it could replace Fig 3 (or if we keep both as the figure emphasises the smooth decrease in flow speed along ice flow).

Figure 2/3 - Did you measure the emergence velocity? I would expect emergence, and this will affect the stress state.

We did not measure this value at this position. However, a reference station about 50 m away from the boreholes shows an emergence of 1.5-2 m a^{-1}.

Line 89 - This paragraph seems to shift to modeling methods, from drilling methods. While it mostly reads ok, perhaps make this a different section? Especially since the section is titled "field site and data acquisition" I also think one paragraph describing the model is a bit thin. If you are actually using the model to interpret your data, please describe it more rigorously and explain the weaknesses with the model output - how much do you trust the modeled principal strain rates and directions? Given that you just assumed a rate factor from another glacier and tuned the sliding to fit this glacier site? Did you conduct a sensitivity study to assess the impact of your parameter selection on the stress and strain rate output from the model? Given that the model inputs are approximate, I'm not entirely sure that the model provides any better qualitative assessment of the expected principal stresses and strain rates than a simpler flow band description explained with clear assumptions.

We improved the modelling description and moved it to a new section.
According to your questions: As you point out in your comment for line 97, the model is only used to constrain our interpretation. However, we could have used a flow band description or our borehole data to interpret. The main task behind employing the model is to get some quantitative values rather than just speculating qualtitatively about different stresses (namely compressional in-flow and shear stress). The most important weakness is that we use the stress information of a single point to explain the stress conditions for a certain area of the glacier. Local stress effects cannot be captured by such a model. Furthermore, we also do not have reliable bed velocities and we have to admit that the model is

only constrained by surface velocity and ice thickness information. To overcome these weaknesses, we will consider strain rates derived from our borehole experiments.

Line 91 - delete "simply"; say "steady state" model or something like that.

We revised the whole paragraph and removed it.

Line 97 - I realize that the model is not intended to be a perfect match, but tuning the model to only one surface velocity is limiting. But I think that's ok, if you are mostly going for the style of stresses and not the real magnitudes (but see my comment above about just using a simpler flow band description because models like this not tuned well can induce complexities that might be interpreted as real). Importantly, the stress distribution with depth at the site of the borehole is highly dependent on the sliding coefficient. When you use these results to interpret the data, please discuss this with respect to the limitations of the model (see my comment above about the vertical distribution of stresses). Oh - and what was used for accumulation/ablation rates? The vertical strain rate at the core site will depend on the ablation rate. Did you measure the vertical velocity at the surface?

We do not model a time-transient evolution but only calculate the stress field for a the actual geometry. Therefore, we did not consider the accumulation and ablation rates. The limitation is that the model only provide stresses and not strain rates or directions. We can calculate strain rates with Glen's flow law. Please regard the model as constraining information for the interpretation. We do not intend to setup a perfect model that describes the ice flow and stress + strain rates. This needs additional measurements and is beyond the scope of this work. In the revised interpretation, we also consider strain rates derived from the borehole data.

Line 123 - delete "as discussed later" and "important" - they don't provide any useful information here.

Changed.

Figure 5 - nice figure, I am interested in the other 2 eigenvectors - are they equal? Also, please provide some examples of the size distribution (histogram? or statistical distribution curve? You have some statistics in table 2, at a minimum, provide the median. But I would suggest putting the size distribution for each depth in the supplementary information. Put a reference in Figure 2 caption to Table 2 and the supplementary information for the size distribution.

We add the other two eigenvectors. The eigenvalues are not 100% equal but both around 0.15-0.25. Usually the second eigenvector is laying in the vertical plane of the diamond shape pattern. We also add the number of grains for 6 grain size classes (<1/1-5/5-20/20-100/100-500/>500 mm^2) to Table 2 and show a histogram for a selected depth and put the others to the supplement. We will use this additional figure in our interpretation as the small grains (<1mm^2) clearly emphasis one of the four clusters. This provides some evidence that recently recrystallized grains in the deeper and intermediate parts of the glacier prefer one of the clusters rather than equally distribute to all four clusters.

Line 166: "oriented in the direction of glacier flow - just be more specific with wording here. The c-axes points within xxdeg of the flow direction (155deg).

We considered this in the revised manuscript.

Line 180 - I think this section would be best started with an overview of the deviatoric stress state (if it isn't already in the background), as measured from the surface stakes and as inferred from the model, in terms of the stress tensors and principal directions.

Here, we also agree and included such an overview showing the information derived from our borehole experiments.

Line 181 - This first sentence doesn't add anything and isn't necessary and is subjective. Just cut it.

Removed.

