Remark 1: I apologise for the delayed submission of this review. I had warned the editor that this would be the case, but also realise that this is obviously of no great help to the authors.
Remark 2: I had at first declined the review due to a perceived conflict of interest as we are currently working on a quite similar topic (Ph. D. thesis M. Ershadi who contributed to this review). However, after discussing with the editor, we decided to move on anyway. I hope that this review is perceived as constructive & helpful.

Summary

In their paper ”Rapid and accurate polarimetric radar measurements of ice crystalfabric orientation at the Western Antarctic Ice Sheet (WAIS) Divide deep ice core site”, Young and co-authors present an ApRES radar dataset, which they use to infer the ice-fabric characteristics continuously to a depth of 1500 m. Main results include quantification of the horizontal ice anisotropy with a depth invariant ice-fabric orientation that is aligned with the directions of the principal strain rates. The inferences are validated with data from the WAIS ice core, and some conclusions are drawn about the ice-divide stability throughout the Holocene.

Overall, this paper is nicely written and the authors do a commendable job in guiding the reader through the methods and results. However, in places I find the paper unnecessarily superficial and I don’t see novel aspects clearly. I also suspect (but I am not certain) that parts of the azimuthal reconstruction may be erroneous leading to wrong inferences in terms of the ice-fabric orientation. Below, I mention a number of major comments/questions how this can be improved. Applications of radar polarimetry are still rare, and I hope that the points raised below will help to improve the next version of this paper.

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Clarify methodological advance

It is stated that this study ”..extends previous qualitative analyses [...] to obtain quantitative measurements..” (l. 285). Can you highlight more clearly what those extensions were compared to previous studies? From what I can see so far, this study nicely applies previous developments to a single new site, but I struggle to see the extensions. The link between the polarimetric phase gradient and ice-fabric parameters is based on the cited papers Fujita et al., 2006 and Jordan et al., 2019. Arguably matching the angular distance of co-polarization nodes with a 2D optimisation is new (l.233), but at least the dependency of this distance as a function of anisotropic scattering is already approximated in Fujita 2006. Also advantages or pitfalls (e.g., in terms of uniqueness and uncertainties involved) of this approach are not discussed.

