Second author response to the review of Young et al. “Rapid and accurate polarimetric radar measurements of ice crystal fabric orientation at the Western Antarctic Ice Sheet (WAIS) Divide deep ice core site” [Manuscript # tc-2020-264]

This response is in regards to the responses following our first revision (11 March 2021) to the original manuscript submission (28 September 2020). We would like to thank the managing editor (Kenichi Matsuoka) and the two reviewers, Martin Rongen and Reinhard Drews, for their reviews . Please find below the editor’s (EC) and the two reviewers’ comments (RC) in **bold**, each followed by the authors’ response (AC). Line, figure, and page numbers mentioned by the reviewers (within RC) refer to the second version of the manuscript (the revision dated to 11 March 2021) and those mentioned by the authors (within AC) refer to the current version of the manuscript.

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13 May 2021

Review by Editor (Kenichi Matsuoka)

General comments

EC0.1. Thanks for submitting a thoughtfully revised manuscript. Both referees provided highly constructive comments and I am very glad to see that the authors made satisfactory responses. I recommend the authors to follow all new suggestions made by the referees and calculate depth profiles of E2-E1 at all sites and plot them in supplemental figure, possibly together with the data from site I or from ice core to facilitate reader’s comparison between this new figure and Fig. 6. Also, please consider my own comments listed below; all are relatively minor.

The authors declared that the full dataset and relevant codes will be released through the UK polar data center, but the link given here is generic. As the review process is nearly finalized now, please upload all relevant files, receive DOI for these resources, and include specific URL/DOI to the manuscript.

I request a minor revision. Please submit the revised manuscript, the manuscript with track changes, and the brief response letter.

AC0.1. Thank you for your constructive comments and we have responded to each of them in turn below. We apologise for not providing a reference to the dataset and relevant codes in the last revision, as they were in the process of a lengthy quality control. The dataset is now published and the DOI is 10.5285/BA1CAF7A-D4E0-4671-972A-E567A25CCD2C. This as well as the citation (Young and Dawson 2021) are included in the Data Acknowledgements section.

We will attach the code to process the datasets to the DOI once the review process closes, in case there are any further corrections that may involve potential changes to the code.

Specific comments

EC0.2. Figure 1 caption: add geographical orientation (ESE, 110o) of the horizontal-polarization plane. Illustrations representing antennas are unnecessarily complicated; just show the antenna element orientation. The WAIS Divide orients 20 degrees, and the horizontal plane orients 110o, but these two orientations are shown the same in the illustration.

AC0.2. We choose to keep the illustrations of the antennas, because we suspect (but are not sure) that the specific *h* and *v* orientations may play a role in whether $s_{hv} = s_{vh}$ or $s_{hv} = -s_{vh}$ in the radar processing steps. In other words, whether the cable exits upwards or downwards when the antenna is oriented in the *v* direction may potentially affect the processing output. This figure shows the exact antenna orientations that we use for each of the four acquisitions.

The horizontal plane orients 110°, which is approximately parallel to the WAIS Divide (~110°).

To address this comment, we have explicitly stated the horizontal and vertical polarisation orientation in the figure as well as in the caption.

EC0.3. Figure 2: adjust color for strain components. Dark blue and red are very hard t read. Clarify the unit of elevations (m a.s.l.?, or mention the geoid height in the caption). Include intervals of the surface elevation contours (10 m), and the ice thickness at site I in the caption.

AC0.3. I have tried adjusting the colours but making the strain components a lighter shade unfortunately is even less distinct. I have adjusted the scale of the background (bed elevation) so that the strain components stand out more. The elevation units (surface as well as bed) now both read m a.s.l., and contour intervals are clarified in the caption.

EC0.4. P7L134: The equation of \epsilon (z) is, I think, incorrect. The authors intend to define \delta\epsilon(z), permittivity anisotropy in the horizontal plane. Figure 5 caption gives the range of \epsilon(z) [0 1.5e-2], but I think it is the range of \delta\epsilon(z), not \epsilon(z).

AC0.4. You are correct on both parts of your comment: the equation should read $\Delta\varepsilon(z) = \Delta\varepsilon(z)' (E_2 - E_1)$, and Figure 5 should give $\Delta\varepsilon(z)$. This has been corrected in the text and in Figure 5 (both in the figure and in the caption).

