Dear Kris,

we thank you for the careful reading of the manuscript and the overall positive opinion on the work. Your valuable comments helped to significantly improve the manuscript. Below please find your comments pasted in black and with our replies in blue.

As a result of all reviewer comments, the major changes in the manuscript comprise: A revised introduction, an extension of the method section, a revision of Sec 4.4. We also realized that, due to to filename inconsistencies, some load states were previously ignored in the overall optimization. This is now corrected. Therefore the fit parameters slightly changed, but without any implication on the results.

Kavitha Sundu (on behalf of the authors)

In the paper under review, the authors characterize the contribution of geometric anisotropy on elastic moduli for snow, firn, and bubbly ice. Specifically, the authors propose a normalized upper Hashin-Shtrikman bound for elastic moduli that encompasses a range in porosity from 0 to 1. Under this scheme, the geometric anisotropy ratio and fabric tensor is related to the elastic moduli by using an Eshelby tensor. The behavior of the elastic moduli is simulated using finite element methods via volume averaging on 395 images taken with X-ray tomography. Although the methods presented herein are not novel (as indicated in the referenced models), the normalization scheme presented in the present work provides an excellent fit to the simulated outputs for elasticity of dilute dispersion of spherical cavities and is, relatively, computationally inexpensive. Moreover, all five components of a transversely isotropic elastic modulus for snow, firn, and bubbly ice used in the present work were predicted using 2 parameters rather than 5 (required for simulations referenced in the present paper) in calculating an orthortopic elasticity tensor. The authors note the influence of both geometric and crystallographic anisotropy in the range of densities from snow to ice. At lower porosities, the contribution of geometric anisotropy is greater than that of crystallographic with a volume fraction around 0.7 (and has appreciable contribution to the elasticity moduli even at densities past the bubble close off density for firn/ice). At higher porosities, the influence of these two effects switches, such that crystallographic anisotropy dominates the behavior of the elastic modulus at greater depths in the firn/ice. However, the point at which this transition occurs is not resolved in the present study and should be a discussion for future work. Although not entirely novel, the present study provides the cryospheric sciences a new method for characterizing the elastic moduli across the range of porosities for snow to ice and the relative contributions of geometric and crystallographic anisotropy across the full porosity range (0 to 1 for snow to ice, as defined in the present study). I would suggest publication, provided the authors resolve the following major and minor points.

Major Comments

1. The authors remark in Sec. 2.2.1 that empirical parameters in Eq.4. need to be estimated by fitting to experimental data. At least it should be explicitly stated that constraints on a_ij and b_ij have not been made, and there has yet to be a widely agreed upon model based on laboratory and/or field measurements of snow to ice porosities. This is a serious limitation in comparing model

outputs in the present work for the elastic moduli to that of the FEM simulations (and other model comparisons, such as presented on in Fig. 3.). Moreover, it would be nice to get a brief description on the conditions under which the a_33 and b_33 components were obtained.

The description has been extended, and the conditions for the estimates of the Gerling parameters have been included (likewise for for the other models). We are limiting ourselves here to the comparison with Gerling, since these experiments are, to the best of our knowledge, the only ones where a measured elastic modulus was to agree with the FEM-based estimates, as employed here for the parameterization. In general there is a wide agreement from other field or lab tests that the exponent b_ij is "large" i.e. somewhere between 4 and 6. Sometimes the power law is even replaced by a yet stronger exponential increase as explained in the beginning of Sec 2.2.1. Note, that the parameter stated here in Sec. 2.2.1 are simply the values as obtained in the original work (also by fitting). In addition, our Sec 3.5 contains the description of how we re-derived the fit parameters a_ij and b_ij from the present data which are stated in Table 2.

2. In Sec. 3.2, the two-point correlation function is defined and computed via fast Fourier transform of the 3D tomography images. It is noted that, if using the model presented in Eq.(9), (11), and (12), which showed the best agreement to FEM simulations of elastic moduli compared to other models presented on in the study, only two parameters are needed to determine all five components of the elasticity tensor, ζ and β. A possible limitation of using an anisotropy parameter as defined in Sec 3.2. and the accompanying appendix, is that it requires knowledge of the correlation lengths using 3D X-ray tomography images, which may not be widely available or accessible to those in the broader snow, firn, and ice communities.

We agree that the proposed parameterization relies on advanced structural characterization (e.g. the correlation lengths) which are presently only accessible by a limited number of methods. However we believe that advanced microstructure characterization has become a standard world-wide in the last decade due to X-ray snow and firn imaging facilities in the US, Japan, Norway, Germany, France, Switzerland. As a result, these advanced metrics and improved parameterizations derived from X-ray tomography led to the developments of alternative retrieval methods, e.g. the anisotropy characterization from radar (Leinss et al 2016). We therefore believe that our work contributes well to this mutual stimulation of developments. We extended the introduction to point out the increasing use of X-ray imaging and advanced microstructural metrics for the understanding of snow, firn and porous ice.

