**Review to Ershadi et al. 2021**

**Summary**

Ershadi et al. 2021 introduce new technical developments within polarimetric radar-sounding to better estimate ice fabric. The novel aspect is an inversion/automation framework that uses radar reflection properties as an additional constraint to recover vertical fabric information, and hence the full second-order fabric orientation tensor (under the assumption the near-surface is isotropic). The inversion framework is validated via comparison with ice-core fabric data at EDML and EDC. In addition to the technical advances, the authors explore spatial variation in ice fabric around the core sites and relate to the ice-dynamic context.

The recovery of the full second-order fabric orientation tensor is a highly significant step forward on current polarimetric radar-sounding methods. It is also very useful for future studies that could use the method to parameterize anisotropic ice-flow models. The paper is well-written and referenced, with attractive figures, and, whilst being quite technical, is well-suited to TC.

Despite my overall positive impression, there are a number of things that should ideally be addressed to improve the paper - see specific comments below. The first of these comments is critical (and is the only reason why I recommended major corrections), as I think there is an inconsistency regarding the definition of `anisotropic scattering' that could require a re-run of the model inversions. My hope/intuition is that any differences in overall results should be small, and my suggested change may result in improved performance of the inversion, as sensitivity should be reduced.

Best regards to the authors – I enjoyed reviewing their paper.

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1. **Definition of anisotropic scattering parameter (E-field amplitude versus intensity/power)**

A key thing to fix within the MS is how the anisotropic scattering parameter (currently called r) is defined. In particular, I believe there is an inconsistency between the reflection matrix, eq. (11) (where r should be defined in terms of the ratio of the electric-field reflection amplitudes/diagonal matrix elements – e.g. Fujita at et al. 2006), and eq. (12) (where r represents an intensity reflection ratio).

From the E-field definition, and combining with the small-permittivity form of the Fresnel equations (Paren 1981), it follows that *r should be linear with respect to the vertical eigenvalue contrast*, following:

r=gamma_y/gamma_x=(lamda_2,i-lamda_2,i+1)/ (lamda_1,i-lamda_1,i+1),.

However, in eq. (12) of the MS , r=gamma_y/gamma_x, is defined to be the square of the above - i.e. gamma_y and gamma_x are taken to represent intensity reflection coefficients, and hence *r is assumed to be quadratic with respect to the vertical eigenvalue contrast*. This results in the fabric eigenvalues having a different scaling relationship with the forward model than they should.

In terms of presentation in the MS this is easy to fix, and I'd recommend:

> i) Being explicit that gamma_x and gamma_y represent E-field amplitude coefficients (e.g. L 154), which in general can be complex and/or negative.

ii) Define eq. (12) as R=|r|^2 =(gamma_y/gamma_x)^2=(lamda_2,i+1-lamda_2,i)^2/(lamda_1,i+1-lamda_1,i)^2, as an `intensity reflection ratio' (rho is another option as I can see R is used for rotation). By convention, most optics fields use lower case for E-field reflection amplitudes, with the upper case for intensity, so I think this the best solution here. Similar, for eq. (29).

iii) Continue to use R within the rest of the MS for the anisotropic scattering parameter (using [R]_dB notation could also be useful to distinguish dB from the linear R form).

In terms of the impact on the results, I am less certain. If it is not just a typo/lack of clarification then, due to the linear rather than quadratic scaling, there will probably be some differences in the results on the numerical inversion, and the results should be re-run. It also follows from the linearized version of eq. (12), that r can now be negative (due to `reflection polarity'), so the impact of this may need to be considered within the inversion. The inversion framework appears adaptable to this change, so I hope this ends up with similar end-results that match the core data.

**2. Fresnel assumption for anisotropic scattering**

Linked to the point above, in applying eq. (12) the author's make the assumption that the reflection coefficients can be described using the Fresnel equations. They therefore implicitly assume that the radar reflections originate from a single sharp contrast between two adjacent fabric layers within a radar range cell. It is my understanding that the Fujita scattering model does not make this assumption (it makes a less restrictive assumption that the reflection is specular, which can accommodate for multiple scatterers/vertical fabric contrasts within a range cell). Eq. (12) should therefore be introduced as an additional assumption to the standard scattering model framework. Note – I think for the high bandwidth resolution ApRES data this assumption likely to be less of an issue than lower bandwidth radar systems, where the observed reflection will be more likely to be aggregated over multiple interfaces.

**3. Motivation for inversion/automation framework**

The inversion/automation framework is a very useful step forward, but I think it could be better motivated for the reader who is less familiar with the sub-field. In my opinion (other than extracting the vertical eigenvalue), a key advantage is how the new method is able to extract fabric rotation within the ice column. Previous (not automated/'via inspection') methods only perform well for the case that the fabric is depth-invariant, so extending to extract rotation within the ice column is significant.