EC0.5. Figure 3: add the level where SNR is considered too low (e.g. horizontal bar showing the minimum signal level that makes adequate SNR, or depth ranges where SNR is too low).

AC0.5. This has been added as an additional panel to the top of Figure 3.

EC0.6. P11L240: spell out FIR (finite impulse response?)

AC0.6. Done.

EC0.7. P11L269: change "the directions of the smallest…" to "the orientations of…."

AC0.7. Done.

EC0.8. P13L300: E1 and E2 orientations are "mostly" depth invariant. So, add "mostly" or indicate the range of depth variability like "depth invariant (within +/- 6 degrees throughout the depth range, Fig. 4a and Table S1)."

AC0.8. See AC0.9.

EC0.9. Figure 5: remove WDC from the beginning of the caption. I think \epsilon(z) is indeed \delta \epsilon (z). At the end of the cation it is said that E1 and E2 plane orientations are set to 0o and 90o following measured observations, but it is not accurate enough. See my comment on P13L300.

AC0.9. WDC has been removed from the caption. Following AC0.4, $\varepsilon(z)$ has been changed to $\Delta\varepsilon(z)$. Because the model requires E_1 and E_2 to be separated exactly by 90° (as eigenvectors are supposed to be theoretically orthogonal to each other), we chose to offset the measured E_1 and E_2 orientations (91° and -3°, as stated on L256) by -2 and +2 respectively. As a result, Figure 3 was generated using prescribed E_1 and E_2 orientations of 89° and -1° respectively. These sentences now read (L277-281):

> For the model, we fixed the E_1 and E_2 eigenvector orientations at 89° and -1° respectively. These values are -2° and +2° from their measured orientations of 91° and -3° respectively, and this adjustment was made to satisfy the orthogonality of eigenvectors of a symmetric matrix. For simplicity, we prescribed both eigenvectors as depth-invariant, given that both their measured standard deviations were only ±6° across the measured depth range of 1500 m (Fig. 4).

EC0.10. P14L315: Now Discussion Section is largely expanded so revise this sentence to narrowly indicate the discussion about this point.

AC0.10. We are not sure which point you are referring to--we have tentatively assumed that it is the sentence about anisotropic scattering (originally L284-285). We have deleted "...the reasons behind elaborated in the Discussion (Section 5)." We have added the following sentence to Section 5.1 in the Discussion (L350-353): "Although the observed anisotropic scattering can be exploited to infer the strength of the third eigenvalue (E_3) under assumptions of fabric isotropy at the ice surface (Ershadi et al., 2021), we do not attempt this method given our observations of significant fabric anisotropy in the firn layer (inset of Fig. 6)."

EC0.11. P15L304: Is beta linearly interpolated and shown in dB scale? It seems that beta is linearly interpolated over the dB scale. Clarify. Also, is it linearly interpolated

between 10-m-separated data points or is it linearly fit using the all data together? Fig. 5 colorbar shows that beta varies very smoothly.

AC0.11. β is linearly interpolated between 100 m depth intervals, where at each 100 m depth interval, β was estimated to the nearest integer on the linear (as opposed to the dB) scale. The range of β on the linear scale is [1 6] which is equivalent to [0 15.6] on the dB scale. We have added the decimal digit in the caption. We have also clarified this in the surrounding sentences, which now reads (L283-286): "At each 100 m depth interval (i.e.100 m, 200 m, 300 m, etc.), β was estimated to the nearest integer before converting to the dB scale and linearly interpolated to match the model depth step of 1 m."

EC0.12. Figure 6 caption (and relevant locations): is the firn correction made only for permittivity anisotropy in the horizontal plane, or both this and mean propagation speed (giving slightly different depths of these data points)? Please clarify.

AC0.12. Thank you for spotting this detail. We have made firn correction only for fabric strength (horizontal permittivity anisotropy) and not for mean propagation speed. We have clarified this in the caption of Figure 6 as well as in the Methods (L213-215): "Firn correction was implemented only for the permittivity anisotropy in the horizontal plane and not for the mean propagation speed (i.e. no depth correction was made to account for the effect firn density on wave propagation speeds)."

EC0.13. Figure 6: consider adding beta to this plot.

AC0.13. We do not implement this suggestion as β is modelled, which contrasts with the content of Figure 6 which are both measured results.

