Response to RC2 for Manuscript egusphere-2023-3146

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We again thank the reviewers for their careful and critical review of our manuscript and for their thoughtful comments, which have helped to improve it. We also extend our gratitude to all related individuals (reviewers and editors) for dedicating their precious time to this process. Below, we describe our responses to each of the reviewer’s comments, presented point-by-point in blue text (with the original comments in black). For convenience in referencing, we provide an ID number for each of the reviewer's comments and for each of our responses. To make these IDs distinguishable from those used for Reviewer 1, we will use labels such as [R2_1] or [A2_1] in this document, indicating review comments and responses for Reviewer 2.

[R2_1] This paper contains some very interesting and potentially important new data and can make a significant contribution to ice core science. The use of the Dielectric Tensor method to get a much higher resolution data set for COF strength as a function of depth is particularly impressive. The paper also presents full crystal orientation data ([c] and <a> axes and this is an important development as c axes alone do not usually give the full information needed to infer deformation kinematics. Although the paper represents a substantial contribution to ice core science, it needs a lot of re-writing and possibly some more research work.

The paper is way too large and tries to cover too much. Any significant outcome is lost as the paper is poorly focused. I would focus on the orientation relationships of COF to layering and the evidence from the microstructure that helps understand deformation kinematics and mechanisms. The cross-correlation work linking COF to chemical impurities seems superfluous and could be thinned down and simplified. There is a lot of repetition in the figures and the paper can be shortened and improved by a redesign of the figure structure. For example figs 7 and 8 can be merged, figs 5 and 9 can be merged.

[A2_1] Thank you very much for the critical reviews. Based on the reviews, we plan to address major points to the revised manuscript as follows.
We agree to shorten the paper. In the revised manuscript, we plan to reduce the discussion of impurities and microstructures. Regarding impurities, we plan to discuss COF with a focus on the contrast between impure and less-impure layers. Detailed discussion about individual factors (such as chloride ions and dust particles) will be removed or moved to appendix (or to supplementary materials). Microstructures are positioned as supporting information for the discussion of COF, layered structures and their relationship. We plan to combine Sections 5.3.1 and 5.3.2 with Section 4.5. Also, we plan to redesign the figures throughout the paper.

[A2_1] The really key new information is the relative orientation of layers and c-axis maxima. The analysis of these data is not robust as it does not consider the orientation relationships in 3 dimensions. I am pretty sure that the authors already have the information they need to do the analysis in 3D or can get this information with a little extra work and this should be done. If grain shapes were incorporated into this it would be even better. I start my review with this main point, followed by other key issues that I think the authors should address. Apologies I have run out of time on this so have not listed all the minor issues.

[A2_2] Before this review comment was provided, we presumed that the horizontal orientation of the c-axes cluster and the normal axis of the layer inclination are within the same vertical plain, which means common orientation. We had no reason to suspect, for example, there is a kind of torsion between these two. After reading the review comment, we checked if these two are in the same vertical plane or not. We confirmed that it is true throughout the LO20%. Grain shapes were not a focus of the present paper. We have limited information. It is beyond the scope of the present paper.

[R2_3] The paper is very difficult to read because of the ridiculous number of acronyms. Many of the acronyms represent long expressions that could easily be replaced with much shorter expressions, that could then be used as words so that the reader can read the paper without constant reference to an array of acronyms. This might make the paper a little longer, but it will take less time to read and the reader will understand it better. Some of the acronyms relate to words that do not clearly indicate meaning. I will send an annotated version of the acronym list later.

[A2_3] Based on the suggestion, we will decrease the number of the acronyms.

Specific comments.

Major issue: Layering, c-axis inclination and core inclination.

[R2_4] The relative orientation of layering and c-axis maxima is really important. In this paper it is presented in a way that is ambiguous and potentially misleading. “Layered structures” is in the paper title so it is crucial that there is no uncertainty as to the nature of the constraints on layer orientation.

[A2_4] Please refer to A2_2. We confirmed that the horizontal orientation of the c-axes cluster and the normal axis of the layer inclination are within the same vertical plane throughout the LO20%.

[R2_5] Firstly, the description of how layering is measured in this manuscript and in the cited report (DFICPM: et al., 2017) is incomplete. It simply says it was measured with a protractor - it is not clear whether this is a measurement on an arbitrary cut surface (in which case the true inclination is equal to or larger than the measured value) or whether it is a measure of the maximum inclination (true dip) - e.g. by cutting the core along a plane that contains the maximum inclination (perpendicular to strike). This needs to be clarified and the layering added schematically to figure 2.

[A2_5] The inclination of the layers in the Dome Fuji ice core was measured using two methods, as follows: Thin, cloudy bands are faint features with thicknesses ranging from about 10 mm to 1 mm. We observed the ice cores using a light stage, approximately 250 mm by 600 mm in size, placed on a table. The ice cores, shaped as half vertical cuts of the original cylindrical form, were visually inspected by observers who looked directly down at the core from above the light stage. The ice cores were positioned between the observers’ eyes and the light stage. The faint and thin cloudy layers are identified when oriented vertically; only in this orientation do the observers recognize the layers as faint but sharp lines. From angles deviating from this, it is difficult to recognize such layers. By orienting the layers vertically and keeping them orthogonal to the core's inclination on the light stage, the observers could measure and record the inclination angle of the ice cores using a large protractor. The inclination angles of the ice cores are the same as the inclination of the layers. With this procedure, layer inclination was measured in a 3D manner, and we always measured the maximum inclination angle.

In some cases, we employed another method where the observers used the coordinates of three or more points in each layer within ice cores shaped as half of the core. Using these points, they measured the inclination angle of the layers in 3D, ensuring the orientation was at the point of maximum layer inclination.

At least two observers (in most cases, Miyamoto and Fujita) measured the inclination angles for each layer as a cross-check. Additionally, we repeated measurements several times to gain skills and confirm reproducibility. The observers estimated that the maximum errors in measurement were about 5 degrees. In the year 2006 when the measurements were performed, the orientation of the inclination was not recorded because core orientation was not a topic of interest at that time.

[R2_6] There is no discussion of 3D geometrical relationships. The presentation of inclinations of c axis maxima and visible layers in figure 5 and the written interpretation suggests to the reader that the c-axis maximum and the pole to layering has the same azimuth and lie in a vertical plane. If this is the case, then the cartoon in fig 10c is valid. However, the inclination constraints as presented have an infinite set of possible orientations on cones around the core axis as shown on the stereonet below (Fig. R1). End member solutions where the c-axis maximum and the pole to layering are in the same plane as the core axis (see Fig R1. below) are quite different. An added complication is that the core axis is not vertical, with an inclination of 3 to 6 degrees in this part of the hole (Motoyama et al., 2021), although this angle is small and unlikely to cause significant complications in analysis.

