The Cryosphere Discussions

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Comment on tc-2021-336

Anonymous Referee #1

Referee comment on "Development of crystal orientation fabric in the Dome Fuji ice core in East Antarctica: implications for the deformation regime in ice sheets" by Tomotaka Saruya et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-336-RC1, 2022

Dear editor,

This paper investigates the crystal orientation fabric of the Dome Fuji Station ice core using a novel methodology as the dielectric anisotropy (from the dielectric permittivity tensor). Dielectric anisotropy is revealed as a good indicator of the vertical clustering of the crystal orientation fabric, also exhibiting a correlation with the concentration of chloride ions and with the amount of dust particles in the ice core. From the results, the authors conclude that the COF clustering is therefore affected by the presence of chloride irons, which increase the dislocation density, promote dislocation creep and enhance the COF clustering, while the presence of dust impedes it. The results show a COF layering in the upper 80% of the ice sheet, where the COF contrast amplitude increases at deeper layers. The conclusion is sound regarding the lowest 20% of the ice sheet, as layers will behave differently under stress depending on the COF cluster strength.

This well-written and well-organised manuscript presents a very useful methodology to be further evaluated in the future. We could obtain the COF contrast from the permittivity contrast obtained in VHF/UHF radar sounding. This will allow comparing deep COF layers avoiding areas with layers with heterogeneous thickness. This heterogeneity can lead to layer disturbances and folding due simple shear close to the bedrock.

The detailed description of the presence of soluble impurities and particles and its correlation with the COF is very useful for the understanding on the effect of them on ice rheology, which is currently unknown.

The manuscript is relevant for *The Cryosphere* and thus can be a valuable contribution once some important issues are addressed. Thus, I recommend that the paper is accepted after minor revisions.

Suggestions for improvement:

Line 50: this statement "*It has been suggested that the finer grain size in glacial ice results from high concentrations of impurities such as dust particles or soluble substances 50 that restrict grain growth via pinning and drag at the grain boundaries"* requires a reference.

Line 80: I would give details of the physicochemical properties obtained.

Table 1: Could you explain in the text why the thickness at the EDC samples (Durand) are not indicated?

Line 110: the authors do not explain why this study focuses on the upper 80% and not in the whole ice core. What are the difficulties to apply it in the bottom 20%? A discussion on this aspect will be useful.

Figure 2. Colouring in red and blue the lines is not necessary as they do not provide extra information.

Line 150: the detrended Ae value is a key parameter used in this work. It is mathematically defined in the text, but it would be very useful for readers to be able to see an explanation of what it value means, in practical terms.

Line 186. has the value of 0.0334 for the single ice crystal been determined in this study or does it come from the literature? In this case, a citation would be needed.

Line 213. Would it be possible to briefly explain the relationship between the permittivity value and the normalized eigenvalues? (here or in the caption of figure 6). I find the reference to Saruya et al. 2021 not enough, as this data is relevant for the conclusions.

Figures 4 and 5: I suggest including the references at the legend, as in figure 7.

In general: please, check the graphics in all figures. Box and axis markers do not match (as in figure 7).

Line 384: the reason why the presence of HCl has a stronger effect on dislocation migration than NaCl is explained later, in line 387. I suggest moving line 384 there to make the paragraph more understandable.

Line 444: In general: It should be explained with a bit more detail, what the positive or negative feedback mechanism referred to Azuma (1994) does mean (Relationship between CPO and deformation conditions).

Line 499: Regarding the alteration of layers in the deep parts in ice sheets, here I miss some discussion with observations already done in ice cores (as in Faria et al., 2010; Jansen et al., 2016, etc…).

Conclusion and chapter 4.5 *Implications for the deformation regime in ice sheets*: both texts are very similar. I would modify the conclusion part in bullet points or in a more synthetised way, because as it is now it reads as a repetition of the explanation given in the previous section 4.5.