Review to Zeising et al. 2022, The Cryosphere

Summary

Zeising et al. present a technical development using phase-sensitive radar polarimetry to estimate ice crystal orientation fabric near the EastGRIP ice core. The key point of novelty is to enhance the polarimetric coherence (a method used by previous radar polarimetry studies) by adjusting the range-bin offset between orthogonal polarizations. This allows them to obtain high coherence throughout the ice column, which is then used to infer two-way travel time differences, dielectric anisotropy, and azimuthal fabric anisotropy. They then compare with the ice core fabric eigenvalue differences, showing very good agreement down to ice depths ~ 1500 m.

I like the general concept, and enjoyed reading the paper. Whilst the coherence optimization/range-bin offset method is a relatively simple refinement from previous studies, it produces agreement between ice-core fabric data and the radar asymmetry estimates (probably the best I have seen to date). However, as it stands, I think the paper needs to better demonstrate how it has improved coherence and fabric estimation from methods used in previous studies. Additionally, fabric orientation information should be provided (both from the radar, and comparison with the ice core if it is available) as other similar radar polarimetry studies have all done this. I also do not agree with the physical interpretation related to the `half-wavelength limit' (regarding why previous applications of polarimetric coherence will be ineffective) and have given some counter arguments and suggestions to rephrase the discussion.

At < 200 lines and 3 figures the study is significantly shorter than a typical TC article, and closer to what I would expect for a `TC brief communication'. I see no issue with this, but potentially the editorial team and authors will want to change the format for the final publication.

Best regards,

Tom Jordan, Plymouth Marine Laboratory, UK

Specific/major comments

1. *Demonstration of improved coherence and fabric estimates over previous methods.*

A central weakness of the study is that it does not explicitly demonstrate the improvement of the coherence magnitude and fabric estimates over previous methods. A reader less familiar with the field will therefore be uncertain about the progress made in the paper.

I think this can be fixed relatively easily by showing:

> (i) A depth profile for |chvv| calculated *without* the range-bin offset, similar to the second column in Fig 2. My guess is that this will decay rapidly with depth, showing the coherence method in the paper to be more effective than previous.
>
> (ii) Adding fabric asymmetry estimates, where possible, using the previous approach: i.e. using vertical gradient of the co-polarized phase difference (no offset) employed by Dall 2010, Jordan et al. 2019, 2021, 2022, Young et al. 2021.

Related to this point, I would put a qualifying statement that the study can only be directly compared with methods that have used multi-polarized data (e.g. Jordan et al. 2019, 2020) rather than quad-pol approaches (e.g. Brisbourne et al. 2019, Young et al. 2021, Ershadi et al. 2021, Jordan et al. 2022). A consequence of using quad-pol is that it enables reconstruction of multi-polarization data at higher angular resolution, which gives a particular advantage to inferring fabric orientation.

2. Fabric orientation information.

In addition to azimuthal asymmetry, polarimetric radar enables estimation of the orientation of (assumed) horizontal eigenvectors. This can be done by comparing data with the polarimetric backscatter model (Fujita 2006, Jordan et al. 2019) and using sign of the coherence phase gradient (Dall 2010). Fabric orientation is very useful information, as it informs about past deformation, aswell as being key information for understanding the impact of fabric on anisotropic rheology.

I therefore find the current study incomplete, and I think it would be significantly strengthened, by including fabric orientation panels in Fig 3 (i.e. azimuthal angle of horizontal eigenvectors as a function of ice depth). I appreciate that the angular resolution will be limited as multi-polarization rather than quad-pol was used, but I still think this will be a nice inclusion.

Additionally, does the EastGRIP core have azimuthal fabric orientation to compare with? If it does, then this should be included, in the orientation plots.

3. Accuracy of the discussion on the `half-wavelength limit'

In their discussion, the authors focus on the `half-wavelength limit' (for the polarimetric phase difference) as an explanation why the polarimetric coherence method has previously been limited. e.g. *`Due to the ambiguities caused by phase wrapping, the previous methods which are based on the coherence phase gradient were limited to the derivation of phase shifts of a maximum of half a wavelength, (Line 61)'.* I don't agree with this interpretation, and I have given some counter arguments and suggestions for revision below.