**4. Significance of coherence magnitude**

One of the key advantages to using coherence methods is that the coherence magnitude, |c_hhvv| gives an idea of uncertainty/phase error. It would therefore be useful if the authors could: first introduce the significance of |c_hhvv| in terms of phase error; second include depth-azimuth plots of |c_hhvv| along-side phi_hhvv. To preserve the figure labels, this could even be done as a grey-scale underlay, as is sometimes done in the inSAR literature. Low coherence magnitude can be an issue in applying polarimetric methods to other regions of the ice sheets (particularly where radar layering is less ordered), so this would be very useful for comparative purposes, and to justify what parts of the ice column phi_hhvv can/cannot be trusted within the inversion.

## 5. Incorporation of uncertainty within inversion

This relates to the point above, and within the inversion framework, I think there are opportunities to incorporate more information on measurement uncertainty. For example, |c_hhvv| and its relationship with phase uncertainty could be used to weight different depth/azimuth bins in eq. (23). Additionally, the authors mention that there is a ±15° uncertainty for georeferencing the data (which seems higher than I would have expected!). In theory, this alignment-uncertainty could be propagated via the quad-multi pol basis change into the scattering model and inversion. As significant developments have already been made in this paper, I think its ok for the authors to flag these as desirables for future work if this ends up  being highly time-intensive.

## 6. Plotting of phase gradient sign/polarity as evidence for fabric orientation and rotation

Is there a good reason why the scaled phase derivative is defined as being positive in the data analysis (Fig. 5 and 6), whereas in the synthetic model it can be positive or negative (Fig. 3)? The sign of the phase gradient is the easiest way for the reader to connect the data analysis to the fabric orientation, and azimuthal rotations within the ice column (as explained in Appendix B).  As it stands, I don't think the reader will be able to easily identify that there is an azimuthal rotation in Fig. 6, therefore, I recommend plotting the phase gradient with sign/polarity rather than the magnitude in the results figures. Also, I think it would make sense to better guide the reader about the `polarity depth-transition' being the marker of the fabric rotation within the ice column (Fig. 6).

## 7. Cross-check with `vertical variability' of ice-core eigenvalue data

It would be interesting to compare the ratio of the vertical variability of the lambda_1 and lambda_2 ice-core eigenvalues, with the inferred anisotropic scatting ratio. Whilst the vertical resolution of the ice cores will be different from the eigenvalue contrast related to a radar reflection, I would nevertheless expect them to be correlated – e.g. when |r| is relatively high, I would expect the vertical variability of lambda_1 to be significantly different to lambda_2. This would be very interesting to comment on, and from looking at Fig. 5 and 6, I think there is likely to be different vertical variability of lambda_1 and lambda_2 present. Related to this – I wonder if the authors know of any dynamic significance of there being higher/lower variability in lambda_1 versus lamda_2? As the vertical r profiles are a novel feature of this paper, this would be good to comment on.

## 8. General significance of recovering the vertical eigenvalue for ice-flow modeling

The recovery of the full second-order tensor is highly significant for parameterizing ice-flow modeling with radar. To maximize the impact of the paper, I think it would be nice to add a brief section to the discussion on why this is the case. In particular: measurement of lambda_3 will give an indication of how the fabric ice becomes softer to shearing with the ice column. Some relevant anisotropic ice-flow references have been included, so these could be incorporated in this discussion here.

## 9. Alignment with ice flow (versus compression) & glaciological interpretation of fabric rotation

It is my understanding that rheological theory predicts that v_2 should be aligned with the lateral compression axis. For an extensional flow, the fabric is therefore is predicted to be perpendicular to the ice flow direction. I was therefore slightly surprised that v_2 tends to be aligned with the flow in the near-surface at EDML. Is it because near-surface compression acts along flow here? This relates to: L390-393, as are maximum and minimum the wrong way around here? I originally assumed that v2 should be aligned with across-flow compression (minimum strain) and v1 with the along-flow extension (maximum strain). In summary: it may make more sense to plot against the lateral compression axis at aswell/instead of ice flow.

Related to this point, an azimuthal rotation of the fabric is inferred at ice depth ~ 200m at EDML. As far as I can see, the glaciological significance of this isn't discussed in any detail. It would be useful to comment on this within the discussion, particularly in relation to the understanding of ice-flow history in the region (i.e. do we expect the lateral compression axis to have been different in the past?).

**10. Notation/terminology conventions**

I appreciate that notation can be subjective, but nevertheless, here are some suggestions that I have:

*Theta/alpha angle convention*. I think this is the other way round than in some other papers. For example, Matsuoka et al. 2012, Jordan et al. 2019, use alpha for the angle between the polarization planes and the eigenvectors, and theta for the geographic angle, whereas it is the other way round here). I don't think this matters, but this distinction with the other studies should be made clear.

