EGUSPHERE-2024-2653
EastGRIP ice core reveals the exceptional evolution of crystallographic preferred orientation throughout the Northeast Greenland Ice Stream
Referee Report #1

R: This paper presents a comprehensive and detailed study of the evolution of the texture (or fabric) along the EastGRIP ice core. This paper is based on a tremendous measurement campaign that must be highlighted (more than 1200 thin sections made and measured!) Overall the data are of very good quality, and their interpretation, for most of them, are reasonable, well argued and well documented.

A few interpretations can be questioned, as detailed further in this review, and I would appreciate the authors to consider these comments prior to publication. Some interpretations are not in good agreement with previous work what is not a problem by itself, but the discrepancies are not always well argued.

Another point concerns the citations. In view of respecting the work of former colleagues (and research ethics rules) I would suggest the authors to avoid citing only review papers (e.g. Faria et al.) but also the original work cited in the review papers that are sometime more closely related with the subject matter.

Appart from that, this paper clearly deserves publication in TC and I would like to emphasize the clarity and quality of the writing.

A: We thank the reviewer for this thoughtful review and appreciate the input, which certainly helps to improve the quality of the manuscript. We agree with most comments and will make sure to include more original studies in the revised version. In the following, we will reply (in blue) to the specific comments .

Specific comments :

- the authors make use of the second order orientation tensor as a proxy of the texture evolution. Please mention that this proxy is not adapted for all types of fabric, and in particular, do not discriminate some multi-maxima fabric with isotropic ones. What about the case of the crossed-girdle fabric ? How well is it represented by the 2d order orientation tensor ? What precautions should be taken when interpreting the evolution of the eigenvalues of the 2d order orientation tensor with depth ?

A: We mention this aspect in section 2.5 ("However, eigenvalues are limited to unimodal distributions due to their inability to differentiate between certain fabric types, e.g. a narrow girdle and a multi-maxima. Thus, additional data representation such as stereographic projections is required to evaluate more complex fabric patterns.") and will strengthen this point further. Keeping this in mind is certainly a precautiona as well as providing a variety of data (see below).

As the crossed girdle has not been described before in ice, it is tricky to judge the use of different proxies – we thus present the CPO patterns, the eigenvalues, and contoured stereo plots, and will add additional microstructure maps for this CPO. This provides a robust data set to the reader.

- In the abstract and along the text a specific « crossed girdle » fabric is mentioned as an originality of the obtained measurements. Nevertheless the specificity of this fabric does not appear so obvious. Would it be possible to find specific illustration or data treatment to make it stand out better? The 241 m depth pole figure that is shown in figure 2 could well be interpreted as a wide girdle with a low anisotropy…

A: We also came across this issue and tried to solve in a good way without giving it too much space in an already rather long manuscript. In figure 8 we combine the data from 6 adjacent thin sections to more clearly visualise the fabric pattern; this turned outto be the best option. We will now include microstructure maps of this depth regime and update the figure displaying the observed crossed girdle CPO in comparison to other studies investigating this specific CPO pattern as observed in quartz.

- I missed some figures of microstructures that would illustrate the interpretations about the mechanisms at play, such as dynamic recrystallization. I am pretty sure a few nice microstructures could be shown, for each depth interval with specific characteristics, and at least in supplementary.
A: This is a very good idea and has also been mentioned by referee 2. We will include more microstructure images in a new figure similar to the CPO overview figure (Fig. 2).

R: - Table 1 : illustration here of my comment about the crossed girdle. A transition from crossed-girdle to vertical-girdle is mentioned but not clearly shown elsewhere.
A: The transition is visible in the provided CPOs in the appendix and very tricky to show in a single figure as it occurs gradually. We will refer to the appendix figure more directly. We will relocate the crossed girdle figure closer to the results section and will, in parallel, enhance its description.