EC0.14. P21L497: Include diameter of the first Fresnel zone. Sampled ice volume is largely constrained by the size of the Fresnel zone, not by the antenna aperture.

AC0.14. We have removed mention of antennas in the section and have added the size of the first Fresnel zone at two nominal depths (L468-472): "In contrast, waveform-based methods average out fabric properties in bulk where, for radar systems, the planar footprint of which the COF is averaged from is dependent on the radius of the first Fresnel zone (Haynes, 2018). This footprint would be approximately 6 m in radius at a depth of 100 m, and expands to approximately 23 m in radius at 1400 m. Therefore, the bulk COF estimates obtained from ApRES is averaged from a much larger area at depth than near the surface."

EC0.15. Table S1: Add two columns to show geographical orientations of E1 and E2. Add \pm before all values showing standard deviations (as it is done throughout the text). I assume that these orientations are measured clockwise, which is not mentioned explicitly; please clarify.

AC0.15. We have added two columns that give the geographical orientations of E_1 and E_2.

The orientations are actually measured counterclockwise, which means that the numbers in the text are slightly off (L258). The cardinal directions for E_1 would then be 110° - 91° = 19°, and for E_2, 110° - (-3°) = 113°. The offset from the flow direction is therefore mod(230,180)° -

19° = 31°. Figure 4f is correct and remains unchanged, and the relative principal axis to the nearest strain configuration of -7° also remains unchanged. The reason for this error is that our code automatically converted all directions to polar stereographic (south) coordinates for plotting purposes, and calculates the offsets in this reference frame. The conversion from polar stereographic orientation to true cardinal orientation (relative to True North) was done by hand at the very end, which therefore resulted in this oversight. We apologise for this confusion and have updated the numbers accordingly.

In light of this, we have made the explicit mention that positive angular shifts in the polarimetric reconstruction results in *counterclockwise* rotation (L256-257), and have updated the numbers accordingly.

EC0.16. COF, ice fabric, etc are used interchangeable ways. Please make the manuscript more consistent.

AC0.16. Done. We have attempted to refer to “COF” when describing the fabric at the crystal scale and “fabric asymmetry” and “orientation” at the bulk (depth-averaged) scale. Please let us know if you agree (or disagree!) with our nomenclature.

EC0.17. WDC can be replaced with “WAIS Divide Ice Core”, “core site” or such. So, please consider avoiding the acronym.

AC0.17. Done.

Review by Martin Rongen

General comments

RC1.1. The authors have addressed all issues raised in the initial review and incorporated most suggested changes. In particular the re-analysis (identifying shv = −svh and a rounding error), triggered by comments from Reinhard Drews, has improved the manuscript.

The discussion sections now offer a more measured comparison between the presented quad-polarimetric measurement and traditional rotational approach. It is now also acknowledged that a direct quantitative comparison is not possible, as no rotational dataset was obtained in conjunction with the quad-polarimetric data. While I generally believe the paper to be in good shape, it would be appreciated if the authors could consider some outstanding issues as detailed below.

RC1.1. Thank you for your critical eye and we hopefully have addressed your remaining concerns in the responses below.

Specific comments

AC1.2. The newly provided Figure 3 (mean power return) and in particular the insets are greatly appreciated and address concerns about the observed reciprocity of shv and svh as raised in the initial review (RC1.12). The reciprocity seems to hold to a very high degree in the deep ice ~1100m. Yet the same can not be said for the second inset ~100m. The explanations offered by the authors (AC1.12) do not explicitly address this depth dependence.
Regardless of the underlying cause, the fabric derivation is likely not applicable in regions where reciprocity is not experimentally observed and it may be warranted to restrict the analysis to depths where it is (in addition to the newly introduced coherence criterion). This may also resolve the surprisingly large anisotropy derived for the firn.

RC1.2. We agree with the reviewer that the reciprocity between *hv* and *vh* in the deeper signals provide greater confidence in the estimates and interpretation of fabric at those depths. However, fabric strength (E_2 - E_1) is calculated using the polarimetric coherence methods (Eqs. 6 and 7) which rely only on the *hh* and *vv* components, and not the *hv* nor *vh* components. Nothing else seems to suggest that the calculated fabric in the near surface (firn, <200 m) is unreal. Given this, we believe our results in the firn layer to be still valid, and we would like to include these results in case some readers are particularly interested in those depth. However, in response to your point, we now explicitly note the lack of reciprocity in *hv* and *vh* at those depths (while also noting that they are not used in the fabric strength calculation) (L359-361) as both an interpretive caution and as a potential avenue for further inquiry for interested readers.