*E-field notation.* It is also unusual, I think, to use lower case **e** for electric field. I'd recommend **E** as **e** is typically used for basis vectors in polarimetry studies.

*HV notation*. I was slightly confused by this, as it appears that H and V are used to describe measurements in both a fixed basis (i.e. the fixed geographic system where the quad pol data is measured), and a rotating basis (the coordinate system that phi_HHVV is analyzed in to extract the fabric). In Jordan 2019, 2020 H and V are reserved for the rotating-basis (i.e. they are a function of theta). If the authors want to make their description of phi_HHVV equivalent, then this distinction needs to be made clear – e.g. use H_0, V_0 for the quad-pol/fixed basis.

*Scattering matrix* – by convention the upper right element is normally HV (or 12, xy etc), and the lower left is VH.

*Power anomalies* – as Pxx , eq .(19) is a `power anomaly', I favour `Delta P' notation (I think Matsuoka et al. 2012, sets a good convention to follow for polarimetric power standards).

**Minor comments/technical corrections**

Should Eigenvalue and Eigenvectors be lower-case?

**Abstract**

It may be helpful that state you can `recover both horizontal and vertical anisotropy' with the inversion method, whereas previous radar methods can only recover horizontal. If it were me, I would probably state the key limitation of the method in the abstract and conclusion (the fact that isotropic ice needs to be present in the near-surface is a pretty strict requirement).

L4   It is important to note that in the inversion framework, the anisotropic reflection information is being `added' to ice birefringence - e.g. state as: ` ..approach within radar polarimetry, that combines ice birefringence and anisotropic reflection…'

L4 – It is helpful to be specific and put `complete second-order orientation tensor' (due to the fourth-order representation that can be used in seismics).

L11 – I think `that ice fabric horizontal distribution' needs re-wording

**Introduction**.

L31 – As this follows the section on optical microscopy, I think It is important to better-qualify that radar is observing a fundamentally different dielectric anisotropy of ice, which is the combined effect of the ice crystal birefringence and the crystal orientation fabric. Due to this  distinction, I'd say that radar is employing `similar' rather than the `same' principles to optical methods to recover fabric (i.e. it is based on measuring a bulk anisotropy rather than an intrinsic).

L39 –Is there a typical value for `high resolution' to add here?

L49 – Li et al. 2018 is another good reference for applications to ice domes: https://tc.copernicus.org/articles/12/2689/2018/tc-12-2689-2018.pdf

L51 – To the best of my knowledge, Robert et al., 1993; Joughin et al., 1999 are not examples of ice-penetrating radar polarimetry/fabric estimation within ice streams (at least, not in the same sense as the domes, divides, and rises references). Maybe they should be added elsewhere?

L54 – It would be helpful to guide the reader here. e.g. `forward modeling framework which relates the vertical distribution of ice fabric to the polarimetric radar signal'

Table 1 - This table is very useful to include! I think `Dielectric permittivity matrix' should be more precisely defined as `principal dielectric tensor'.

**Section 2**

L 67 the ice flow

L70/71 – I assume ` largest principal strain' and `maximum strain' will be the same?  If so, it would be helpful to use the same term throughout.

**Section 3**

3.1. Within this section, I would briefly describe how the 1,2,3 axes are typically assumed to orientated relative to the vertical.

L 78 –I'd  check the wording of this sentence

L 80 – It would be helpful here to be specific that you are describing the second-order orientation tensor representation

L 124 birefringence  >birefringent

L185 -I assume the derivative was applied to the real and imaginary components of the complex coherence?  This should be made explicit.

**Section 5**

L331 I'd replaced `dielectric orientation tensor' with `second-order fabric orientation tensor ' (strictly the dielectric tensor is a separate object).

L339 – `*However, more systematic investigation is warranted if this also holds when the ice fabric is not aligned vertically.'* I think the basis transform/polarization synthesis should hold irrespective of the vertical alignment, so multi-pol should always map to quad-pol for the case of no georeferencing error). The respective SNR could be different though.

**Appendices**

B - I think it would be helpful to guide the readers why a `triangular space' arises in Fig B1 (due to the constraints $\lambda_1+\lambda_2+\lambda_3=1$ and $\lambda_3>\lambda_2>\lambda_1$).

C- I think $z_1$ should be $z$ in eq. C5?

D – This derivation is nice to include. I think if you were to remind the readers how the k components relate to permittivity or refractive index and phase velocity, then it would help guide the reader.

E - I like the quad/multi-pol comparison! I think comparing the coherence magnitude and power-SNR are the most interesting thing to look at here, as these are what will differ between the methods. The inclusion of cross-mode (HV, VH) terms, which generally have lower amplitudes than HH, VV is relevant.

F – If you agree with my change to eq. (12), then sqrt(r )should be replaced by r , and r with R.

**Figures**

Fig. 1 - If it is easy to do, then it would be helpful to include a velocity scalebar.