Author's response to review by anonymous Reviewer # 2

January 12, 2025

Dear Reviewer,

Thank you very much for your thoughtful and constructive feedback on our manuscript! We are grateful to hear that you find our work valuable.

We value your detailed suggestions in making this work more accessible, in particular your input on improving figures and text clarity. Thank you also for being critical on whether specific sections are really necessary to deliver the most important messages. We have carefully implemented your comments in the revised manuscript which is now shortened, more focused and hopefully more accessible to a wider glaciological audience. Please find the point-by-point response below.

Thanks again for taking the time to review this paper.

On behalf of all authors,

Tamara

1 Major Comments:

[1] Since most of the conclusions are based around the idea that anisotropic scattering signatures will have 180-degree periodicity vs 90-degree periodicity for birefringence, it would be incredibly helpful to the reader to spend a few sentences describing why this is the case. I think I've convinced myself that it must be due to the integrated two-way path propagation effects for birefringence, but discussing these ideas explicitly would be great for readers who are not deep experts in radar measurements of fabric.

Thanks for this comment. We have added an explication for this at the beginning of Section 3 which hopefully gives the readers some intuition, (line 120–134 in revised manuscript). However, as this characteristics has been known for around five decades now in the radio glaciology community (see Hargreaves, 1978) and discussed in a number of papers, some of which also referenced in this manuscript, we would prefer to refrain from a further detailed description.

The effects of anisotropic scattering and birefringence can be distinguished by their periodicity of co-polarized power anomalies, dP_{HH} (for definition see Section 1 in the supplementary Information or Ershadi et al., 2022). These patterns differ because the two mechanisms are governed by distinct symmetries.

Amplitude variations due to anisotropic scattering originate from variations in scattering properties within the ice that have a two-fold symmetry. This means that the returned signal is strongest in two opposite directions, resulting in a 180° periodicity when co-polarized antennas are rotated.

In contrast, birefringence splits the transmitted radar wave into two orthogonal components that travel at different speeds through anisotropic ice. The relative amplitude of these two wave components depends on the orientation of the antennas relative to the COF axes, while the phase difference depends solely on the degree of anisotropy, but is independent on the antenna orientation. Interference between the two wave components modulates the return power. A 90° rotation of co-polarized antennas flips the amplitudes of the two components, but the interference pattern remains unchanged. Therefore, birefringence creates a 90° periodicity in the co-polarized signal.

Both mechanisms exhibit 90° periodicity in cross-polarized anomalies, dP_{HV}*, because the transformation of the polarization state, whether through scattering or birefringent phase shifts, inherently alternates every quarter-wave (90°), driven by the geometry of the polarization ellipse.*

[2] Section 3.1 rederives a polarimetric scattering model from Fujita et al. (2006) to model the radar response to COF at EGRIP. In the end, this model does not seem to be central to the paper's major conclusions, especially since there are some significant differences between the modeled and measured polarimetric responses. To my reading, the model provides a very broad sanity check on the measured polarimetric response and is briefly used to justify the argument that COF variability drives the deep anisotropic scattering. Considering this, I think that the discussion of the model can be much more concise and probably just point readers to Fujita et al. (2006) model, which will lighten the mental load for readers.

We have considerably shortened Section 3 and removed the model equations and refer to Fujita et al., 2006 instead. We moved Fig. 3 and Fig. 4 into the Supplementary Information with the necessary details to reproduce our results in order to improve the readability of the manuscript and focus more on the essential parts. Section 3 now focuses primarily on how we synthesized the polarimetric response and on the curve-fitting method to determine the relative importance of each contribution.

[3] Almost a quarter of the paper (pages 6-12) is devoted to convincing the reader that the quad-pol synthesis can be trusted. I actually do not think that level of detail is necessary and bogs the reader down in a long technical discussion before they ever get to the main methods of the paper. The quad-pol synthesis method is firmly rooted in the governing equations of electromagnetics, has been demonstrated multiple times with quad-pol ApRES in our field, and is routinely used outside the field in other radar applications. If anything, the turning circle may be less reliable because it aggregates the polarimetric response over a series of radar footprints that do not fully overlap and may be subject to effects from layer slope, for example. Therefore, I think it is totally sufficient to just cite the quad-pol synthesis method and make this section as concise as possible. To me at least, the main value of the comparison with the turning circle is to demonstrate that the quad-pol instrument has sufficient radiometric calibration and phase synchronization across channels, a motivation which was surprisingly not mentioned in the paper.