AC1.3. The addition of data from 9 additional close-by locations, not mentioned in the original manuscript, is a great addition to the paper. While I agree with the chosen presentation (relegating most plots to supplementary material), I would encourage the authors to also perform the fabric calculation for these locations.
The distance between locations appears to be small enough for the fabric not to be expected to change significantly (and is partially even within the estimated 1000m beam cone at 1500m depth) and similar to the distance to WAIS which is already being compared to. The spread between sites as well as the average in comparison to WAIS, might also yield more quantitative insight into the reliability of the method.

RC1.3. We have included a 10-panel figure in the Supplementary Information (Figure S11). Although the output E_2 - E_1 values for some panels are more robust than others, the fabric does not change significantly between sites. Although we are able to individually tune the parameters to ensure more robust results for any one site, we choose to use the same parameters as those chosen for Site I for consistency and transparency.

We have added a short paragraph at the end of Section 4.3 to summarise the areal spread of the fabric strength calculations (L330-333):

> Estimates of azimuthal fabric asymmetry at the other 9 sites reveal similar trends, with those situated closer to the WAIS Divide ice core site in general showing a higher correlation with the core-derived fabric estimates (Fig. S11). The match between the ApRES and ice core fabric asymmetry estimates were generally higher

> at depths below 1000 m. Larger errors were observed where ApRES estimates deviated from the depth-coincident ice core measurement.

Technical comments

AC1.4. Page 12, line 244 doubled ”the”.

RC1.4. Done. There was also a similar error at L233 (the line numbers referring to the previous draft) that we also fixed.

Review by Reinhard Drews

General comments

RC2.1. The authors have responded in detail and convincingly to my review. I am glad to see that comments were perceived as helpful and triggered a number of changes. The inclusion of the 10 additional sites is helpful as is the study relating to the mysterious minus sign in the quad-pole synthesizing. The problem is not solved, but it will help others (and myself) to see this written out explicitly. As detailed below, I have only very minor comments on the current version and don’t insist on the implementation of any of them. This is a nice paper and I congratulate the authors for a job well done.

AC2.1. Thank you for your review and we are glad that you approve of the direction of the manuscript! We have hopefully addressed your remaining concerns below.

Specific comments

RC2.2: This study resolves depth-variability of COF on a 15 spacing which is correctly advertised as a step forward compared to previous studies. Maybe it is worthwhile to elaborate a little bit more about the limits in depth-resolution. I suppose that it is linked to calculating the polarimetric coherence phase which contains some vertical averaging, but this is not explicitly stated.

AC2.2. I share the same suspicions on the resolution of the *hhvv* phase being the primary limiting factor of the bulk-averaged depth resolution of fabric asymmetry. Your suggestion of elaborating on this fact is a good one. We have added the following paragraph to Section 5.3 (L409-420):

> In this study, we were able to achieve coherent estimates of fabric strength at depth intervals (the bulk-depth resolution) down to 15 m. In combination with a convolutional derivative, the use of depth averaging improves fabric estimates by reducing noise, removing anomalous “phase excursions”, and isolates the effects of propagation-related phase behaviour with that from scattering (Dall 2010, Jordan et al. 2019). A limitation of this method is that, due to the depth-averaging when calculating the *hhvv* phase, it is not suited to detect and calculate fabric strength at

and crossing fabric boundaries (Jordan et al. 2019). Additionally, the size of the window and filter used is important, especially when applied over sections with high fabric asymmetry. As the depth periodicity of asymptotes present in the *hhvv* phase angle (which manifests in phase wrapping) are proportional with azimuthal fabric strength (Fig. 3), a large window has a greater risk of smoothing over these areas. This caveat may possibly be the reason behind the cluster of anomalously low measured E_2 - E_1 values at depth (Fig. 6). Conversely, a smaller window may naturally produce results with higher variability as a result of lower number of samples used to calculate the bulk-average. Therefore, with respecto the methods used in this study, there is like a delicate balance between the bulk-average resolution and precision.