First, I don't think previous methods based on phase gradient are limited by the phase wrapping. Jordan et al. 2019 used an identity (equation 23) to differentiate the phase. This approach (adapted from the InSAR literature) gets around the issue of phase wrapping, as the real and imaginary components of the coherence are continuous functions, enabling the derivative to be taken. Figure 5 and 6 from this paper illustrates that the phase gradient can be obtained at the phase discontinuities.

There are other examples in the literature that illustrate that the coherence magnitude, phase difference, and fabric estimates are not physically limited by the phase discontinuities and the proposed `half-wavelength limit'. Notably, Young et al. 2021 (Fig. 4) shows high coherence persisting over multiple (~4) phase cycles.

It therefore follows that `strong azimuthal asymmetry' (and rapid phase-cycling) should not be a singular limitation on the previous method. I think it is probably coincidence that the fabric in Jordan et al. 2022 is often only obtained in the first phase cycle (line 171). This study proposed a degradation in the radar stratigraphy as the reason for the coherence drop-off with depth.

Despite discounting the `half-wavelength limit' interpretation, I do agree with the authors that their optimization should lead to higher coherence, and therefore improve the fabric estimates. This is because their method should act to better co-register the signal from a given radar layer. I think, if they better show the impact of the optimization on the coherence (following my comment 1), then they will be able to refocus the discussion around improved co-registration being the physical mechanism that improves the coherence and fabric estimates. (As an aside, I don't think it is strictly necessary for the reflection to occur from the same layer for each polarization. As long as the layers behave as flat, specular, reflectors, the original coherence method should still work to some degree).

Aswell as the discussion, lines 35-38 and line 186,will also need addressing regarding this point.

Minor comments/typos

Title - The MS title is quite generic (in effect all COF/radar polarimetry studies estimate horizontal asymmetry!). As the phase co-registration/optimization of the coherence is the key point of novelty, I recommended changing to something like: `*Improved estimation of ice COF from polarimetric phase co-registration'* or `*Improved estimation of ice COF from optimization of the polarimetric coherence'*

L 4, L 25, etc – I would use the term `polarimetric radar' rather than `radar'.

L 16 – is a new paragraph needed?

L 25 – Maybe `dielectric anisotropy due to crystal anisotropy'?

L 34 – A better description of what is meant by the `polarimetric coherence method' is needed here (something like: polarimetric coherence refers to the strength of the phase correlation between orthogonal polarizations)

L 35 – I have different interpretation of when the polarimetric coherence method will/will not work – see specific comments.

L 53 – should `accuracy ~ 1 mm ' be `precision ~ 1mm'?

L 56 onwards - I would be clearer from the offset that this study is considering muti-polarization data (i.e. co-polarized data as a function of azimuth), whereas most studies are now using quad-polarized data (also see specific comment 1).

L 71 – Does the East GRIP core contain azimuthal orientation and tilt measurements/zenith angle for the fabric eigenvectors? (Also see specific comment 2).

L 91 - I would replace `According to Fujita 2006 and Jordan 2019…' with `If it is assumed that the ice crystals are an effective medium at IPR frequencies…'

L 125 – I think a bit more context on the `fine-scale' ranging capabilities of ApRES is needed here. E.g. what is the physical interpretation of the l/Bp term in equation (10)?

L 136 – I would quote the difference with the core data after this sentence

L158 – The use of quad-polarized data was proposed a long time before Ershadi et al. 2021. Notably, the work of Doake et al. 2003: https://folk.uib.no/ngfso/FRISP/Rep14/doake.pdf. Quad-polarized acquisition has the key advantage of reconstructing co-polarized data at a high angular resolution. I therefore think that combining the authors' co-registration method with the quad-pol method (e.g. Brisbourne et al. 2019, Young et al. 2021, Ershadi et al. 2021, Jordan et al. 2022), will be what people will do in the future, so I recommend writing a paragraph along these lines.

L176 onwards – As noted in specific comment 1, I think the comparison with Ershadi/other previous methods needs to be explicitly demonstrated to the reader.