[A2_6] Please refer to A2_2. We confirmed that the horizontal orientation of the c-axes cluster and the normal axis of the layer inclination are within the same vertical plane throughout the LO20%. For each ice core, borehole inclination is known, we will add the data to one of figures. However, orientation of the borehole inclination for each core is unknown.

[R2_7] I think the authors can do much better here. If there is visible layering in the core then you should be able to measure it in 3D; either by extracting the sine curve of the layer from the outside of the cylindrical core (although I realise that the cylinder probably does not exist now), by measuring the apparent inclination on two non-parallel surfaces or by cutting a plane that contains maximum inclination (maybe this is done, as I said before). A great circle (with a point for the pole) can then be plotted on each stereonet along with the COF data (fig 4) to show the orientation of layering to show the true 3D relative orientation of the c-axis maximum and layers in that sample. If layers are hard to see in the core, they are usually easy to see in a 5mm slice viewed in crossed polars.

[A2_7] We agree with the idea. This better procedure needs to be used for future measurements, based on present achievement and understanding.

[R2_8] With 3D data the relative orientations of layers, core axis and c-axis max can be shown for all samples in one stereonet. Below (Fig R2) I present three possible data outcomes (Fig R2 b,c,d) presented in a reference frame where the core axis is fixed (plotted in the centre of the stereonet, as it is easier to see the patterns that way) and the c-axis maxima are all assigned to a common azimuth (arbitrary as you do not have azimuthal data).

[A2_8] Same as above.

[R2_9] This figure could equally be plotted by assigning a common azimuth of the poles to layers - but as drawn it would match better with layers marked on individual sample data in figure 4. The outcome in Fig R2d would be consistent with the generalized cartoon in fig R1b and the geometrical interpretation provided in the paper in fig 10c. The outcome in Fig R2c would be consistent with the generalized cartoon in fig R1c. You cannot draw figure 10c in the paper, nor make any useful interpretation of the c-axis and layer inclinations without this analysis in 3D. Without this data are pretty meaningless.

[A2_9] Please refer to A2_2.

A few other things related to orientations

[R2_10] In Appendix D there is a statement: "We assumed that the IACC consistently develops towards the same horizontal orientation within the ice sheet...". I agree that this is plausible, but I don't think you need to make this assumption. Fig C1b is in the measurement ref frame: the transformation of $\Delta\varepsilon$' to $\Delta\varepsilon$ is simply a function of the vector orientation of the c-axis maximum relative the measurement reference frame. There should be no need to make any assumption about how this varies from one core piece to the next? Appendix C is easy to understand. (although line 801:"horizontally rotating the frame..." is not a good description: "rotating around the core axis to align the c-axis maximum within plane of the electric field vector" would be better). I really do not understand what Appendix D is saying. Is this about correcting measurements between different COF measurements??

[A2_10] It seems to us that there is either some poor writing on our part or unsuccessful communication from the authors to the readers. In the ice sheet, there is a possibility that the inclination angle of the c-axis cluster may rotate with increasing depths if the dome summit position migrates in a complex manner. Although it seems unlikely, we cannot completely deny this possibility. Thus, we required this assumption. This is one aspect.

Another aspect is that when handling many cylindrical ice cores, sometimes the continuity of orientation between adjacent cores is lost, especially when irregular ice core breaks occur. When we perform the DTM measurements, we do not know the core orientation in advance. We can determine the orientation only from the Laue data (or orientation of layer inclination if they were recorded). Only with the assumptions we made were we able to apply corrections from $\Delta\varepsilon$' to $\Delta\varepsilon$.

[R2_11] It is unclear whether there are constraints independent of the core on the layering orientation in the DF hole. Are there televiewer data (Hubbard and Malone, 2013)? Are the radar data (Karlsson et al., 2018; Wang et al., 2023) good enough to extract the dip direction at the borehole site? What are the constraints, other than from the core, on the layer structure shown in cartoon form in fig 10. Knowing the dip direction could turn the data into 3D in a geographic reference frame.

[A2_11] We have no independent data source to determine the orientation of the inclination angles. Additionally, we lack borehole-logging data to detect such orientations. Please note that the literature you provided deals with data from relatively coarse airborne survey lines (typically every 10 km), compared to our ground-based survey map in Figure 1c, which features very dense data within an area of 580 m x 580 m. Figure 1c provides the best geographic information on the subglacial topography at Dome Fuji.

Eigenvector analysis outcomes: error related to the number of grains.

[R2_12] Robustness of the eigenvalues, eigenvector orientation (Inclination angle of c-axis cluster) and median cone half angle (median inclination of c-axis cluster) from the c-axis measurements depends on the number of grains measured. None of the five deepest samples (as shown in fig 4) have enough grains to provide robust eigenvector measurements and points related to these (eigenvalue, Inclination angle of c-axis cluster, median inclination of c-axis cluster) should not be shown on figure 5 or any other figure. I wonder whether other samples with large grain size, particularly between 2860 and 2890 m also have too few grains to make the eigenvector analysis robust.

[A2_12] Thank you for your comment. At a depth of 2860m, where the largest grain size is about 3 cm², a single measurement of a microwave beam (with diameter of approximately 16φ) contains only a few grains. By scanning a 50-cm-long core, at least 50 grains are included in the data. This situation is similar even when we use thin-section methods. When the grain size is about 3 cm², a thin section with an area of 90mm x 50mm contains only about 16 grains. By analyzing a wider area, for example, 500mm x 50mm, we can measure about 80 grains. This situation of large grain size is common for analyzing the deep zones of the summit ice cores at Dome Fuji and EDC.

[R2_13] Error analysis for eigenvectors is tricky - a simple way to do this is, is to recalculate the eigenvectors for different randomly selected subsets of the full data set. Since all of the c-axis patterns (where there are enough data) seem to be single maxima I suggest that you do this with one sample to give a representative value for errors of all the eigenvector derived numbers for all samples. Pick a data set with many grains (e.g. the data in fig 4 b). Compare the eigenvector results for ten randomly selected sets of n grains from this larger data set. Use n values from 10 to 200 (in increments of 10?) to get an idea of the errors for the parameters you show, as a function of the number of grains in the data set. Then all the parameters derived from eigenvector analysis can she shown with error bars corresponding to the number of grains measured.