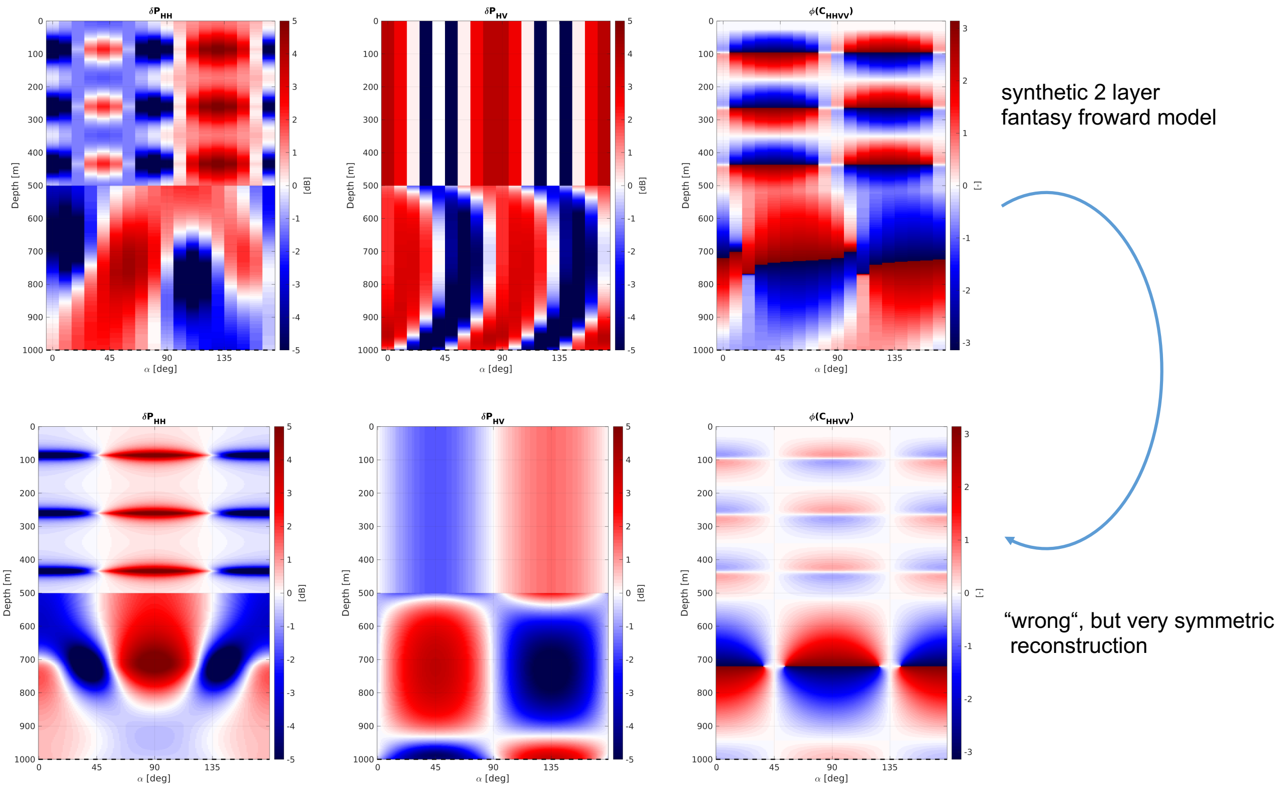
I suppose that this paper is the first to explicitly focus on synthesising quad-polarimetric measurements for ApRES, although the related methodology is known from radar polarimetry textbooks (e.g., the cited Mott, 2006). The inferences drawn from this method about the ”high angular” resolution are not credible as currently presented (see comment below). Also the lack of rotational dataset at this site makes it hard to discuss advantages/disadvantages of both approaches. I suggest a dedicated section were improvements and distinct differences compared to previous studies are highlighted more explicitly.

Coincidental symmetry at $\theta = 90°$?

In Figs. 2b-e one principal axis of the ice-fabric appears at the local azimuthal angle $\theta = 90°$ (i.e., all panels have a reflectional or rotational symmetry around the $\theta = 90°$ axis). This means that during measurements antennas were coincidentally placed parallel (hh) and perpendicular (vv) to the (at the time) unknown ice-fabric orientation. It is possible that the operators in the field made a conscious decision here because $\theta = 90^\circ$ aligns with the strain rate (not the ice-flow) direction. However, given uncertainties involved in determining the direction of maximum strain rate and the antenna orientation, the $\theta = 90^\circ$ symmetry almost seems too much of a coincidence. Based on our own experience with analysing quad-polarimetric data, we suggest that the authors double-check that indeed $s_{hv} = s_{vh}$. We found occasionally that $s_{hv} = -s_{vh}$ without satisfying explanation as to why this can be the case (e.g., inconsistencies in labelling and naming of antenna orientations in the field?). However, if it is the case, then reconstruction of the ApRES signal using eq. (4) forces a symmetry axis at $\theta = 90^\circ$ exemplified below for the s_{hh} component:

$$S_{11} = s_{hh}(\theta) = \underbrace{\underbrace{s_{hh}\cos^2\theta + s_{vv}\sin^2\theta}_{\text{symmetric at } \theta=90^\circ} + \underbrace{(s_{vh} + s_{hv})\sin\theta\cos\theta}_{\text{anti-symmetric at } \theta=90^\circ}}_{\text{in general no symmetry axis at } \theta=90^\circ \text{ unless } s_{hv}=-s_{vh}}$$

The graphic below illustrates how this would be reflected in a full azimuthal reconstruction where the principal axis around $\theta = 35, 125^\circ$ in the top plot are erroneously mapped to $\theta = 90, 180^\circ$. Without a co-polarized, rotational dataset this will occur unnoticed.



