Reply to the reviewer 2

We thank the reviewer for careful review of our manuscript and thoughtful comments to improve it. In the following, we describe our responses (in blue) point-by-point to each of reviewer's comment (in black).

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 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.

We thank the reviewer for the appreciation and summary above.

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.

We will add following sentences in "4.1.1 Basic facts and questions" (Discussion section):

Data on the complex permittivity of ice around megahertz frequencies are reviewed by Fujita et al. (2000). The real part of the complex permittivity of ice in the ice sheets is a function of several controlling factors as follows: (1) COF, (2) density, (3) impurity concentration (mainly acidity), and (4) temperature. In contrast, both (5) hydrostatic pressure and (6) air-bubble shape have relatively minor effects. The effect of (7) plastic deformation can be significant and needs to be investigated further. We explain in more details. In ice with bubbles, either density, impurity or temperature has no effects on the dielectric anisotropy. There has been no data that can raise a possibility that grain boundaries, dust inclusions, clathrate hydrate inclusions or salt inclusions within ice can have detectable impact on the permittivity. Matsuoka et al. (1997, 1998) investigated the influence of soluble impurities on dielectric properties by measuring the permittivity of impurities doped ice. They reported that the small amount of impurities did not significantly affect dielectric properties.

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."

As answered in #1a, we can rule out chloride ions or dust particles from influenceable factors to the permittivity values.

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.

Thank you for your suggestions. Experimental procedures and diagrams are detailed in Saruya et al. (2022, this paper was finally published in 2022); however, we will add experimental diagrams in revised manuscript to make readers better understanding. An example is shown in below:

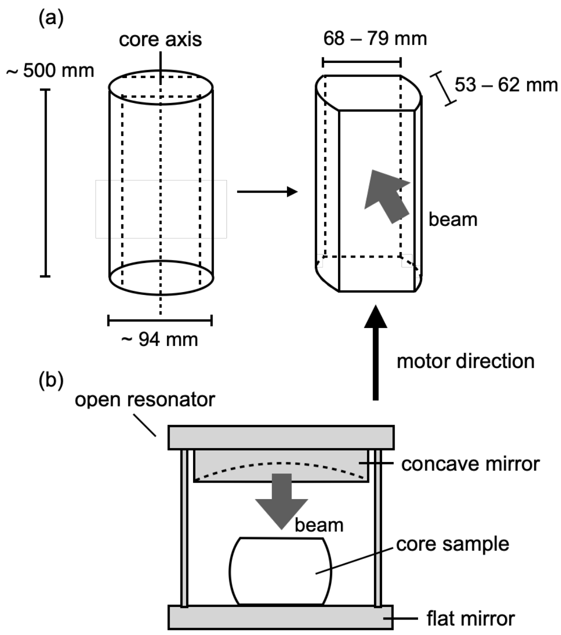

Fig.# Schematic diagrams of (a) the core cutting and (b) experimental setup (view from the front side).

3a. 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.

We will add following sentences in “4.1.1 Basic facts and questions” (Discussion section):
Estimation of errors are detailed in Saruya et al. (2022). They reported that errors were minimized by solving equations for multiple resonance frequencies simultaneously to find a unique solution for ε. The final errors in ε were –0.01 ± 0.01. The systematic error is mainly caused by limited widths of the ice core sample.

3b. 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.

We will remove “statistical validity of the thin-section-based method is inferior to that of the thick-section-based method” because this sentence is not suitable for an explanation of thin-section measurements. However, we consider that an increase in sample volume produces statistical significance because thick-section results are comparable with superimposed results of more than 100 thin-sections. In revised manuscript, we will explain the advantages of thin-section measurement and thick-section measurement as follows:
Thin-section measurements can provide local features with distributions of c-axis orientations in each grain, while thick-section measurements can provide bulk and representative features of the COF.
Regarding the relationship between COF and dielectric anisotropy, we consider the relationship is fairly straightforward since we can regard other properties (except for COF) as uninfluential factors, as answered in #1a and #1b.

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.

We will add following sentences at around Line 154:
The detrended $\Delta\varepsilon$ represents the relative degree of c-axis clustering and the extent of deformation relative to the surrounding depth. Detrended values are more useful than original values to investigate the fluctuations of COF and to compare with other physicochemical properties.

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.

The concentration of dust particles was measured by a laser particle counter. In this system, we can verify the increase of dust concentrations when the increasing amount is significantly large; however, it is difficult to detect the small variations of dust concentrations. Therefore, multivariate analysis using dust concentrations is difficult. Additionally, we could find correlations between dielectric anisotropy and concentrations of chloride ions and dust particles in only specific depths, so valuable results are not expected in a multivariate analysis through whole depth.