3. The authors should consider including the temperature time series presented on in Figures 6 an 7 and discussed in the concluding remarks. To that end, it is not clear, at least from how the model in Sec. 2.6 and the accompanying Appendix are presented, how the elastic modulus (or, similarly, Eshelby tensor) depend on temperature. It is clear that there is an effect on anisotropy that is due to temperature effects, however, without a formulation for the dependence of the anisotropy ratio or Hashin-Shtrikman upper bound of the effective elastic modulus on temperature one would expect it difficult to implement the model presented on the in the current work.

We agree, and also following Antoine Wautier's second comment, we now provide a comparison to one temperature gradient time series in Fig 5.

Regarding the impact of the temperature on the elastic modulus: Our parameterization explicitly contains the elastic constants of ice as parameters. For the comparison to FEM we therefore used only one set of ice parameters (elastic constants of ice from Petrenko at -16°). Now, any known temperature dependence of the elastic moduli (derived elsewhere) could be used in the parameterization by inserting the temperature dependent functions for the ice moduli into the parameterization. This has been pointed out in the discussion.

4. It should be noted early on in the present study how you are defining porosity and the reference frame you are using.

We agree. We added the definition of porosity at the beginning of Sec. 2.1 and also specified the coordinate system. The z-axis is always aligned with the vertical.

5. In equation 8, it is assumed that the dependence on the eigenvalues for the ice volume fraction are of power-law type. Why? One can ad hoc assume the relation follows a power law, but a more detailed explanation should be provided.

This question could be maybe better answered by the authors who derived that model. From our understanding there is no deeper justification for this functional form, besides simplicity and consistency with the functional dependence on density. On theoretical grounds (cf. e.g. Cowin "The relationship between the elasticity tensor and the fabric tensor", Mechanics of Materials, 1976) symmetry arguments imply more general relations that involve the invariants of the fabric tensor, which in turn lead to more complicated functions of the eigenvalues. In (Zysset Cunier 1995) a Taylor expansion was then involved which ultimately led to the form used by (Srivastava 2016) and Eq 8. A comment in this direction is added in the text.

6. In figure 3(b), all components of the elastic modulus from the FEM simulations are compared to the C33 components of the power law model presented in Eq. 4. It may be beneficial to clarify why the density power law agrees more with C44 components (rather than the C33 for which other comparisons are made) obtained from the FEM simulations.

This is due to a typo on our side: In subfigure (d) (you may have referred to in the comment) the x-axis must read C_ij and not C_33. The fact that C44 is best matched by a density-based parameterization with two adjustable parameters for all components is simply due to the fact that the C44 values lie somewhat "in the middle", i.e. in between the diagonal terms C11/C33 (typically higher) and the off diagonal terms C12/C13 (typically smaller). We improved the explanation of the figure in the text and in the caption.

7. It would be nice to see a plot of the upper HS bound with the polarization or fabric tensor (as Srivastava et al. (2016) notes, the choice in which one does not effect the representation of geometric anisotropy).

We are not sure if we understand the question correctly. The upper HS bounds require the fourth-order Eshelby tensor which is later converted to a 6x6 matrix in Voigt notation. In contrast, the Zysset-Curnier formulation underlying Srivastava 2016 involves the orientation/ fabric tensor represented by a 3x3 matrix. Mixing these approaches is therefore not immediately possible. Presently, the upper HS bound is plotted "as is" (Eq. 9) in Fig 4 against the FEM data or in Fig 1 against ice volume fraction. Both are based on the same representation of anisotropy (through the Eshelby tensor from Appendix B). For the comparison to Srivastava we wanted to make sure that the same anisotropy parameter alpha is used (ratio of the correlation lengths) relying on Srivastava et al. 2016, stating that for their formulation of elasticity tensor, the choice of method in the characterization of geometrical anisotropy of a microstructure through mean intercept length (MIL), star volume distribution (SVD) does not affect the results. Which is confirmed by other studies that have shown similar results for other formulations of the fabric tensor (e.g. https://doi.org/10.1016/j.medengphy.2011.09.006). A comparison of different representations of the fabric tensor/anisotropy in the Srivastava formulation is out of the scope of the present paper, since it would require to implement yet another fully different image analysis algorithm to derive an alternative formulation of the fabric.

8. Please provide a more explicit relation for effective elastic moduli (presented as Eq. (2) in the original text) to Young's modulus, bulk modulus, and Poisson's ratio. To that end, it would be useful to to see these relations plotted as a function of mass fraction for all discussed models.