R: Anisotropy indexes could be added in this table, similar to the one shown in figure 3.
A: We are not sure how and which anisotropy index shall be calculated. We already show the eigenvalues (alone and in comparison), all CPOs, the woodcock parameter, and the Point-Girdle-Random distribution and will add microstructure images. We could add the Point-Girdle-Random mean values for the derived depth regimes in Table 1, but this might be rather confusing to the reader and repetitive to the figure. Performing the PCYS (Polycrystal yield surface) analysis would be a good way to indicate the degree of anisotropy. However, this is complicated and involves microstructural modelling going beyond the scope of this overview study, but could be the objective of future research.

R: - Figure 3 : please remind somewhere how the anisotropy indexes are calculated. Is the Woodcook parameter the more adapted ? Why not use ln(a3/a2) and ln(a2/a3) that evolves in a smaller interval ?
A: The used indexes are calculated following Vollmer (1989, 1990) and are analogous to the Woodcock calculations of $\ln(a_x/a_y)$ vs ln(a1/a2). The PGR plot differs in a few ways: 1) it is closed, 2) it has three instead of two end members, and 3) it is based on the differences between the eigenvalues and not their ratios. We don´t judge here which one is more adapted and only decided, that for the EGRIP ice core, the PGR plot is better to represent the dynamic CPO patterns. If required by the editor, we are happy to include a ln(a3/a2) and ln (a2/a3) plot in the appendix.

R: - Figure 6 : please increase the size in the final version.
A: We will edit this figure to include new electrical conductivity (ECM) data providing a better depth-age estimate. In doing so, we hope to increase the size and make it more comprehensive. We understand that the figure seems comparably small in this format. However, it is the maximum size allowed by *The Cryosphere* and should appear larger in the final document. We tried different formats and there seems to be no better fit without losing information or using a full page for it, which could be an option.

R: - Part 4.1 : please cite Alley et al. 1988, or Castelnau et al. 1994 for instance to illustrate the link between rotation of c-axes and deformation !
A: Done.

R: Idem, regarding the impact of recrystallization, De la Chapelle et al. 1998 or Thorsteinsson et al. 1997, provided already an overlook of recrystallization along ice core (maybe not the first ones still), please site them (or others) instead of Faria et al. (or on top of).
A: Done. We edited references throughout to show more of the original research.

R: - Part 4.1.1 : I don’t remember in details the experiments presented in the paper by Azuma and Higashi 1985 but one should be careful when comparing deformation-induced fabrics observed along ice cores with experimental ones since, even for some of the slowest experiments made by Jacka and co-authors, the fabric results from dynamic recrystallization that takes place already above

1 % strain, and dominate from about 10 % strain… And the DRX fabrics can be way different from the deformation ones, especially in compression.

A: This is true and should be kept in mind. We will edit the section mentioning to be careful when comparing laboratory and natural observations.

R: - Figure 8 : here is the illustration of the crossed-girdle that is not so obvious, although clearer that in figure 2. Could a representation of the microstructure help ? Well, I understand that it is not easy to find a clearer illustration.

A: During writing and editing, we shifted this figure several times to find the best position in the manuscript, which turned out to be tricky. We agree, it turns out to be better to show this figure already earlier in the manuscript and to describe the CPO in more detail as also requested by referee 2. We will shift the figure to the fitting results section, include microstructure images, and display skeleton patterns displaying the CPO more clearly.

R: - Part 4.1.2 : some of the tentative explanations lack justifications.

For instance, the comparison with quartz does not really holds since, for some temperature and deformation ranges, quartz has several slip systems of similar activity, it is not the case for ice.

About the activation of non-basal slip systems : please go back to Hondoh 2000 review work where one can read that (1) the critical resolved shear stress required to activate non-basal dislocations is way more too high regarding the level of expected stresses here, (2) it is clearly mentioned that non-basal edge dislocation segments can help activating more basal dislocations by multiplying the active basal slip planes, but can not move on long enough distances to participate to deformation. As a summary, although there exist non-basal dislocations that we observe by EBSD for instance, they are first located mainly close to grain boundary and triple junctions, where local stress can be high, in the form of subgrain boundaries but it does not mean that non-basal dislocations actively participate to deformation. As such, they participate as an accommodation mechanism that facilitates basal glide. A high enough non-basal activity is required to have non-basal glide impact on fabric.

