Review of *Development of crystal orientation fabric, microstructures and deformational regimes in the deep sections and overall layered structures of the Dome Fuji ice core, Antarctica*
By Saruya et al.

This paper presents detailed and potentially interesting measurements of microstructural properties of the deepest part of the most recent Dome Fuji Ice Core. It provides comprehensive measurements of crystal orientation fabric using DTM, Laue X-ray diffraction, and an automatic thin section analyzer. It also presents complementary information about layers and microstructures.

Particularly in the deep part of the core, the crystal orientation fabric and grains size vary over short depth intervals, and these variations correspond to impurity content and thus climatic interval (as shown in other cores). The authors argue that angle between c-axis clusters and layering is evidence of simple shear in the bottom 20% of the ice sheet (as expected). They draw parallels with fabric in other ice cores, particularly EDC and GRIP.

Overall, I think this might be a meaningful contribution after further major revisions. After finishing my own read, I looked through the original reviews; although I was not an original reviewer, I agree with most of their comments, and I find it disappointing that the same issues I identified were already brought up by the previous round of review but not addressed. I detail these below but give examples here. The manuscript is still too long and lacks focus. I would argue that this is not simply a matter of style—it is the authors job to distill their results into new insights, and this paper does not do that. Important points about the orientation of the structures in 3d from Dr. Prior were essentially dismissed (in response to about 5 paragraphs, all of which I found insightful, it seems the authors only added the sentence in A2_2). Other revisions introduced problems of their own because of the flippancy—for example, the section headers in the introduction do not match the text of the paragraphs since the structure was shoehorned on without corresponding changes to the body, which leads to confusion for the reader. Shortening this paper does not have to mean cutting content. Much of length is due to needless complex sentences, extraneous adjectives and adverbs, and a kind of personification where "comprehension" or "understanding" of phenomena are needlessly substituted for the phenomena themselves. Simplifying the language, and making it more precise in doing so, would go a long way toward letting the reader access the full range of ideas in the paper. Other opportunities for very easy shortening include the multiple places where figure captions are essentially repeated in the main text. I think that the final line of Dr. Montagnat's original review still describes the manuscript: "At the end, the novelty of the study stands only on the fact that these deep core measurements have never been published. Care should be taken in the discussion in order to focus on what is potentially new or relevant for the community."

I have additional scientific qualms as described in overall comments. I have not gone through and made an exhaustive list of detailed comments, since I think hope that structural issues will result in major changes to the text first. I am not an expert in microstructure the way that Drs. Montagnat and Prior are, so I do not focus on the rigor of methods of the observations, or their interpretation in terms of processes at the crystal scale. Instead, I focus on interpreting the results in the ice-sheet setting, the implications for ice flow and radar, and the structure and writing of the paper.

Overall comments:

The paper lacks focus. While the data are undoubtedly complex and extensive, I strongly disagree that this means that the paper cannot tell a story or stories in the way that the response to review claims (and indeed The Cryosphere's evaluation form seems to agree with me). Without better structure and clearer writing, those in very close but not identical fields (including those like myself who have worked extensively on crystal orientation fabric from other perspectives) cannot glean meaningful insight from the work. Put differently, while I agree that "Over-simplifying this paper would compromise its scientific message," the authors have instead under-synthesized and thus obscure the scientific message to all but themselves or similar experts on ice-core microstructure. Part of the issue is contextualization within the literature as Dr. Montagnat noted before. For several of the conclusions, there are citations she mentioned that make identical statements—this makes it exceedingly difficult for somebody with adjacent expertise to understand the value of the present work. I cannot see how the paper really touches on deformation the way the title suggests, other than providing some (unsurprising) evidence of simple shear in the bottom of the ice sheet. I think the paper is currently inaccessible to most of the readership of The Cryosphere, largely due to its presentation not its inherent complexity.

Using the UP80% and LO20% framework leads to misinterpretation, convoluted language, and incorrect conclusions. Take the first conclusion labeled (i). It ends with "at the bottom of the UP80% and fluctuated in the LO 20%." This is not correct (the abstract really indicates that the fluctuations begin at 2650 m, as do the figures). It would be clearer and more precise to write "down to 87% of the ice thickness and fluctuated below." The zones are confusing, both here and elsewhere, since the results described (e.g., properties to 2650 m) cross zones. Overall, getting rid of these UP80% and LO20 acronyms would reduce confusion. This is particularly important for the abstract, which currently involves tortuous language to fit the results into this artificial framework.

