Review of Saruya et al.,: "Development of deformational regimes and microstructures in the deep sections and overall layered structures of the Dome Fuji ice core, Antarctica"
By David Prior, University of Otago

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.

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.

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.

Specific comments.

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

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.

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.

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.

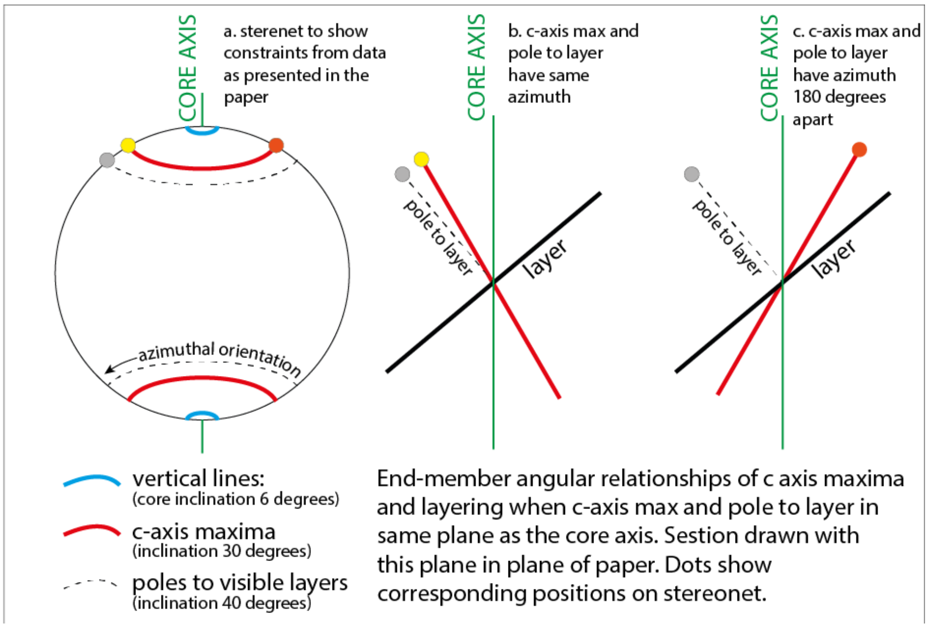


Fig R1. Possible 3D relationships of c-axis maxima and poles to layers

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.

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).

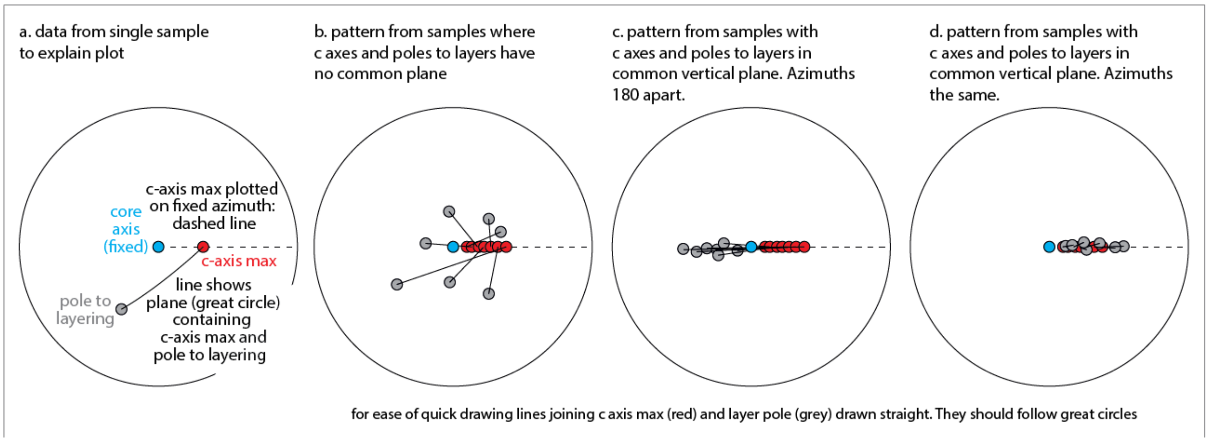


Fig R2. Possible outcomes of measured 3D relationships of c-axis maxima and poles to layers

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.

A slightly simpler way of plotting Fig R2 is shown below in Fig R3. This may be easier to read.

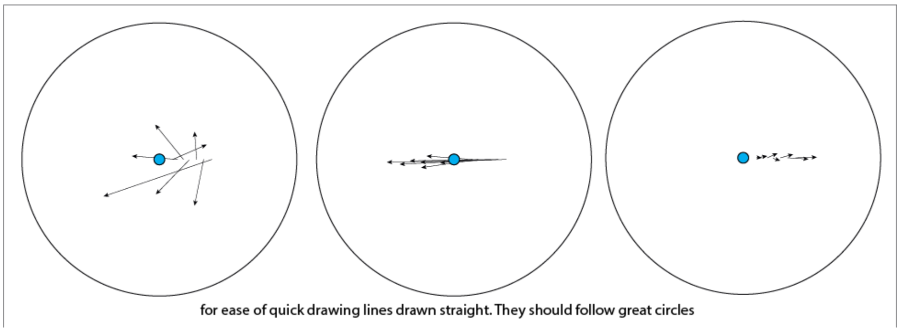


Fig R3. Figure equivalent to b, c and d of fig R2. Maybe clearer like this.

<u>A few other things related to orientations</u>

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??

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.

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

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.

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.

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.

Elongate [c] axis patterns

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).

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.

<a> axis data

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.

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.

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.

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.

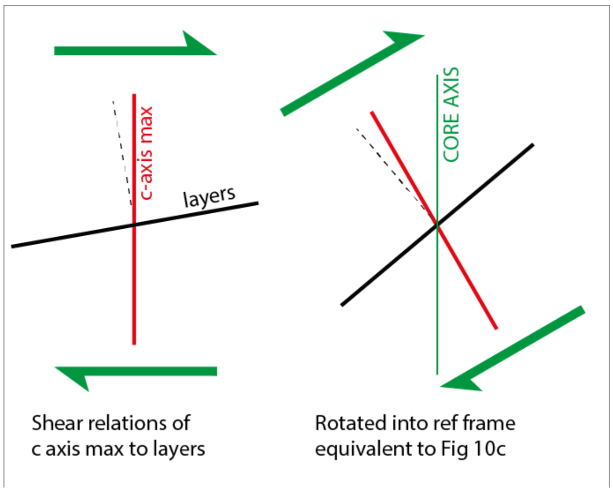
Relevant comparative data from the literature.

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).

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).

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 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.

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 left 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.



Microstructures and their interpretation

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.

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?

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 often 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.

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 f(ig 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).

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.

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).

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.

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.

Lobate and highly irregular boundaries are the observation often 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.

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.

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.

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 (T_H >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.

Temperature

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).

Sorry out of time here!!!

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