We agree that the comparison with the turning circle is not crucial for the main conclusions of the paper. As mentioned above, we have considerably shortened Section 3 and moved Fig. 4 to the

Supplementary Information. There we also added a sentence regarding the radiometric calibration and phase synchronization across channels.

[4] You might consider breaking out the discussion of the sinusoidal fit into its own section. This is the main analysis method that is used throughout the rest of the paper, so it would be very valuable to give it a clear emphasis rather than burying it at the end of the discussion on the quad-pol synthesis.

This is now the main aspect of Section 3, which has become much shorter.

[5] Overall, I would encourage you to think carefully about the specific purpose(s) of presenting the turning circle-synthesis-model comparison and be explicit about this purpose at the beginning of the section. Then limit the technical details and discussion of the comparison to the most salient points that are needed to support that purpose.

See previous comments.

[6] I found Figures S7-S13 really helpful for following the discussion of how anisotropic scattering vs. birefringence varied with depth. If at least one of those plots could be added to the main paper, I think that would be very valuable. For example, perhaps adding a fourth panel to Figure 6 (or Figure 7) with the amplitude of each sinusoid as a function of depth for each location a-j.

Thank you for this suggestion. We have added another row of panels in both Fig. 6 and Fig. 7 showing the amplitude-depth profiles.

2 In-Line Comments:

- **Line 40:** since dual or quad-pol satellite SAR is also used in many glaciological applications and has a different viewing geometry, it would be good to specify something about radio-echo sounding here.

  We specified this as follows (line 27-28 in revised manuscript):

  For nadir wave propagation, the standard setup in ice-sheet RES, these polarizations lie in the horizontal plane, assuming that one eigenvector is vertical.

- **Line 58-60:** the mention of optical anisotropic scattering seems unnecessary since this entire paper is about radio frequency measurements.

  We have removed the reference to the optical spectrum.

- **Figure 2:** you might consider marking ice flow directions and adding labels for inside vs. outside the ice stream and the shear margins in this image, just to help the reader who otherwise has to flip back and forth with Figure 1 quite a bit.

  Thanks for this suggestion — we have added flow directions and shear margin locations to Fig. 2.

- **Line 110-111:** I would recommend adding a few comments on the final horizontal resolution and trace spacing after processing, and perhaps why SAR focusing was not employed.

  Thank you - we added the following clarification (line 102–105 in revised manuscript):

While synthetic aperture radar (SAR) focusing of the RES data could improve some radargram sections, challenges related to irregular tracks and high bandwidth complicated motion compensation, limiting overall improvement. Consequently, SAR focusing was not employed. The final trace spacing after processing is approximately 25–30 m on average, while it is approximately 0.7 m in the turning circle.

- **Figure 3:** the colors in panel a are hard to distinguish due to the black outlines, particularly the purple.

  Thanks for flagging this. The line thickness has been decreased to improve color visibility. This Figure has also been moved to the Supplementary Information (Fig. S1).

- **Line 196-197:** where does this reflection ratio come from and what justifies this choice?

  We reduced the scattering amplitude derived from COF variations in order to better match the scattering amplitudes observed with the radar. The reflection ratio derived from COF may be too high because of the low sampling rate of COF measurements, so the eigenvalue variations might be 'smoother' and thus the reflection ratio lower than captured by the COF record.

  This section was moved to the supplementary Information, but we've added a brief explanation there (Supplementary Information Section S2):

  The amplitude of the COF-derived reflection ratio in the model is nearly twice as high as that observed for anisotropic scattering in RES data. This discrepancy may stem from the low sampling rate of COF measurements, which might fail to capture eigenvalue variations that are smoother in reality. Consequently, the actual reflection ratio may be lower than suggested by the COF data. To account for this, we use a value of 0.5 $r[dB]$ for comparison with the RES data.

- **Figures 6-7:** it would be fantastic to add markers in some way for the same isochrones which are shown in Figure 9. This would help the reader better visualize how changes in the azimuthal response with depth are related to stratigraphic units and age. It would also be very helpful to have some annotations showing the key features that a reader should take away from the dP_{HV} and Φ_{HHVV} plots. They only get 3-4 sentences in the discussion, and I found it a bit hard to track the key points that I should take away from these plots.