The change to structure in response to the first round of review is window dressing; in the introduction, the section titles do not match the sections in the new version, so they create confusion rather than insight. The section titles in the introduction might be okay in principle, but they would need text to match. However, they would need to be continued in some meaningful way further into the paper. As is, the section headers in the introduction suggest broad implications that the paper fails to deliver upon. I see no major implications for ice-sheet dynamics from this work. I would be happy to be wrong, but the authors would have to provide a structured argument (i.e., a story) that shows why such interpretation is supported by the work.

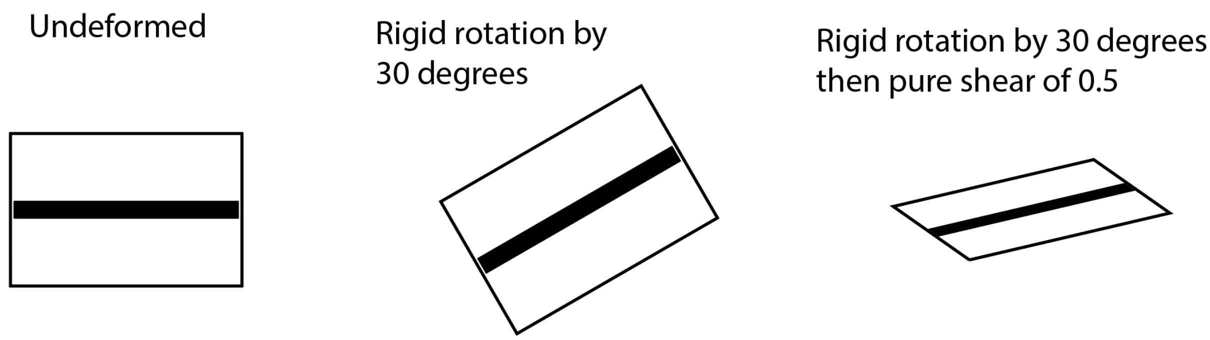
The sentence-level errors are largely fixable during production, but some are substantive and need to be fixed before acceptance. This issue is most prominent in the introduction. Some sentences are imprecise to the point of being incorrect. It is not my job to rewrite them, but here is one example: "In these studies, the influence of anisotropy on the movement of ice aggregates is so substantial that comprehending its impact on the expansive flow patterns of ice sheets is essential" (L70-71). Despite the long sentence, the authors do not argue what this influence is essential for (maybe projecting sea level rise?), define what ice aggregates are (though I surmise this means bulk ice?), or indicate why “comprehending” rather than just the impact itself is important. In this case, simplifying to “The anisotropy is sufficiently strong to affect bulk deformation” would be less wordy and remove imprecision. Again, I am not trying to say that the authors should use my version of this sentence, but ambiguities like this are not made precise in the production process, and there are enough of them that I cannot flag them all. Instead, I think some careful editing by the authors themselves, really focusing on what they can support with evidence (in the above example, that anisotropy influences bulk flow) and not opinion (in that example, that anisotropy is “essential” or that comprehension is necessary).

I find the response to the orientation issues identified by Dr. Prior in the previous version too flippant. As he pointed out, lying in the same vertical plane is different than saying closely aligned (i.e., I do not think the changes answer the issue identified in Figure R1 in his review). For examples of continuing issues involving angles, in section 3.1.2, I had to re-read 3 times to understand how the principal-frame permittivity was identified. While I think I now understand what was done, I do not see why it was necessary to use other measurements of c-axis orientation to do the rotation if the plane matches the normal to the layers. In this vein, I do not understand why Figure 3 is worth plotting—are not these data essentially meaningless without knowing how the section was cut relative to the azimuth of the cluster maximum? Perhaps this just requires methods clarification to say how the thick sections were cut, if it was done in such a way that gives these measurements meaning, but if I understand Rev2_10 correctly then the correction happens after the DTM measurement. Dr. Prior provided great suggestions for plotting the layer normal (which he called the pole) on the same stereonet as the c axes—I really want to see these on Figure 4. They do not match the clusters very well (Fig 5f), and the reader has no idea how well they align from what is essentially a single sentence added in response to the original comment.

Observations and interpretations are still mixed. Section 4.5.3 is the clearest example—each of the paragraphs mixes observation and interpretation. Section 5.2.1 is results.

The use of the phrase “statistical significance” in the paper is incorrect. At times, I think I know what it means, while at others I do not even have a guess. Usually statistical significance means an ability to distinguish between possibilities above a pre-specified level of confidence. The paper essentially uses it to mean “precise,” but with no quantification. This is unacceptable. We need clear definitions whenever the term is used. As a hypothetical, this could be “the difference between measured c-axes distributions at 2500 and 2600 m is statistically significant (p=0.001)” or something similar. Including the confidence and the alternatives considered is required for proper usage.