[A2_13] In our preprint, we did not perform a derivation of the error range for the COF data obtained through thin-section methods. Generally, if the total number of grain samples is N (in our Laue example, ranging from about 50 to about 250), the addition of a grain means the normalized eigenvalue can vary by about 1/N at maximum. When c-axes are clustered, as in the Dome Fuji core, the range of variation will be much smaller than this 1/N. Assuming each grain contributes an eigenvalue between 2/3 and 1 (see Figure 5a), the range of variation will be 1/(3N) or less. Therefore, the total number of grains is very important. The error in the eigenvalue is on the order of 1/(3N) or less.

[R2_14] You should show all of the stereonets of c-axis and <a>-axis data (in an appendix?) and tabulate the measurements (e.g. eigenvalues) for all the samples. The paper refers to fabric analyser data as well as Laue data. Where are these data? They should be included.

[A2_14] In revision, we plan to present all the data in Appendix or Supplementary information.

Elongate [c] axis patterns

[R2_15] The c-axis maxima in fig 5 are not all circularly symmetric. Subfigs a,b,d,e,g, and h are all elongate. You should highlight this observation. This is important because all laboratory experiments in shear give elongate c-axis maxima (Bouchez and Duval, 1982; Journaux et al., 2019; Kamb, 1972; Li et al., 2000; Qi et al., 2019; Wang et al., 2024) - with elongation perpendicular to the shear direction. The elongation is less clear in natural samples from shear zones: it is present in the Whillans shear margin (Jackson and Kamb, 1997) but absent in other studies (Disbrow-Monz et al., 2024; Monz et al., 2021; Thomas et al., 2021).

[A2_15] We agree that it is important to comment on the elongated single pole fabric. As previously mentioned in papers by Azuma et al., 2000 and Fujita et al., 2006 (DOI:10.3189/172756506781828548), this phenomenon is observable even in dome summit areas of polar ice sheets, where ice does not flow perfectly radially from the dome but undergoes deviatoric strain depending on orientations. Such summit areas often feature ridge-like ice divides, which cause uneven strain along two principal orientations. Because this elongated single pole fabric is already observable at shallow depths, it should not necessarily be linked to the presence of simple shear. Simple shear is only one of the possible causes mainly in deeper zones; the phenomenon can be explained without it.

In a paper on polarimetric radar sounding at Dome Fuji by Fujita et al., 2006, the authors discuss the amount of radio wave birefringence caused by this elongated single pole at the Dome Fuji drilling site, up to depths of about 2200 m. They demonstrated that the orientations of the elongated single pole are consistent at least to this depth. Figure 1b in this paper by Fujita et al. (2006) shows two principal axes of the elongated fabric, inferred from polarimetric radar sounding. These axes are comparable to the bedrock topographic map shown in Figure 1c of our preprint. We observe that the two principal axes tend to align with the orientation of the subglacial slope (WNW) and its orthogonal direction. Below, we present two principal axes.

According to View 3 in Figure 4 of the preprint, in many cases, the elongation of the single pole includes the maximum inclination, from Figures 4a to 4h. At depths deeper than about 2860 m, the feature of elongated single pole is almost lost, which is another aspect we should comment on in the revised paper.

In summary, and importantly, the elongated single pole is not necessarily due to shear.

Additionally, we have reviewed the reference papers introduced by the reviewer and have created a table to present the experimental setup as follows.

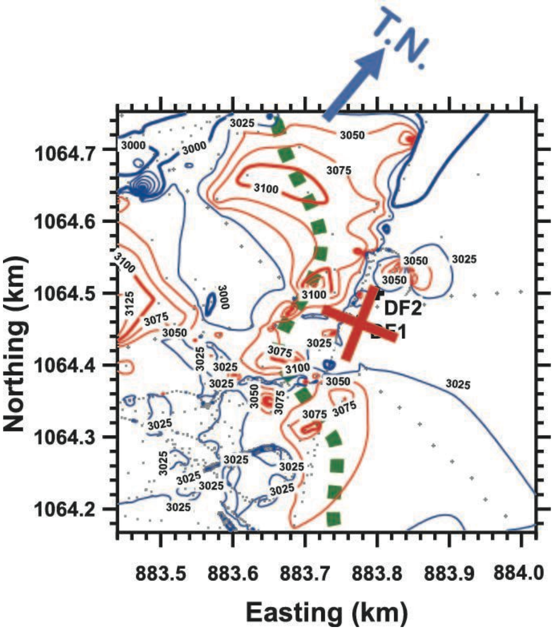

Figure A2_15. On the Figure 1c in the preprint, two principal axes of elongated single pole fabric inferred from the polarimetric radar sounding (Fujita et al., 2006) was indicated.

Table A2_15: Summary of the key experimental conditions for studies introduced by the reviewer.

Paper	Ice type	Type of deformation	Temperature	strain rate
Bouchez and Duval, 1982	The polycrystalline samples obtained by filling a cylindrical container with snow, saturating the snow with previously boiled water	constant-stress torsion experiments	–8 ~ –12 ℃	from 10^{-7} to 10^{-6} sec^{-1}
Journaux et al., 2019	Unstrained equiaxial polycrystalline ice prepared by evenly packing 200μm sieved ice particles in a mold	Torsion experiments	− 7 ± 0.5 ℃	from 10^{-7} to 10^{-6} sec^{-1}
Kamb, 1972	Ice prepared by filling a container with fresh snow, saturating the snow with water, and allowing the water to freeze	Torsion experiments	0 ~ –5 ℃	~10^{-7} sec^{-1}
Li et al., 2000	Polycrystalline with initially random crystal orientations and a mean crystal area of about 1.2 mm^2	Torsion experiments	–2.0 ℃	from 10^{-7} to 10^{-8} sec^{-1}
Qi et al., 2019	polycrystalline ice samples with a controlled initial microstructure	Shear experiments	–5, –20 and –30 ℃	~10^{-5} sec^{-1}
Wang et al., 2024	synthetic ice (doped with ~1 vol.% graphite)	Equal-channel angular pressing technique	− 5 ℃	not specified
Jackson and Kamb, 1997	The ice was obtained from a depth of 300 m in borehole, by means of a hot-water ice-core drill	unconfined uniaxial compression	− 22.0 ± 0.1℃	~10^{-8} sec^{-1}
Disbrow-Monz et al., 2024	Unavailable for access because it is in press			
Monz et al., 2021	polythermal valley glacier ice close to melting point		close to melting point	
Thomas et al., 2021	A 58m long azimuthally oriented ice core collected from the floating lateral sinistral shear margin of the lower Priestley Glacier, Terra Nova Bay, Antarctica		0 ~ –20 ℃	

In some of the suggested papers on laboratory experiments conducted at temperatures near the melting point, the numbers for strain rate are larger than those in the Antarctic ice sheet by about 10^5 to 10^7 times. Additionally, ice samples from glaciers also come from temperature conditions close to the melting point. Our concern is that to draw analogies with dome ice cores, obviously extensive discussions—and possibly even entire papers—are necessary. This complexity makes it difficult for us to cite these papers in a straightforward manner without issuing cautions. It seems unlikely that ice deformation with strain rates differing by 10^5 to 10^7 times would result in the same ice fabric. Time-dependent factors must be argued extensively, to draw analogies with dome ice cores.