Line 192 - see my note from line 180. It is difficult for me to separate the effect of the longitudinal compression alone - I'd rather see a description of the full deviatoric stress state as a function of depth and then look at what components are doing most of the work. Also, there are two horizontal stresses (sigma xx and sigma yy) better to describe these as longitudinal and transverse.

We consider this in an introductory paragraph of this section. We added the stress components. Subsequently, we refer to these 6 components. We also considered your recommendation to distinguish between the dominant longitudinal and the transversal stress.

Line 198-202 - Misorientation is most likely, is there a need to go into complex (and incorrect) explanation about surface stress? See my comment above that the "absolute stress" doesn't affect the fabric, only the deviatoric stresses do. This is a really fundamental point, please interpret your fabric in terms of deviatoric stresses.

We removed this immature argumentation.

Line 211 - I'm not sure I understand this, overburden doesn't generate anything, only gradients in overburden (even better to use formal deviatoric stress terminology).

Indeed, this needed some revision.

Line 228. I think the author is referring to recrysllization when they say "these processes" - please note that it is not true that they were "just attributed to temperature" - cummulative strain has always been known to be a key part of the process. Please cite earlier work - maybe back from the 70s or 80s on this rather than suggest that this is new knowledge?

We had to revise the literature here and also for the following comments about Faria. However, to our knowledge, Faria et.al. (2014) was the first approaching this in a systematic way using the recrystallization diagrams in their Figs. 13+14. They particularly distinguished between strain-induced boundary migration with new grains (SIBM-N) and strain-induced boundary migration with keeping the old grains (SIBM-O).

Line 233 Because normal and shear stresses are the two types of stresses, then the statement that a combination of normal and shear must have been involved to create the fabric is minimally useful. Please provide more specific description.

We revised this paragraph to be more specific with terminology.

Line 239 what does it mean for a tensor to provide "hints" (that seems to me like an anthropomorphism)?

Same as before. We tried to be more precise with terminology.

Line 239/240 Do you mean that this site is not 100% sliding? That's the only way to avoid borehole shearing. It seems like the model set up already defined a limited amount of sliding, there must be some non-zero component of tau xz. So that was an input to the model, not an output.

We were just describing the obvious state and considered this, when revising and shortening the interpretation.

Line 241 A parabola is typically for an x^2 relationship, that is not the case for the curve resulting from Glen's flow law.

It is a hyperbolic curvature.

Line 243 - how long ago was "recently" can you provide estimates for the timescale of the last significant change in stress state and express that timescale as a percent strain the crystal experiences?

"Recently" must be within the last two decades as the ice flow direction changed about 500 m up-glacier and our pattern is in good agreement with the current flow direction. The strain % is difficult to assess as we do not have information about the surface velocities in that area further up-glacier.

Line 244 - I think the authors mean "latter" not "later"

Changed.

Line 244 - I'm not quite sure why a mean grain size reduction would necessarily occur after a change in flow direction, unless you are suggesting that the change in direction is triggering specific recrystallization (migration or rotation). I am also not sure I understand the citation to Faria here, as recrystallization has been described in many papers before. Perhaps you can be more specific about what Faria contributed that is specific to this analysis? And please more carefully cite the statements here (alternativly, if you write an overview of the stress state and metamorphism of the crystals in the beginning that describes and cites each process as background and properly cited, you can avoid having to add too many citations in this discussion section.

As described in our answer to line 228, Faria was (to our knowledge) the first, who distinguished between SIBM-O and SIBM-N. Others only referred to dynamic and rotational recrystallization (RRX). This distinction is particularly important for observed grain-size changes at high temperatures as in our case. That even leads to a new process understanding, e.g. Steinbach et al (2017) in Frontiers in Earth Science, Vol 5.
The main flow direction further up-glacier is 220° about 500 m further up-glacier. Our data show a clear alignment with the local flow direction. Even when assuming about 20 years for the ice to flow downglacier, we assume that the c-axes were rearranged within this rather short period. As we have high temperatures, we expect fast grain growth until the grains reach the steady-state described in Faria (2014). If these grains are not suitably oriented anymore, the most unsuitably oriented grains will recrystallize (SIBM-N) as new and most probably smaller grains (as the grains would have reached the maximum size that was possible under previous stress conditions). Faria explains the recrystallization as function of grain size, temperature and strain rate. This is why we assume that the grains need to recrystallize and start as small new nuclei and then grow again. We considered such an introduction as you propose.

Line 256 I really like the images of the bubbles and the grain boundaries - it does show fast grain boundary migration and active interaction between bubbles and boundary movement. How do you know it was a "complete" recrystallization?

We derived this from the alignment of all grains with the current stress conditions. There is no grain with intermediate or large size that has an orientation that is not aligned with the current flow direction.