  Thanks for this suggestion, we have implemented the depth of the isochrones in Fig. 9 in Fig. 6–7. However, we find these two Figures now getting quite busy, and therefore hesitate adding further annotations on $\mathrm{dP_{HV}}$ and $\mathrm{\Phi_{HHVV}}$ panels. Instead we have elaborated on these two aspects a bit more in the text (line 176–192 in the revised manuscript) with reference to the corresponding panels in Fig. 6–7.

- **Figure 8:** I find the high frequency spatial variations in the apparent horizontal eigenvalue difference hear the eastern shear margin very notable. Do you have an idea of what might cause this? Is this "real" or an artifact of low signal to noise ratio and the vertical "streaking" that we commonly see in shear margin radargrams due to dipping layers and/or damage? Thank you for pointing this out. Indeed, these fluctuations are artifacts due to steeply inclined layers, where the correlation between traces is decreased and anisotropy likely underestimated. We've revised the automated algorithm we used to derive eigenvalue difference and implemented stricter thresholds for where the trace correlation is good enough that we accept the derived eigenvalue difference as quite reliable. We added this in line 194–201 in the revised manuscript:

The eigenvalue difference was determined using an automated process that measures the travel-time difference between the HH and VV traces. Specifically, the cross-correlation of each trace pair was calculated within a 20 m sliding window to estimate the time delay between signals. Linear regression was then applied to correlated reflections to obtain the depth-averaged apparent eigenvalue difference (for method details see Gerber et al., 2023). The uncertainty of this method increases when only shallow reflections are available or when the number of reflections is low. To ensure reliability, we included only results where at least ten internal reflections could be correlated with a correlation coefficient above 0.6, and where at least one reflection lies below 1200 m depth. Results were discarded where these criteria were not met, particularly in areas with steeply dipping internal layers near shear margins.

- **Lines 293-294:** how can we know that there is isotropic scattering if the region is “echo-free”? I would guess this just reflects the isotropy of thermal (e.g. white Gaussian) noise rather than something about the ice sheet?

  Thank you for pointing out this confusing notation, we were referring to horizontally isotropic by absence of anisotropy, but removed this term, as ’echo-free basal zone’ is sufficiently accurate.

- **Section 5.1.2** I’m not entirely convinced by this discussion on the direction of folding vs. scattering. In the citations in this section (Bartalis et al., 2006 for example), anisotropic scattering occurs because the radar is side-looking and so in one orientation the folds act like corner reflectors (high backscatter) and in the other orientation they do not (low backscatter). It’s less clear to me how this would work for a nadir-looking radar sounder. My first thought is that you might have stronger co- polarized scattering parallel to the folding axis in the same way that backscatter from a half cylinder can (in some cases) be strongest when the wave polarization is aligned with the long axis of the cylinder, rather than perpendicular to it (see for example (Scanlan et al., 2022)). Anyway, this would further support your argument that roughness is likely not the cause of the anisotropic scattering you observe, but it is worth thinking through the mechanisms in this discussion in the context of radar sounders a bit more.

  Thank you for being critical on this section and for pointing out the paper by Scanlan et al., 2022. The relation between anisotropic scattering and layer roughness is, admitedly, more complex than what is outlined in the paragraph, and depends on a variety of factors such as radar beamwidth, roughness amplitude and wavelength. I think it is indeed difficult to understand how exactly it would affect our radar return. We have adressed this issue by 1) clarifying that the findings by Bateson and Woodhouse, 2004 and Bartalis et al 2006 are for side-looking radars and 2) that for nadir surveys the scattering direction might be opposite (as found by Scanlan et al, 2022), which 3) makes it unclear how exactly anisotropic roughnesss would affect radar returns.

  However, we still think it is safe to discard interface roughness as the major driver of anisotropic scattering for two reasons: First, the difficulties to explain reversed scattering directions between ice units and second, the only marginal differences between scattering amplitude inside (where roughness is expected to be higher) and outside NEGIS (where roughness is expected to be lower).