[R2_16] It would be good to know whether this is a parameter that changes between the UP80% and the LO20%. I cannot find any c-axis stereonets related to UP80% (Saruya et al., 2022) in although I note that samples 1720m and deeper in DF1 (Azuma et al., 1999) have elongate c axis clusters.

[A2_16] Please refer to [A2_15] above. In the uppermost 2200 m, the principal axes of the ice fabric development are known in terms of the geographical orientation. However, we have no direct evidence that the principal axes in the uppermost 2200 m keep continuous orientation in the deeper depths.

<a> axis data

[R2_17] The <a> axis data are not well used. They are, in part, hard to evaluate because the quality of figure 4 is poor (this figure needs to be better). You say in the text that the <a>- axes girdles have no maxima - I don’t believe that, it looks to me like some of the girdles contain maxima. Fig 5 g, h and I all look to have maxima in the middle of the stereonets in the corrected view. Remember that the maximium value for any measure of <a> axis orientation density must be less than 1/3 the maximum value for the [c] axis maximum, so <a> axis patterns will always look more subtle than [c] axis patterns. The hexagonal repeat of <a>-axes renders an eigenvector analysis uninformative. You need to present <a>-axes in a contoured form to evaluate them. To compare <a> axis and [c] axis patterns they need to be contoured independently, so that both are scaled to a maximum value for that crystal direction.

[A2_17] The author members engaged in discussions on this point before submitting the preprint. Our views are as follows: We have conducted calculations to create histograms of the orientation distribution along the a-axis for all samples. Here, we examined whether there are any dense or sparse features in the distribution of the a-axis by dividing the data into 18 sections at 10-degree intervals. The results show that, fundamentally, the ice's a-axes have some form of non-uniform distribution, and since there are three equivalent a-axes, a 60-degree periodicity inevitably appears. However, we recognized that there are no signs that this 60-degree periodicity systematically indicates any structure. In some samples (such as at a depth of 2730 m), the sparseness interspersed with the 60-degree periodicity results in what appears to be three peaks. However, the depths at which this occurs are random, and there is no correspondence to glacial or interglacial periods. Rather, we recognized that this seems to be more appropriately considered a case of random bias. Apart from the few cases mentioned, no structured 60-degree periodicity was observed, even at depths where recrystallization is predominant. Based on this understanding, the description in the current preprint was deemed appropriate. Highlighting the sample at 2730 m deep, as mentioned above, would impose a bias as if there were meaningful significance in what is merely a random variance element. We plan to add these points in Appendix of the revised paper because these will be one of interesting points.

[R2_18] This is pretty important as a distinct <a> axis maximum would add strength to the inference that these are COFs related to shear. All shear experiments where <a>-axes have been measured have a preferred <a>-axis maximum (Journaux et al., 2019; Qi et al., 2019; Wang et al., 2024). Natural examples from sheared ice (Disbrow-Monz et al., 2024; Monz et al., 2021; Thomas et al., 2021) also have a preferred <a>-axis maximum, although in the case of Thomas et al it is variably developed.

[A2_18] Please refer to [A2_17].

[R2_19] If you do have a -axis maxima they could also be plotted on a figure like Fig R2 or Fig R3. How to do this would depend on the nature of the data, but I am certain that they would be useful.

[A2_19] Please refer to [A2_17].

[R2_20] I think your Laue data are one point per grain? In this case there will be insufficient data to calculate crystal vorticity axes (Kruckenberg et al., 2019; Michels et al., 2015). It would be worth thinking about collecting full crystal orientation data at higher density (EBSD data) to enable crystal vorticity axis (CVA) analysis. Obviously I'm not suggesting you need to do this for this paper- an idea for the future. (Thomas et al., 2021) have very clear CVA maxima that are consistent with the dominant simple shear that is also constrained from other data- with the very strong single point maxima you have a CVA analysis may provide excellent constraint on deformation kinematics.

[A2_20] Yes, Laue data are obtained by one point per grain. This method can determine the orientation with a resolution of less than 0.5 degree (Miyamoto et al., 2011), so that misorientation angle within a grain can be estimated. We would like to challenge CVA analysis in the future.

Relevant comparative data from the literature.

[R2_21] A key part of the paper is about what might be controlling the COF, including kinematics mechanisms and conditions. Excellent constraints on these come from laboratory experiments and field studies where deformation kinematics and/or conditions are constrained. There is virtually no reference to this extensive literature. I have already related (in the previous two sections) a couple of your observations to the literature for COFs from experiments and kinematically constrained. This is sorely needed if we are to understand ice cores such as DF where there are no measured constraints on deformation kinematics (the thinning model is not a measured constraint- it is a model with imposed kinematic assumptions).

[A2_21] Please refer to the latter half of [A2_15] and Table A2_15. To discuss the analogy between these examples and dome ice cores, obviously extensive discussions (even entire papers) are necessary. This complexity makes it difficult for us to cite these papers in a straightforward manner without numerous cautions. It seems unlikely that ice deformation with strain rates differing by 10^5 to 10^7 times would result in the same ice fabric.

[R2_22] A good starting point is the fact that your c-axis patterns are very tight single maxima, with most of them close to vertical. This is common in palaeo-climate focused ice cores (Faria et al., 2014). Many of the papers that describe these data infer that the primary cause of the vertical tight c axis maximum is vertical uniaxial compression due to lattice rotation. This is entirely at odds with the experimental data. There are no uniaxial experiments published that have tight c-axis maxima parallel to compression. Virtually all uniaxial experiments (my list of refs is just a subset) have open cones (small circle distributions) with the cone axis parallel to compression (Budd and Jacka, 1989; Fan et al., 2020; Hunter et al., 2023; Jacka and LI, 2000; Jacka and Maccagnan, 1984; Montagnat et al., 2015; Qi et al., 2017; Vaughan et al., 2017). Single maxima can form in uniaxial compression at high stress corresponding to high strain rates and or low temperatures (Fan et al., 2020; Qi et al., 2017) but these are weak maxima, tight maxima never form under these conditions. Lower stresses, as expected in nature, would tend towards open cones. Tight single maxima are observed in experiments, but only in shear, where they form normal to the shear plane (Bouchez and Duval, 1982; Journaux et al., 2019; Kamb, 1972; Qi et al., 2019; Wang et al., 2024).