Please go a little further in showing the limitation of such an interpretation, not to leave ambiguous information in the paper that are later re-used in other papers with no clear view on the hypotheses behind.

A: Thank you, we appreciate the in-depth discussion. We only state that quartz has been shown to work as an analogue for ice (see cited references), which of course includes that there are limitations. However, as crossed girdle have been observed predominantlyin quartz and remain a challenge to interpret in glaciology, this comparison is feasible without provoking the wrong idea especially since no other reference data from an ice stream exists so far. Interestingly, both referees have very different perspectives on the slip-system hypothesis, which displays the need for a better understanding of the occurring processes. Eventually, only additional data from e.g., EBSD will help in explaining the crossed girdle CPO fully.

We will add extra text describing the limitations aiming to prevent wrong interpretations.

R: Overall, in this part the authors could give the likelihood of each suggested mechanism.

A: Providing a likelihood of the suggested mechanisms remains challenging without further, complex analyses, such as a dedicated EBSD campaign. We hope to conduct such measurements in the future, potentially enabling a ranking of the likelihoods. We can´t rank them with a good conscience, but we will discuss the different ideas in more detail.

- Part 4.1.3 :

In tension, experimental paper by Jacka and Maccagnan, 1984, could be cited ?

A: Done.

The effect of DRX on the strength of the tension-induced CPO could also, maybe, be mentioned ?

A: Done.

R: - Part 4.1.1:
If I'm right, the CPO transition referred to in this part is not shown in figure 2.
A: We do not refer to a CPO transition in 4.1.1. In the overview Table 1, transition zones are indicated starting from 294 m. These zones are however difficult to display with single images and are thus not included in Fig. 2. We thus provide all CPOs in the supplementary material. We will clarify this issue in the text and link more strongly to the appendix, which makes the transition zones more accessible.

Similarly, line 279, figure 2 doesn't seem to show the maxima of varying strengths in the horizontal plane. Nor does the supplementary. Did I got it right? I just see a broadening of CPO from ai data.
A: We might have formulated this in a confusing way. In the supplements, the varying strengths of the point maxima in the vertical girdle are visible as well as in the PGR diagram in Fig. 3. Investigating the hundreds of CPOs from the deeper parts of the core displayed in the appendix gives a good impression of this.

R: From line 285 I find the explanation based on dynamic recrystallization very confusing. First because I don't see the necessity of evoking polygonization to explain the vertical girdle CPO. In particular since polygonization (one of the mechanism of rotation recrystallization) is known (not only in ice) to weaken the CPO, at least slightly, and clearly not to strengthen it…
Again, please also refer to De la Chapelle and Duval 1998 when mentioning rotation recrystallization since their modeling include some energy calculation that help interpreting the occurrence of different recrystallization mechanisms. Also in Montagnat and Duval 2000 did we link the grain size evolution with depth with the rotation recrystallization modeling.
A: We include the citations in the revised version. We will rewrite this part of the discussion and will leave out the discussion of recrystallisation.

R: From line 295: could DRX with various level of impurity content also be mentioned to explain these rheological differences?
A: This is certainly a possibility and now included in the discussion. Bulk impurity data from this depth are unfortunately not available, but might, eventually, help to explore this possibility further.

- Part 4.1.5:
R: line 310: at the bottom of Talos Dome ice core we very likely observe some stagnant ice (Montagnat et al. 2014), if you want to find a reference to illustrate this statement.
A: That´s a great idea, we include this citation.

R: Line 314, again Faria et al. 2014 did not initiate the dynamic recrystallization conceptual models! Please refer to the original paper (cited in Faria et al by the way).
A: Done.

R: I don't see the interest about separating SIBM-N and SIBM-O mechanisms since, discontinuous recrystallization contains both mechanisms, nucleation and grain boundary migration (owing to the reduction in stored strain energy). On top of that, nucleation is much more likely to end up with orientations different from the parent grains than grain boundary migration. This sentence is at least unclear, but maybe also not very accurate.
A: We here follow the nomenclature by Faria et al. (2014b) to assist the broader TC readership in seeing the differences of the processes. We will reevaluate this sentence.