Line 258 The image of bubbly and bubble free ice brings up a question I have as to whether there are signs of refrozen water in these thin sections. Water filled crevasses refreeze with a different microstructure that is typically bubble-free or with patterns of bubbles and distinctly different crystals. Some of the small grain "fracture" noted in the paper also might be a post-depositional process. Perhaps it is ok to include these in your analyses, as the same crystal evolution processes are happening, but it might be useful to discuss the ice from snow compaction versus any refrozen water and how that might influence the fabric and grain size distribution (and bubble)

We have seen those fracture traces in two depths (22+45 m). We also analysed these fracture grains separately and can provide information about their orientation. Some of them are perfectly aligned with the surrounding large grains (especially if the fracture is thin). Others (if not a fracture but rather a patch of small grains) show a girdle structure. This girdle is aligned with the glacier flow (extension in transverse direction). We could exclude these grains from our analysis. This would emphasise the diamond pattern in 22 and 45 m.

Line 265 - again, I believe you mean to use the word latter.

Changed.

Line 265 - please define "fast" - fast compared to what? How fast is fast?

We revised stating a fast boundary migration within a few years at maximum.

Line 273 - delete "as employed in our study?

Changed.

Line 278 - again, Faria is not the first one to say that temperature is not the only driving process behind boundary migration recrystallization.

This is true, but to our knowledge, Faria is the first one who distinguished between SIBM-N and SIBM-O which assumes a reorientation of old grains (SIBM-O) or a complete creation of small new grains (SIBM-N). According to his model and considering our strain rates and temperatures we have SIBM-N conditions here. This is the difference to earlier studies.
We rephrased according to the calculated strain rates from borehole measurements

Line 280 - be careful using such a strong word as "only" - also, this is a long run-on sentence and would be better to be split up and explained in more specific wording.

Thank you, we rewrote this long sentence and consider, that we do not have 100% evidence for our argumentation and thus “only” is obmitted here.

Line 280-295 - These sentences don't actually explain how the diamond shape forms, just that it happens at high strain rates in certain orientation of stress. Rewrite this to explain the underlying physical process, if possible. If not possible to explain the physics, then explain this as being associated with specific conditions, with physics still to be determined.

We cannot explain the exact physical processes but we rewrite our suggestions that may be responsible for the diamond pattern.

Line 290-291 - The word "only" is too strong, this sentence seems to be a hypothesis you are trying to suggest that your data support (but I don't know what the "certain strain rate" is).

Here we need to be more conclusive and argue with the actual values of strain rate.

Line 293 - "the absolute strain rate... is expected to be" - please clarify which components of the strain rate tensor you are referring to, or if you mean the effective strain rate (tensor invariant). There is very little discussion of inherited fabric in this paper, How does inherited fabric affect the deeper layers (I don't agree that the surface is necessarily inherited because of any less strain rate at the surface - the only component of the strain rate tensor that is smaller at the surface is the simple shear parallel to the bed).

This paragraph was revised. The strain rate and also the deviatoric stress is not smaller at the surface compared to other depth.

Line 310 - how do you judge "good agreement"?

We wanted to point out that the stress conditions in these laboratory experiments are in a similar range as we find them in the glacier.

Line 317 - "clearly" is not a helpful word - at this point, I am a bit bogged down in generalities and imprecise wording in the fabric and stress/strain relationship, that I am struggling to judge for myself what the source of the 4 maxima are.

We avoid these words in the revised version.

Line 335 - I do believe twinning has been observed use EBSD (such as Obbard's work on the Fremont glacier and/or at Siple Dome), I can't remember which one she noted the a-axis alignment that would suggest twinning.

Up to date, we could not find the respective part in the papers of R.W Obbard. However, her work is worth to consider as it clearly points out the ambiguities of our technique (analysing the c-axis without the a-axes information).

Line 340 - This statement isn't correct, at least the way I am understanding it (increasing overburden/pressure). Please describe the fabric in terms of deviatoric stress tensor as a function of depth, and, in addition, explain more clearly why 4 single maxima are created rather than a girdle, I think you tried to explain this, but it didn't come through very clearly.

Indeed, this needed a revision. The overburden pressure is hydrostatic and not responsible for strain rates that drive c-axis developments.

Line 346 - yes, in terms of "comprehensive" analysis of the thin sections measurements - this paper is *awesome. In terms of interpretation based on stress state, this paper needs work. There have been some other work on temperate glaciers (including some ongoing work on a glacier in Alaska I think - by Gerbi and others? I'm not sure the status of their publications).*

We agree that we have to revise the interpretation part and added a couple of additional details about the stress state in the glacier as derived from the model and further provided information about the strain rates from in situ measurements. These data should simplify the interpretation and allow a better access to the information provided in our paper.
We also figured out that there are a couple of presentations from Gerbi at AGU. However, there seems to be no field data published yet.