[A2_22] Based on the comment by the reviewer, we checked all papers mentioned above.

Table A2_22: Summary of the key experimental conditions for studies introduced by the reviewer.

Paper	Ice type	Type of deformation	Temperature	Strain rate	Type of ice fabric
Budd and Jacka, 1989	Laboratory-prepared ice samples with initially consisted of randomly oriented crystals.	Uniaxial compression tests	–3.0°C	~ 10^{-8} sec^{-1}	Small circle girdle (= cone)
Fan et al., 2020	Polycrystalline ice samples with a controlled initial microstructure	Uniaxial compression tests	–10, –20 and –30°C	~10^{-5} sec^{-1}	At temperatures warmer than –20°C, CPO is a cone (i.e. small circle) around the compression axis.
Hunter et al., 2023	Polycrystalline D2O ice prepared using the technique described by Wilson and others (2019).	Pure shear and simple shear	Mostly above –10°C	from 10^{-5} to 10^{-7} sec^{-1}	Cone
Jacka and Li, 2000	Laboratory prepared initially cylindrical (60 mm long; 25.4 mm diameter), with randomly oriented, small-grained crystal structure	The compression tests with constant stress	–10 to –45°C	from 10^{-7} to 10^{-8} sec^{-1}	At higher temperatures and stresses, the dominant deformation mechanism is recrystallization, while at lower temperatures and stresses crystal rotation may be more important.

Jacka and Maccagnan, 1984	Laboratory-prepared ice samples with initially consisted of randomly-oriented crystals.	Uniaxial compression tests	–3.0°C	~ 10^{-8} sec^{-1}	Small circle girdle (= cone)
Montagnat et al., 2015	The ice polycrystals were made from sieved seeds within a controlled size range	Uniaxial compression tests	−5 or−7°C	more than 10^{-7} sec^{-1}	cone
Qi et al., 2017	Fabricated ice samples with starting average grain sizes of either 0.23 mm or 0.63 mm	axial compression	−10°C	from 10^{-4} to 10^{-6} sec^{-1}	A transition from cone to cluster CPO pattern with increasing stress.
Vaughan et al., 2017	Synthetic polycrystalline ice	Uniaxial compression	–5°C	10^{-6} sec^{-1}	cone

We noticed that almost all of these cited laboratory experiments were performed under temperatures very close to the melting point (between –3.0°C and –10°C), and the strain rate is extremely faster compared to deformation in the inland ice sheet. There is no experimental setup listed that can reproduce the temperature conditions of the inland plateau (–50°C or lower). Jacka and Li (2000) have already stated in their abstract, '*The results support the conclusion that at higher temperatures and stresses, the dominant deformation mechanism is recrystallization, while at lower temperatures and stresses, crystal rotation may be more important.*' This view explains why under high temperatures, a cone can appear. The reviewer's comments are partly based on comparisons between these deformation tests close to the melting point and much colder polar ice. In this case, our view is that the comparison is not straightforward. If comparison is between these laboratory-based studies and the more temperate ice within the LO20%, we understand the phenomena as follows.

Once the cluster strength of the single pole fabric reaches its maximum at the bottom of the UP80%, the temperature of the ice is still approximately –20°C. Thus, empirical knowledge based on laboratory measurements (as listed above) is more directly related to temperature conditions at depths deeper than approximately 2700m, where the ice temperature is about –10°C. Recrystallization will create new grains favorable for the maximum shear stress of the vertical compression (cone with wide angles). The c-axes within the cone continuously rotate toward the vertical. Then, a cone-like fabric, or alternation from it, such as less clustered single pole, is reasonably formed. This is entirely in agreement with our observations of COF in that very zone.

We note that this kind of phenomenon was already mentioned by Budd and Jacka (1989). We cite this here.

If single maximum fabric ice is subjected to sustained stress of some other configuration, e.g., compression (uniaxial or confined plane strain) in the direction of the single maximum, then the fabric can change, e.g., to spread and form girdles or multiple maxima (cf. Fig. 3(e)). This can occur in ice sheets, particularly near the base. The 'overprinting' of fabrics from different stress configurations in ice sheets is also similar to the results obtained from laboratory studies of combined shear and compression, such as the torsion experiments of Duval (1981), or the unconfined compression of Azuma and Higashi (1984).

Along with the views of Jacka and Li (2000) as mentioned above, there is no contradiction if we consider the temperature range of natural phenomena in the ice sheet and the laboratory experiments. In revision, we plan to cite papers in the list explaining these laboratory data agree with the observation in very deep part of the ice sheet at Dome Fuji.

There is another point which we hope will draw the readers' attention. In polar ice sheets, at the bottom of the firn zone where bubble close-off ends, the c-axis is already clustered by metamorphism processes in the firn. This is documented in section 5.4 of the preprint. The normalized eigenvalues of the DF cores at ~100m depths (bubble close-off depth) are about 0.4 (Fujita et al., 2009, 2016). At NEEM Camp in Greenland, the normalized eigenvalues at bubble close-off depth are about 0.5. These high values, caused by the c-axes cluster, are the initial values for further deformation. Randomly oriented fabric is often used as an initial stage of ice in laboratory experiments. However, in polar ice sheets, COF with a vertically clustered tendency is the initial state for deformation. This means that the resultant ice fabric will be different from those developed from random orientation.

In summary, there are two major differences between the laboratory experiments and polar ice sheets. One aspect is physical conditions, including temperature and strain rate. The other is the initial state of COF for compression deformation (random or already clustered to some extent).

[R2_23] I think that some general discussion as to why you have tight single maxima, with reference to the experimental literature is needed. I suspect it is the dominance of shear on a shallow shear plane at all depths. I don't think there are many direct measurements of deformation kinematics in deep ice boreholes: (Treverrow et al., 2015) show that shear strain rate (on shallow shear plane) is much vertical shortening at all depths in the Law Dome boreholes.