I do not think the authors’ interpretation of the relationship between c axes and layer orientation near the bed is correct (line 511, Figure 8c). Layer slape is not solely caused by rotation. For example, this is my 30-second version in Illustrator—after rotation, subsequent pure shear in the vertical *does* change layer slope:



The language is too muddy for me to understand if this is causing misinterpretation. Overall, no explanation that treats both layers and c axes as passive tracers can cause this deviation. Instead, this could be easily explained a result of intracrystalline slip. Particularly in the conclusions (L830), it sounds instead like the authors disagree with the cartoon version above, and instead are imagining that the pure shear has no effect on slope. Adding in the actual mechanism of c-axis rotation would help.

The conclusions are excessive, variously going beyond the results, repeating themselves, and restating ideas that have long been known. I do not really understand how the first (i) and (ii) differ. Both of these conclusions look nearly identical to other locations (e.g., EDC, (Durand et al., 2009)), and given the emphasis on comparison between sites this should be cited. The second (i) is not supported by the results—at most there is evidence that right at domes climate controls the fabric, but this paper shows nothing about how that might look even 5 km off the domes. Similarly, I do not buy the second conclusion (v); the work shows very little about how large-scale deformation is taking place. The first (vii) is very similar to claims from work at EDML (Weikusat et al., 2017), and the idea was originally suggested in the 90s (Castelnau et al., 1996). While there is nothing wrong with concluding the same thing as previous work, the reader needs context. A much shorter conclusion section, focusing synthesizing rather than repeating key results and contextualizing them rather than providing so much detail, would allow the reader to see how this work fits into the literature.

Detailed comments:
Title: The title does not really make sense. I cannot tell if the overall layered structures are a wholly separate thing considered at all depths or whether the crystal orientation fabric is within them.

Abstract: The abstract is too long and detailed, to point that I had to read it three times to figure out what the manuscript was showing. The paper would greatly benefit from shortening the abstract to meet normal length standards (i.e., 250 words, or at least ~300 as it was before, rather than 450). As discussed above, this does not necessarily mean cutting content. For example, it would benefit both accuracy and word count to delete the first words of the abstract "An in-depth examination of."

L24,184: Statistical significance cannot stand on its own like this. I assume that the authors mean that they have measured enough grains for the results to approximate the underlying c-axis

distribution with some amount of precision (perhaps that it differs from isotropy, but I truly do not know). But a measurement itself cannot be statistically significant in the sense used here.
L35: "dislocation creep is the primary deformation mechanism in polar ice sheets" cannot be concluded from this work. There is no reason to think that these results can be extrapolated to the ice sheets writ large.
L45-50: This paragraph has multiple logical jumps to try to get from the ultra-broad (sea-level rise) to the ultra-specific (dynamic layer structure). Rather than filling all these gaps, I suggest starting less broad, since the readership of the cryosphere surely knows what ice sheets are and that they contribute to sea-level rise. I recognize that this suggestion is stylistic as well as substantive, but if not heeded then we need the gaps filled in (e.g., how is dynamic layering a process challenge that affects models, why is continuous improvement in models needed rather than one really big improvement, how does a single 11-year-old reference indicate ongoing concern).
L68: Abbassi is not a good reference for SPICEcore fabric, they measured fabric loosely from IceCube. Voigt 2017 is the correct reference (https://doi.org/10.15784/601057).
L70: The correct reference for the full EastGRIP fabric is Stoll et al., 2024 (10.5194/egusphere-2024-2653)
L81-85: references needed
L220-221: How can a plane and a vector be in the same plane unless the vector is in the plane? I think this probably means that the normal to the layer is in the same vertical plane as the COF cluster?
L223: What are non-principal components? The components in an arbitrary xyz reference frame? I have not seen this terminology before and think it needs to be defined.
L227: Still confused since I do not know what these are, but are they really "adjusted" or are they rotated in some way (thus changing the value)?
L276-287: This is a repetition of the figure caption, not results.
L436-437: This sentence needs to be re-written. It is unclear if this means temporal changes or time differences that result from the depth-age relationship of ice sheets.
L497-501: This is a repetition of a figure caption, far from the original appearance, not discussion
L648-655: This discussion of twinning is convoluted. Starting with a simple definition would help.
L725: I do not think this really differs from Durand—it is a different location, and there may simply be regional or local differences
L774-778: The implications for radioglaciology need to be clarified. The idea that recrystallization is important near the bed is not new, nor is the evidence of simple shear which also been noted previously near the bed at ice-core sites (for example, it is suggested at EDC by Durand).

Figure 1: Confusing to use red and blue on contours in c when they do not match the colorbar in b. If two colors are needed, it should be explained in the caption (and the colors should not be red/blue, or why not just use a single color?).
Figure 8f: The label is confusing. The LO20% spans almost 90% of the thickness.