Also by request of other comments, explicit relations have been included now. For isotropic materials we agree that stating the elastic tensor in terms of e.g. Young's and Bulk modulus is more intuitive and widely used. In the TI case, though, it requires to introduce longitudinal and transversal Young moduli (and Poisson ratios), which are already less intuitive. And a comparison to an isotropic Young's modulus (e.g. derived elsewhere) cannot be simply made by averaging the corresponding longitudinal and the transversal moduli. For wave propagation applications, on the other hand, the C_{ij} are generally easier to interpret. For the overall representation of the results and the plots we therefore prefer to stick to the tensor elements C_{ij} instead.

9. On line 148, it would be useful to see a figure of the geometric result of $\alpha > 1$, $\alpha < 1$, and $\alpha = 1$, to illustrate the result of prolate inclusions, oblate inclusions, and isotropic bubble distributions. Better yet if a movie of this transition could be provided across a range of porosities.

In general we agree, but we prefer to do this later in the results. We included a slice view of the microstructure from our extreme anisotropy cases in the results in Fig. 6. A movie across the range of porosities cannot be compiled from the data.

Minor Comments

I3: "... geometrical) that give rise to macroscopically anisotropic elastic behavior." to "... geometrical), which can give rise to elastic behavior due to macroscopic anisotropy."

Corrected.

I16: "...the elastic modulus is the probably..." to "the elastic modulus can be used to represent the mechanical properties of snow, firn, or bubbly ice, and so knowledge of the effective elasticity tensor plays a crucial role in…"

Corrected.

I22: "In particular,...anisotropy..." to "In Schlegel et al. (2019), the role of elastic anisotropy was emphasized. Specifically, the retrieval of elasticity…"

Corrected.

I24-26: "...anisotropic, on one hand...orientation" to "an anisotropic with respect to ice matrix geometry (e.g. ...) and crystallographic orientation [there needs to be a citation here]."

Citations added.

I29: "Recent work wave propagation..." to "Recent work by Hellmann et al. (2021) on measuring wave propagation in glacier ice suggests that at low porosity [give value] the effective elastic... is influenced by geometric effects (such as porosity)." Reduce the intensives (e.g. "already"). They weaken your argument.

We added the porosity specification (<1%) and changed the formulation.

I37: "Using the Finite-Element (FE) methods..." to "Using Finite-Element (FE) methods via volume averaging, a solution for static linear elasticity yields the material effective elastic properties."

Changed accordingly.

I48-49: "the HS bounds incorporate the non-linear interplay between structural anisotropy and density." HS bounds incorporate the non-linear relation between density and bulk and shear stress, but you need to be more careful defining how anisotropy is represented in the limit of these bounds in the introductory remarks (or refer to the description in Sec.2.4).

We added "via the Eshelby tensor" for specification here, since a forward reference in the introduction to a future section is not appropriate.

I51-56: This entire paragraph is one sentence. Although this is fine, consider breaking it up to make your points more clear to readers.

Sentence has been split up.

I62: "by comparing it with the above mentioned shortcomings of previous work" to "... with previous work in which these parameters are not captured," or something similar. Refrain from adding subjective words.

Sentence changed as suggested.

I69: "... is defined by Hook's law" to "is defined by Hook's law, using Hill's lemma,..." Add the reference frame.

Changed accordingly.

Eq.1. Consider adding the region over which the continuum is occupied. Also, consider adding a remark on the use of the notation in eq.1. in connecting volume averaged strain energy of a heterogeneous material at micro length-scales to that of a macroscopically heterogeneous material under uniform strain.

The description has been adapted, this also complies with the introduction of "porosity" and ice volume fraction at an early stage of the description.

I86: Specify how you are defining ice volume fraction (see Major comments for a related remark).

See comment above.

I86: "...parameterization often..." to "parameterizations use a power law…"

Changed.

Eq.6. Consider showing the limit explicitly.

This limiting behavior is documented in textbooks, so we do not see the necessity for repeating the derivation here. We added a reference.

I128-129: It can be left to the reader to refer back to the cited text. However, to avoid ambiguity, consider providing a brief description on how these parameters were obtained.

Explanation on how these parameters were obtained has been added.

I132: "Hashin-Shtrikman..." to "when using Hashin-Shtrikman (HS) bounds, the effective elastic properties of porous materials can be derived based on volume fraction and microstructural anisotropy (incorporated through n-point correlation functions)."

Rewritten as suggested.

I130: Consider re-phrasing the subsection header to specify the case of geometric(?) anisotropy, since the distinction is clearly made on I140-141.

The bounds themselves do not include any specification on whether the origin of anisotropy is geometric or crystalline. The HS bounds are also used for polycrystalline materials. In the subsequent text the specification needs to be made though

Eq.10. Consider expanding on eq. 10 with 8. Also, make reference to accompanying definitions given in the appendix.