RC2.3. Another take-away (at least for me) is that COF patterns do not change significantly over this 10 km profile. Also in Ershadi et al. (TCD) differences along a similar transect are not very significant. Knowing this, if I were to collected new data somewhere in the ice-sheet center I would aim for larger distances between sites (but obviously this is site-specific).

AC2.3. We have added the following sentence to the Conclusion (L523-525): "These observations of fabric orientation and strength were consistent for all 10 measured sites along a 6 km-long transect extending away from the core site."

RC2.4. Fig 1a: It seems that some of the compression axis at approximately -460 km polar stereographic northing are¬ blue but should be red.

AC2.4. We have checked the dataset that these measurements come from and they show extension at both orthogonal axes (which is odd). We have removed the smaller extensional axis and made a note in the caption that says that measured strain rates below 2.5 x 10^{-5} a^{-1} are not shown.

RC2.5. Fig 1a: The velocity arrows (black?) are not arrows but start with a dot. Maybe double check that this is consistent with what is shown in the legend.¬¬

AC2.5. Thanks for noticing the inconsistencies. We have modified the legend so that it reflects the dot and line. We have also removed the arrowheads for the strain rate tensors as they were very small.

RC2.6. Fig 1a: Consider putting white line and red dot in foreground. I was looking for it for a while.

AC2.6. The white line and red dot are already in the foreground but you are correct that it is difficult to spot among the other information within the figure. We have made the white star (WAIS Divide Ice Core site) larger and have specified in the caption that the white line and red dot are ~5 km NE of the core site.

RC2.7. L 96: "…birefringence reflects the bulk COF as [WORD MISSING].."

AC2.7. Sorry! "as" has been removed.

RC2.8. L 125 "… eq. 1 relates to microscopic ice fabric anisotropy.." not sure what microscopic refers to here. It could be misunderstood as single-crystal.

AC2.8. We have clarified the various components of this sentence which now reads (L126-128): "This equation directly relates the bulk-averaged (*Δε*) and crystal (*Δε'*) birefringence anisotropy to dielectric anisotropy, which serves as the basis for the radar processing methods that follow."

RC2.9. L 130f: It is possible that I missed it, but it should be clearly defined that the angle \theta is in the horizontal. Also I would prefer to call R' the "inverse" of R which is the same as the transpose in this case. However, possibly this makes it clearer to the readers indicating that R' undoes what R did.

AC2.9. Corrected (for both points).

RC2.10. L 224 state how SNR threshold is estimated

AC2.10. We have moved this sentence up to the last paragraph of the Methods section, and added two additional sentences to explain how we calculate and implement the SNR threshold (L218-222): "We restrict our observations of the co- and cross-polarised measurements only to measurements with sufficiently high signal-to-noise ratios (SNR). For each of the four acquisitions, the SNR was found by calculating the 95th percentile of the noise floor. Observations were excluded from the output if the magnitude of the complex amplitude of any one acquisition falls below the calculated SNR for any one acquisition at a given depth (Fig. 3). "

RC2.11. Figure 4: Nice Figure. Consider adjusting flow arrows in 4f as in comment to Fig 1a. It is unclear if ice flow is from top-left to bottom-right or the other way around.

AC2.11. We have chosen to leave the flow markers and tensor as is (this is a direct copy of Conway & Rasmussen 2009). We have written the flow direction (230°, SW) in the caption.

RC2.12. L 490 add reference point to the 21 \pm 8 degrees (e.g., relative to magnetic North..)

AC2.12. Added the reference point (to true North).

RC2.13. L 440 For information: COF in the NEEM ice core is (and will be) measured at much much higher vertical resolution. Those results will be interesting.

AC2.13. This is interesting, thank you for the information!

References

Conway, H. and Rasmussen, L. A. (2009) Recent thinning and migration of the Western Divide, central West Antarctica, Geophys. Res. Lett., 36(12), 1–5. doi:10.1029/2009GL038072.

Young, T. J., & Dawson, E. J. (2021). Quad-polarimetric ApRES measurements along a 6 km-long transect at the WAIS Divide, December 2019 (Version 1.0) [Data set]. NERC EDS UK Polar Data Centre. doi:10.5285/BA1CAF7A-D4E0-4671-972A-E567A25CCD2C