[A2_23] We already provided our view on this point. When dislocation creep is dominant, tight single maxima can appear without the presence of simple shear. This view was already given for example, by Azuma and Higashi (1985) based on numerical simulation and Jacka and Li (2000). Recrystallization which occurs dominantly under temperatures near the melting point (typically above about –15 °C) is another problem.

[R2_24] My knowledge of the geometry of c-axis maxima relative to shear kinematics together with some basic knowledge of structural geology leads me to suggest that he cartoon in figure 10c (if it is correct: see section on layering) requires a sense of shear opposite to what is alluded to by the authors. In shear, the c -axis maximum will remain perpendicular to the shear plane as seen in all ice experiments (Bouchez and Duval, 1982; Journaux et al., 2019; Kamb, 1972; Li et al., 2000; Qi et al., 2019; Wang et al., 2024), with no significant rotation as a function of shear strain. In shear, layering will rotate so that the pole to layering will be aligned with the short axis of the finite strain ellipsoid (Fossen and Cavalcante, 2017; Hudleston, 2015; Jennings and Hambrey, 2021). These two relationships, shown on the lev side of the Fig R4, are consistent with observations from ice shear zones (Hudleston, 1977; Thomas et al., 2021). On the right side of the figure I have rotated the picture to match the reference frame of fig 10c.

[A2_24] Please look at the figure here and the explanations for it. We hope that this explanation makes sense for readers. The simple shear in principle contains components of compression, extension, and rigid-body rotation of the system. This is a firm basis. Progressive increase in simple shear with increasing depth is a key point.

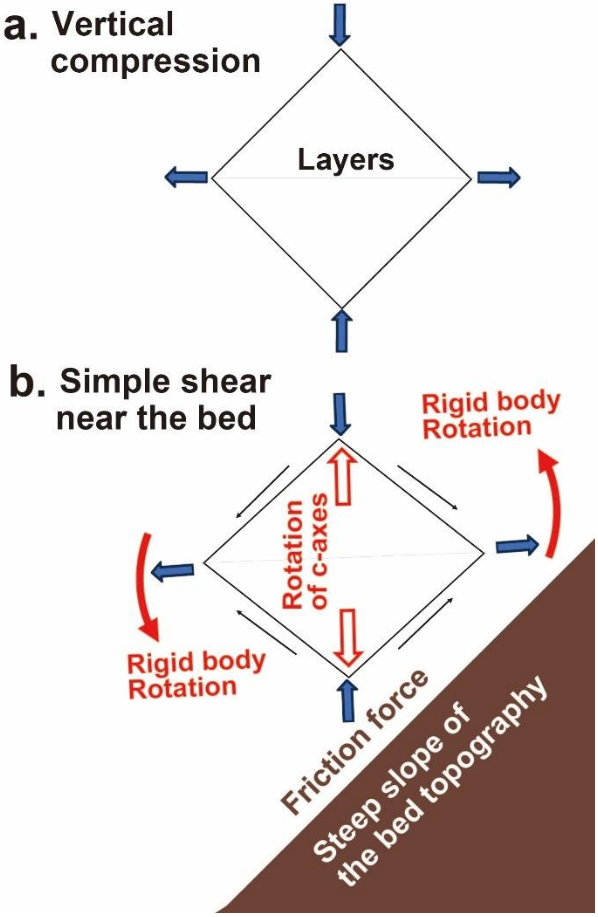

Figure A2_24. 2D schematic explanation for configuration of strains and rotations in the ice body above the steep bedrock slope. Rectangular shape means a body of ice considered for deformation. Black thin arrows and blue bold arrows mean components of shear strains and normal strains. Bold red arrows indicate rigid body rotation of the system. Open red arrows indicate the axis toward which the cluster of the c-axes rotate. (a) A condition well above the bed. Uniaxial compression in the vertical dominates. (b) A condition under more influence from the bed topography. Because of the friction force between ice and bed, simple shear system rotates the entire system including the internal layers and the c-axes cluster together. However, because of the normal components of the strains (both compression in the near-vertical and extension near the horizontal plane), the all the c-axes thus the c-axes cluster rotates toward the vertical at the same time. This mechanism of the rotation in counter-direction works only for the c-axes and not to the layers. In this way, total amount of inclinations becomes larger for the internal layers than the cluster of the c-axes.

Microstructures and their interpretation

[R2_25] Section 4.5 is not too bad, although there are some unintelligible bits (It is noteworthy that the concentration of the less impure ice is markedly smaller than in impure ice??). The key issue in this section is to make clear in Fig. 6 what are grain boundaries and what are sub-grain boundaries. The arrows are not good enough. I would suggest that an additional column is added with the photo from column 3 repeated, but with an overlay with three different coloured lines for boundaries on the top side, boundaries on the bottom side and subgrain boundaries. The description of boundaries being concave towards complex subgrain boundaries is not correct, wishful thinking I suspect. In both examples cited in the text (a and b in Fig 6) there are both concave and convex boundary segments adjacent to the subgrain structures.

[A2_25] Thank you for the comment. Yes, the impurities concentration of the less impure layer (shown in Fig.6) is very much smaller than that of the impure layer. We meant that the contrast or difference was quite large. Please look at the concentration of dust particles indicated in the main text. Additionally, we will attempt to make new figures illustrating grain boundaries and subgrain boundaries (next to micrographs or in Appendix). We plan to modify the statement to avoid readers' misunderstanding regarding the shape of grain and subgrain boundaries.

[R2_26] I think you need to locate the micrographs in column 3 of Fig 6. on the micrographs in a and b. This is important for the reader to assess whether boundaries are grain or subgrain boundaries. The only one I can locate myself is b. The "subgrain" structures highlighted in b are weird. I've never seen subgrains like this in any naturally or experimentally deformed ice. I have seen grain boundaries like this and this could be a grain boundary with c-axes closely aligned but a-axes misaligned by 10-15 degrees?

[A2_26] Thank you for the comments. We will add squares in column 1 and/or 2 indicating the location of micrographs in column 3. Subgrain boundary in panel (b) is indeed curious. Since we can find microparticle (or subgrain boundary?) pinning, it would not be an artificial error. Unfortunately, we cannot estimate the misorientation angle around this subgrain boundary. It will be very interesting how this sugbrain boundary grows.

[R2_27] Observations from sections 5.3.1 and 5.3.2. should be incorporated into the descriptions in section 4.5, so all the microstructural observations are together. Section 5.3.1 and 5.3.2. are both really poor: microstructural observations and interpretations are oven intermixed and many features are described in interpretive terms (e.g. "migrating boundary" used as a description). This is poor science as the paper omits clear microstructural observations (facts that will never be wrong) that enable future researchers to build new interpretations on these. Sorry I have run out of time to make my comments clearly structured so the following sections might be a bit of a mess. There is a lot to comment on as these microstructural sections need extensive re-evaluation and re-writing for both the observations and the interpretations.