Maybe it will be helpful to investigate this further. Alternatively, state explicitly how the hh and vv directions were defined in the field, and why it makes sense that those axis align almost perfectly with the principal directions of the ice-fabric.

Terminology linked to azimuthal resolution

In numerous instances (e.g., l.7, l39, l49..) the authors advertise that synthesizing the azimuthal response from quad-polarimetric data (eq. 4) results in improved angular *resolution* compared to rotational setups. I disagree with that. The chosen azimuthal spacing of 1° (l. 397) is completely arbitrary and any value works with eq. 4. I agree that advantages and disadvantages of quad-polarimetric vs. rotational measurements should be discussed, but choosing an arbitrary gridding for θ is not enough in this regard. Also, no rotational dataset is presented so that the claims about the superiority of quad-polarimetric measurements are not rigorously substantiated (apart from the obvious fact that they are much quicker to obtain).

Minor remarks

- Abstract should state limitation that the methodology only works if one of the c-axis is pointing upwards.
- l 57: Eq. 4 *reconstructs* the azimuthal response, but this is something different than *resolving* it. See comments above.
- l 63: Specify what resolution you refer to. ApRES surely has lower potential for vertical resolution than ice-core data.
- l 65: Specify what the angular resolution is. It cannot be the 1°. I would also prefer more modest wording for "unambiguously". Defining the direction of the ApRES antennas alone is already error-inflicted and there is no rigorous statement in this paper on how this was done.
- Overall nice structure of the introduction. This works for me.
- Fig. 1a include orientation of the E-field vector. Statement that antennas are oriented parallel to the divide is conflicting with inference that principal axis is at $\theta = 90^{\circ}$ (which is parallel to flow, which is oblique to the divide according to Fig 1b). See major comment 2.
- l 114 not only ice-dynamics but also ice properties induced through climate variations imprint on the ice-fabric evolution. I am not sure the principal ice-fabric axis always line up with today's strain rate regime as suggested here.
- l 127 the terminology "anisotropy" for β has confused me. You also need "anisotropy" for birefringence. Why not call it the "anisotropic reflection ratio" or something like that? "boundary reflection" is more appropriate than "boundary scattering" (the latter suggests some diffuse scattering which is not accounted for in this context. However, this may be a matter of taste.)
- l 137 remove abbreviation SISO. It is not used later on.
- l 150 include uncertainties for this angle here and elsewhere.
- l 169 double-check that $s_{vh} = s_{hv}$ (see comment above).
- I understand how this azimuthal phase difference is calculated, but I don't understand what the additional value is. What inferences are drawn from the co-polarised phase difference that cannot be drawn from the hhvv phase angle?
- l 193 This first paragraph is more methods to me than results.
- As stated above the 90° are suspicious. Also, what are the $\pm 7^{\circ}$ based on? I would think that errors in antenna positioning are larger.
- l 213 Typo? This ambiguity cannot be resolved in the hh power anomaly shown in Fig 2b. You need to use the polarity of the phase gradient.
- l 221 at least estimate these "human" errors
- why is the "anisotropy" an integer value?
- l 237 I think I missed something here: Aren't those node pairs simply depths where the phase shift between ordinary and extraordinary wave is odd integer multiple of π ? Clearly they will have a correspondence in the azimuthal phase difference (which is directly related to the phase angle). I don't understand the deeper physical implication of this 'four quadrant pattern yet.
- Fig .4 I appreciate the error bars on the ApRES derived $E_2 - E_1$. Please state more clearly how those were derived.
- l 289 This is not the "best model" that matches observed results. It is a model that explains some of the features in the observations
- l 330 How fast does the ice-fabric structure adapt to a new strain regime? I think some sort of

statement in this regard is required to better justify statements of ice-divide stability.