The Cryosphere Discussions

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Comment on tc-2021-336

Anonymous Referee #2

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-RC2, 2022

General comments

This contribution provides an excellent methodology for exploring the chemical and mechanical heterogeneity of ice, with a likelihood of inferring crystallographic fabric from permittivity anisotropy. The approach is valuable for the community and the data appear robust. I have no concerns about the data acquisition. The comparisons with with the nearby cores and with the Dome Fuji 1 core chemistry make good sense, including using the orientation tensor as a metric. This is a large dataset that will serve a purpose for many years to come.

I have a few significant concerns about the interpretations. Some of these can be addressed with additional explanation, and some may require reevaluating the text.

Specific comments

1a. Crystal orientation fabric (COF) is not the only factor that affects permittivity or permittivity anisotropy. Dust, salts, or other impurities that are layered in the ice core, even at a fine scale, can cause permittivity anisotropy. I suggest that the paper review the potential impact of these factors on anisotropy and evaluate whether they can robustly related the permittivity data to COF.

1b. If this investigation cannot rule out impurities as factors, then I suggest that the interpretations, including the discussion and conclusion, focus more on reporting the permittivity anisotropy and its correlation with the other features in Fig. 9 and less on COF. I recognize that several sections in the discussion consider how the impurities affect COF, all of which appear to be valid and substantive ideas. At the same time, the lack of a consistent relationship between permittivity anisotropy and, e.g., Cl and dust, indicates that the mechanisms are quite incompletely understood. I do not feel that the data and reasoning support the interpretation (line 400) "Consequently, we propose that the relative strength of COF clustering is mainly determined by a balance between the levels of Cl- ions and dust particles."

2. I was not able to understand the data collection methods from the text, in particular the geometry of the sampling. A figure that shows the spatial relationship between the core, the samples, and the measurement and motor directions would be extremely useful.

3. The text does not include an explanation of the source of uncertainty. It appears that the reported standard deviation is the result of some form of averaging, and it is not clear whether any systematic uncertainty is factored it. I suggest the manuscript add a clear method for calculating uncertainty.

3. On the topic of choosing which technique to use to analyze a core, lines 209-210 state that the "statistical validity of the thin-section-based method is inferior to that of the thick-section-based method." I don't find that statement accurate. The thick section data unquestionably average over a larger volume, but that doesn't mean that they are more statistically valid. I do think that representing the larger volume will provide a better relationship to rheology than the potentially high-frequency variations recorded in thin section data, but that is not the claim currently made. Additionally, as implied by my comment #1, the relationship between COF and permittivity anisotropy is not necessarily straightforward.

4. Much of the discussion focuses on the detrended data. The manuscript mentions the method only briefly in the caption to Figure 3 and on Line 151. More description of the method, including physical and statistical rationale for the choice and comparison with other methods, would provide more confidence in the value of the detrended data.

5. I suggest that a revised manuscript include more statistical exploration of the data comparisons stemming from Fig. 9. I noticed two locations with reported correlation coefficients (lines 355 and 390), which seem to be for timeseries pairs (e.g., delta-e and HCl). I feel that a more systematic, potentially multivariate approach would have more value. Part of this request is to add more reliability to the interpretations: at present, the mixed signals of whether dust or Cl or something else will affect delta-e (e.g., Type A and Type B relationships) does not provide a pathway to predict the effect.

This paper has the potential to make a significant impact in the field. I appreciate the authors considering these comments as ways to strengthen the paper and improve its impact.