[A2_27] Thank you for the critical comments. We plan to combine Sections 5.3.1 and 5.3.2 with Section 4.5 and reduce the discussion. We agree that the 'observation' and 'interpretation' must be distinguished. We will be careful on that point. In the present study, microstructures are positioned as supporting information for the discussion of COF and layered structures.

[R2_28] I fail, to see the "brick wall" patterns you describe. You need to show a lower magnification microstructure, with many more grains for the brick wall structure to be convincing: I doubt it will be from what I can see. None of the micrographs in Fig 6 or in Fig 12 look like Fig. 9 in Faria et al, 2009. (Weikusat et al., 2017) does not show "brick wall" patterns but infers their possibility based on very high grain elongation data: I can't see mean elongations that compare with Weikusat (fig 4c) or individual grain elongations that compare with Weikusat (fig 4) that justify a comparison. The other cited reference (Kuiper et al., 2020b) does not use the term "brick wall" (nor does (Kuiper et al., 2020a)- this is a better comparison to your data as it is the deep part of NEEM).

[A2_28] In the revised manuscript, we will redesign the example of the brick-wall pattern to provide clear features. Polarized pictures in Fig.6 are provided as a comprehensive image (with low magnification). As the reviewer noted, Weikusat et al. (2017) and Kuiper et al. (2020) observed elongated grains but did not use the term "brick-wall pattern". We will refer to EDML and NEEM ice cores as similar microstructures (elongated grains) in the impure layers. In the revised manuscript, we plan to include the aspect ratio of the short and long axis of a fitted ellipse data obtained through microstructural analyses. The aspect ratio in the impure layer ranges from 1.9 to 2.0, while in less-impure layers, it ranges from 1.5 to 1.7. In deep impure layers, it ranges from 1.5 to 1.6. Kuiper et al. (2020) reported that flattened grains in the glacial ice had an aspect ratio of about 2:1. This value is similar to our results.

[R2_29] The inference of microshear processes based on the description provided is unjustified. What is the evidence for shear being localized? I can believe, as a general assumption, that it may have been localized but you show no clear evidence. A shape fabric does not mean localization unless it varies in orientation reflecting variable strain (Hudleston, 1977, 2015; Jennings and Hambrey, 2021; Ramsay, 1980). The grain shape fabric provides really useful information about deformation kinematics – you should use it. Grain long axes at 2648m (fig 6a) are ~ 60-70 degrees clockwise of the vertical on the picture. If the c-axis max is vertical in this pic, then this would imply shear with top to the right (Bouchez and Duval, 1982; Fossen and Cavalcante, 2017; Hudleston, 1977). The shape fabric is another thing that would be very usefully oriented relative to layering and the COF.

[A2_29] We have introduced the term 'localized microshear,' citing features of the brick-wall pattern in the preprint. In this type of layered alignment of grain shapes, we can presume that shear is localized at chains of grain boundaries or at junctions between layered grain boundaries and those with different, for example, orthogonal angles. This seems reasonable to us, and we believe that our presumption is valid. In contrast, the reviewer's comment is that it is not justified because we did not provide any clear evidence for the localization of shear. For us, chains of brick-wall-like aligned grain boundaries are at least circumstantial evidence. The last four lines that the reviewer provided are related to the presence of shear on larger spatial scales. Comparing ice features at a dome with laboratory measurements (Bouchez and Duval, 1982) is not straightforward because the laboratory measurements use extremely fast strain rates compared with ice at the dome. It can be one of the references, but it is not suitable to use it as a firm basis for discussions. In summary, our view is that chains of brick-wall-like aligned grain boundaries are at least circumstantial evidence, which we will present in the paper.

[R2_30] The paper by Faria et al, 2009 predates a lot of work that relates to the GBS process in rocks and ice. Rock deformation studies report small recrystallized grains have CPOs that are randomly dispersed equivalents of the stronger parent grain COFs (Bestmann and Prior, 2003; Jiang et al., 2000; Warren and Hirth, 2006) (and many more recent papers). These observations were interpreted as the result of an increase in the contribution of GBS in fine grains. (Craw et al., 2018) and (Fan et al., 2020) reported similar observations in uniaxially deformed Antarctic ice and synthetic ice respectively, and the reduction of COF intensity in grains with finer sizes was attributed to GBS. (Fan et al., 2020; Qi et al., 2019; Wang et al., 2024) all infer that strength of COF in experiments is a competition between grain boundary migration (strengthening COF) and "rotation" processes, where those rotation processes include lattice rotation related to dislocation creep and recover and grain boundary sliding: following broadly ideas outlined in (Alley, 1992).

[A2_30] In the present preprint, we did not compare the textural features of ice with the GBS process in rocks or laboratory-based ice deformation tests because such a comparison would not be straightforward. The reviewer introduced many reference papers about rock deformation studies that report small recrystallized grains have CPOs that are randomly dispersed equivalents of the stronger parent grain COFs. We believe that we need papers that compare rocks and ice sheet ice at the dome, including comparisons of the physical properties of each substance, phase diagrams, or deformation mechanism maps in terms of temperature and strain rate. We did not understand the latter half of the comment. The reviewer commented that 'the strength of the COF in experiments is a competition between grain boundary migration (strengthening COF) and "rotation" processes.' It seems to us that the rotation process also strengthens the COF. Indeed, we did not observe any small grains such as those the reviewer mentioned here. Additionally, when comparing the ice sheet and laboratory-based deformation tests, the latter have conditions with extremely high strain rates. It seems that making straightforward comparisons without a lot of caution is difficult.

[R2_31] The listed controls on GBS are not the most important. The prime drivers of whether GBS is important are likely to be grain size and stress; if you scale grain size sensitivity from experimental data (Goldsby and Kohlstedt, 1997, 2001; Qi and Goldsby, 2021) to natural grain sizes and stress (Kuiper et al., 2020a; Kuiper et al., 2020b) then a significant GBS contribution is predicted. Impurities, in small volume fractions, likely have a secondary effect through restricting grain growth (Fan et al., 2023; Qi et al., 2018), keeping grain sizes small.