- Part 4.2: line 340 "we show that assumptions valid for other,…" what assumptions are you referring to?

A: This is indeed unclear. This sentence should display that the strong anisotropy at EGRIP is very different to other ice coring sites, which has to be kept in mind in future studies. We edit this sentence.

- Part 4.4: line 365 “no strong grain-size dependence of the dominating deformation regimes”. To my point of view it also suggests that grain growth and grain size reduction are driven similarly, what goes in favor of continuous DRX, at least in the first part of the core.

A: We agree, that seems to be the case for the first part of the core. We will add this to the sentence.

- Part 4.5: First paragraph: It would be interesting (necessary?) to discuss the relative viscoplastic anisotropy resulting from these different fabrics observed along EastGRIP. In particular, is the crossed-girdle fabric resulting into a mechanical response strongly different from an isotropic fabric, or from the slight cluster fabric that is observed just above? If no, then why bother to try to simulate such a fabric development in an ice flow model?

A: That is true, the impact of this CPO pattern needs further research. As mentioned by reviewer 2, this CPO has neither been reproduced in the lab highlighting the need for further in-depth investigations. We here provide an overview about the data from the EGRIP drilling, the impact of this will certainly be explored in future studies and goes beyond the scope of this manuscript. It would be interesting to include this in ice flow models, but we see that this might be difficult. However, we encourage the community to use this observational data set in future approaches.

Lines 402-404: when discontinuous dynamic recrystallization dominates, the fabric only reflects the stress conditions, and looses the strain history, so the estimated preserved duration could be even lower in the bottom part of the core. Maybe it would be worth mentioning it.

A: Good point, we will mention this aspect.

Line 415: Please see my comment for part 4.1.2: the actual knowledge of dislocation slip systems activity and activation and the too weak information make this hypothesis of secondary slip system activity in EastGRIP very unlikely, and one of the less likely hypotheses over the ones mentioned in part 4.1.2. By bringing it back in the discussion part that way you put a relatively high weight on it and this is misleading.

At minimum, an estimation of the level of stress required to activate a secondary slip system with a high enough level of activity to explain the crossed-girdle texture must be provided. I doubt that this level of stress can be achieve at EastGRIP.

A: Additional EBSD data is required to investigate this hypothesis further. Not mentioning it at all in the discussion is also tricky, as it was found in modelling experiments. E.g., Llorens et al (2016) (Figure 10 -experiment 25) found that under compression (pure shear) and a high DRX rate, the pyramidal is even more active than the basal slip system, at aproximately 55% of shortening. The critical resolved shear stress for the basal is 50 times lower than the pyramidal, but the microstructure activates the non-basal activity due the lack of localisation and was related with a a-axis maximum. We will edit this section and include the uncertainty regarding the slip systems.

Lines 424-425: Why would shear occur DUE TO a strong recrystallization?? I would expect shear near the bedrock to induce high level of strain that, together with the high temperature, favor discontinuous dynamic recrystallization and therefore large grain size. Shear-induced fabrics are similar in deformation and recrystallization (at least for high level of strain) and should lead to a strong cluster. So either this cluster is hidden by the large grain size (too few measured orientations) or the main stress component at the bottom is not shear??

A: Our formulation was not clear enough here. We do not want to state that shear occurs due to strong recrystallization, but that strong recrystallization hampers the analysis (large grains, few data points etc) and thus a clear identification if shearing takes place. We will rewrite this sentence.

References

LLORENS, M.-G., GRIERA, A., BONS, P. D., ROESSIGER, J., LEBENSOHN, R., EVANS, L., & WEIKUSAT, I.

(2016). Dynamic recrystallisation of ice aggregates during co-axial viscoplastic deformation: a numerical approach. *Journal of Glaciology*, *62*(232), 359–377. doi:10.1017/jog.2016.28