  The revised text now is (line 275–294):

The effect of directional interface roughness on radar return power is complex. Interface roughness can transition radar signals from specular reflection to more diffuse scattering and wave depolarization when roughness amplitudes are comparable to the radar wavelength (Peters et al., 2005; Giannopoulos and Diamanti, 2008). Studies with side-looking radars have shown that higher backscatter occurs perpendicular to the folding axis, as folds act as corner reflectors (Bateson and Woodhouse, 2004; Bartalis et al., 2006). However, for a nadir-looking radar system with a much narrower beamwidth, this anisotropic scattering mechanism may not operate in the same way. Instead, stronger co-polarized scattering might occur parallel to the folding axis, depending on fold size and radar characteristics (Scanlan et al., 2022). Despite the unclear relationship between folds and anisotropic scattering, we can rule out directional interface roughness as the major source of anisotropic scattering for the following reasons: First, if directional interface roughness results from ice dynamics, particularly lateral strain, we would expect layers outside the ice stream to be smoother, with less pronounced anisotropic scattering. Indeed, the scattering amplitude is generally slightly higher inside the ice stream than outside (Fig. 6). However, this pattern is not consistent. For example, scattering amplitudes outside NEGIS in profile B exceed the amplitudes in the ice-stream interior particularly downstream of EastGRIP and in profiles which are not in the ice-stream center (Fig. 6a–c). Although roughness outside the current ice stream might be remnants of previous ice-dynamic configurations, the spacial distribution of scattering amplitudes is difficult to be explained by roughness alone, particularly the lower amplitudes towards ice-stream margins where folding amplitudes are known to increase (Jansen et al., 2024). Second, while scattering differences between ice from different climate periods could stem from variations in folding amplitudes associated with viscosity differences, the reversed directionality of anisotropic scattering between Holocene and Wisconsin ice north of the NW shear margin would imply an exceptionally distinct strain history between these ice units if attributed to ice-flow-induced interface roughness, which is unrealistic.

- **Line 399:** is there any evidence for a COF-induced reflection at this transition (e.g. an englacial layer in the radiostratigraphy marking what appears to be a quite abrupt transition)?

  We did not identify evidence for a reflector explicitly caused by a change in COF type. Reflections at the Wisconsin-Holocene transition are commonly observed in radar echograms due to differences in ice acidity, which affect conductivity. However, changing impurity content can also cause COF changes which then would appear in radar and seismic data (Horgan et al., 2008). While COF-induced reflections are typically distinguishable by their anisotropic nature, in this case, the strong anisotropic scattering observed in the data may obscure any reflection resulting solely from a COF change.

- **Lines 407-415:** this is a very interesting and important piece of the discussion! I will admit I found it a bit hard to visualize how the COF would be changing with depth to achieve the anisotropic scattering, and I wonder if you might consider adding a conceptual diagram. Maybe some idealized Schmidt diagrams as a function of depth to explain how you envision the fabric changing?

  Based on a suggestion of reviewer 1, we have moved this paragraph to the introduction, in order to provide this context of our analysis early on in the manuscript. Additionally, we have tried to visualize our idea on how anisotropic scattering might be caused by different COF types in two different locations, inside the ice stream, where COF is known at the EastGRIP drill site, and in the folded units north of NEGIS, where the COF is unknown. We hope this additional Figure (Fig. 8 in revised manuscript) helps readers to understand the concept, even though some of it remains quite speculative.

- **Line 441:** I am not entirely following how the folding/overturning of stratigraphy would lead to this expression of anisotropic scattering – perhaps you can expound on this a bit? (Maybe this is something else that could be part of an idealized fabric as a function of depth sketch?)

  We suggest that the difference in anisotropic scattering is related to ice units formed under different (climatic) conditions, but we don't claim that the folding/overturning itself lead to a change in scattering properties. The fact that different units within these large folds outside of NEGIS exhibit distinct scattering properties could indicate that ice from the Eemian period is present between the 65.8 ka and 74.7 ka isochrones within these folds.This would mean that the stratigraphy there is disrupted, and possibly overturned. In that sense, anisotropic scattering can be indicative of overturned stratigraphy, but not so much the result of it per se.

  We have addressed this comment in line 344–351 in the revised manuscript:

  In Fig. 8, we proposed a potential mechanism for the reversed scattering pattern, though we do not claim to fully explain the formation of these COF differences. Ice from colder periods, like the Wisconsin, tends to have higher impurity content and smaller crystals, promoting easier deformation compared to ice from warmer periods like the Holocene and Eemian (Paterson, 1991; Cuffey et al., 2000; Faria et al., 2014a,b). The folding of ice itself does not inherently produce a 90° rotation of COF needed to invert the scattering signature. However, changes in the regional ice dynamics could have altered the local strain regime to which the COF adjusts accordingly. The rate and manner of this adjustment may differ between ice units, with Wisconsin ice, having generally higher impurity content and smaller grains, potentially adjusting more rapidly or distinctly than Holocene ice, which could explain the observed scattering differences.

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