[A2_31] Thank you for the comment. Based on the literature, we agree that grain size is likely an important condition. We plan to rewrite the statement at L532-533 ('A combination of high impurity content, temperature, and moderate stress causes GBS.') as '*A condition favorable for the occurrence of GBS is likely a combination of smaller grain size, the presence of significant stress, and higher temperature. Smaller grain size is often achieved by the presence of solid impurities such as dust particles.*' The reviewer also suggested a scale comparison between laboratory-based experiments and the Dome Fuji data. However, estimating the quantitative contribution of GBS would lengthen and complicate the discussion, and it is beyond the scope of the present study.

[R2_32] Nucleation. We only know that nucleation must occur by analogy with experiments where the number of grains increase with strain (Fan et al., 2020) and in zones of localized deformation where grain size reduces (common in silicates and carbonates: few observations in ice). We know virtually nothing about nucleation: two possible mechanisms are proposed: sub grain rotation recrystallisation and bulging nucleation and inferring these processes are difficult (Craw et al., 2018; Fan et al., 2020; Urai et al., 1986). Spontaneous nucleation in random orientations has been suggested (Chauve et al., 2017; Falus et al., 2011) but the physics of this process remains unconvincing to me. The evidence from experiments is that grains with orientations where the shear stress is high on the basal plane will grow (Fan et al., 2020; Qi et al., 2017; Qi et al., 2019) during strain induced GBM at the expense of other grain orientations. The grains that grow are already established and their nucleation is irrelevant to this process. Identifying nucleated grains is very hard: we can’t do it in a natural samples.

[A2_32] What we know about the ice sheet is as follows: (i) Despite the older and deeper ice, the number of grains per volume of ice does not decrease significantly. (ii) The clustering of c-axes becomes less pronounced as we go deeper. Our view is that these phenomena can be explained by the nucleation of grains with angles well away from the major axis of the c-axes cluster. Of course, we are not observing a process where nucleation is just occurring. However, we cannot claim that we know virtually nothing about nucleation based on this condition alone. Two possible mechanisms proposed, subgrain rotation recrystallization and bulging nucleation, will not significantly change the COF. Nucleation with crystal orientations favorable for further dislocation creep has been suggested (e.g., Alley et al., 1992, JoG; Cuffey and Paterson, 2010). The reviewer’s view is, 'The grains that grow are already established, and their nucleation is irrelevant to this process.' It seems to us that this view is in disagreement with our knowledge stated as (i). We agree that identifying nucleated or nucleating grains is very difficult. However, the fact of an increasing rate of grains with a wider angle of c-axes cluster itself suggests they are nucleated grains. It is likely that in the long history of the ice sheet ice, spanning up to about 10^5 years, nucleation and subsequent grain growth at the expense of other grains with orientations unfavorable for the continuation of the deformation occur relatively rapidly without giving a chance for the observers to identify the moment of growth. In the revised manuscript, we plan to provide microstructure images indicating “grains with nucleation potential”.

[R2_33] Lobate and highly irregular boundaries are the observation oven used to infer that grain boundary migration has been an important process (Rollett et al., 2017; Urai et al., 1986). The micrographs you present do not have strongly curved/ irregular/ lobate boundaries. Compare them for example with boundaries of grains in micrographs of the NEEM core (Weikusat et al., 2017). I see no highly lobate boundaries that would suggest that strain induced grain boundary migration was a dominant process. The grain boundaries are slightly curved. This needs to be the basis for this discussion. Telling the past direction of grain boundary migration is fraught with difficulties (Jessell, 1986, 1987). You have not presented what the observations are that allow you to infer migration directions. I don't think these inferences are robust.

[A2_33] We agree that the microstructural examples in the preprint are not sufficient to illustrate migration recrystallization. In the revised manuscript, we will redesign microstructure images to provide clearer examples (more lobate and irregular boundaries.). Additionally, we will remove the description about boundary migration direction. (The direction of boundary migration is not so important in this paper.)

[R2_34] The photos of particles on grain boundaries are very nice. I'm not sure they help particularly in the analysis- they are expected. They might be better in a short paper focused on that topic.

[A2_34] In the present study, we provide this as an interesting microstructure, but a more detailed analysis will be carried out in the future.

[R2_35] A key set of conclusions relate processes that control microstructures and COF through the whole core, with emphasis on the difference of the bottom 20% from the upper 80%. I cannot find any microstructural descriptions or micrographs of the upper 80% in this or any other paper. Similarly, I can find no directly measured COFs (Fabric Analyser, Laue, EBSD) for the upper 80% that allows the inclusion of COF shapes in the discussion. The dielectric tensor data in (Saruya et al., 2022) reduces the proxy COF to eigenvectors and loses shape and symmetry information.

[A2_35] Microstructures down to 2500 m have been analyzed by Azuma et al. (1999, 2000), but the DF1 ice core was used for these papers. Upper 80% of the DF2 ice core is not analyzed in terms of the thin-section-based methods. In Saruya et al. (2022b), we compared DF1 eigenvalues obtained from thin section measurements (Azuma et al., 1999, 2000) and our results from dielectric tensor measurement. Their results were approximately consistent. Therefore, we presume that the upper 80% of the DF2 ice core and DF1 ice core have the same COF. (DF1 and DF2 boreholes are 44 m apart).

[R2_36] Section 5.4 is not very good. You cannot have dislocation creep without both dynamic recovery and dynamic recrystallisation mechanisms. At the high homologous temperatures throughout the ice core (TH >0.8) it is inconceivable to have no dynamic recrystallisation. The key factor is the relative contribution the sub processes that are all needed for dynamic recrystallisation to occur: sub-grain rotation recrystallisation (following from recovery and sub-grain rotation: see nomenclature in (Trimby et al., 1998)), bulge nucleation, and strain induced grain boundary migration. See interpretive sections of (Fan et al., 2020; Qi et al., 2017) for example.

[A2_36] We agree that the description in Section 5.4 is not good enough. The contribution of dislocation creep and recrystallization could be changed continuously with increasing depth. We will emphasize that the development of $\Delta\varepsilon$ values changes significantly after around 2400 m (from approximately increasing to remaining high and large fluctuation). Deletion of Section 5.4 is another possibility.

Temperature

[R2_37] It is clear that temperature is an important parameter in controlling ice behaviour. Fig 5 needs temperature data from the hole and any figure covering the whole hole depth (e.g. fig 9) needs this as well. I see that there are measurements in (Motoyama et al., 2021) and broader modeling by (Obase et al., 2023).

[A2_37] Temperature at deep sections increases almost linearly and representative borehole temperatures are shown in the upper side of Figure 9. We will add temperature profiles in appropriate figures.