These two equations are not immediately related. Eq. 8 was been derived from symmetry principles following Cowin (reference given in your comment #5) while 10 follows the standard HS variational approach. A sentence has been added to state this.

I155: "...the formulations including anisotropy, three different anisotropy ratios..." to "including geometric anisotropy, three different anisotropy ratios (alpha = 0.1, 1, and 1.6) were evaluated…"

Changed accordingly

I161: "...tend to an isotropic state" to "the geometric fabric must tend to an isotropic state"

This sentence has changed due to a comment from the other reviewers. This was badly formulated, it is not correct to say that the geometric fabric/anisotropy must tend to an isotropic state, it is the "the elastic anisotropy that must vanish for $\phi\to1$.

I161: "the U bound" to "the upper bound (C^U)."
Changed.

In Fig.1. Gpa should be GPa
Corrected.

Eq.11. Define the normalized HS bound before introducing the transformation.
We now define normalized HS bound before Eq. 11, as suggested.

Eq.13. This assumes no mass exchange between the two phases, correct? If so, please note this. I.e. that you assume no sublimation (a process that occurs in glaciological contexts and is a deviation in model applicability to natural environments).

Throughout this work we consider the elastic properties of snow/firn at a given instant in time where the microstructure (pointwise information of ice and air) is given by Eq 13. We therefore do not consider any underlying time dependent process here. This is also important to keep in mind for another comment below: The goal of this work is not explaining why different microstructures exist at different temperatures or temperature gradients. The latter question is certainly highly influenced by sublimation etc as explained elsewhere in snow metamorphism related papers. We clarify this now from the very beginning on in Sec. 2.

I213: Consider including the definition of Q(\alpha) used here, for completeness.

The definition of Q has been included in the Appendix.

I216-217: What temperature is this valid for? It would be useful to run FE simulations over a range of shear and bulk moduli that correspond to a range of temperatures. (See major comments)

See answer to the main comment above. The corresponding temperature for the ice moduli is stated now.

I221-222: Consider adding a table summarizing the model equations, names, unknown parameters, and porosity range over which they are valid, and their main assumptions.

Such a table is already included, namely Table 2 which shows up only in the results though, when the parameter estimates have actually been done. We added a reference here. The (density) range of validity for the models have been specified in Sec. 2.2.

In Fig 3. Refer back to table 1. It is unclear in Figures 2 or 3 what the legend means. These codes should also be explained in the body of the text in Sec. 4.1.

We now explain the color code used in Fig. 2 and Fig. 3 explicitly in the body of the text in Sec. 4.1 and 4.2 and refer to Table 1.

In Fig 3.b. it is clear this is the only value for the elastic modulus for which the empirical parameters (a_33 and b_33) are known. However, either consider omitting this plot (3.b), since it adds confusion as to which models provide the best agreement to simulations of C, or obtain empirical parameters for a_ij and b_ij from other experimental datasets. Also, please explain a possible reason that C_33 from the density power law agrees more with C_44 from the simulations.

As already mentioned in the reply to the major comment #6, there was a typo in the axis label. With the correct axis label, the subfigure should not add confusion anymore.

I247: Possibly refer to eq.(5) here.

Done.

I248-249: "...and with the literature P-wave velocity of ice...". Please include the conditions under which this was measured.

Specifications have been added.

In Fig 4. "Gpa" to "Gpa"

Corrected.

I258: "...for reasons discussed in Fig. 1" to "..., as mentioned in the caption of figure 1,"

Reformulated.

I259: "...gives the right prediction" to "...parameterization provides an elastic modulus that agrees well with simulated values, taken from images of EastGRIP samples that were close to the surface. At these depths, anisotropy values are low (\alpha < 1), and are consistent with..."
The entire paragraph changed due to the modification of the figure.

I261:"... for deeper snow" to "at greater depths, the geometric anisotropy increases."

Corrected.

I262:"...demonstrate a good performance..." I am not sure what you mean by this, or at the least it is slightly vague. Please explain what you mean by good performance here.

This sentence dropped out.

I265: "... reasons discussed in Fig. 1" Omit and consider stating clearly the reason for greater error values for the ZC model with greater densities. Also, reasons cannot be discussed in figures (it is up to you to discuss what the figures mean). Please re-phrase.

The comparison to the ZC model is now removed from this section/figure.

In Figure 5: "...Bottom: Error plot which is given by the difference between the simulated elastic modulus..." to "Bottom: Error in FEM and PAR parameterized elastic modulus calculated from the difference between simulated elastic modulus…"

The figure changed in the revision.

I274: "...with zero relative error by" to "with zero relative error for isotropic…"

Formulation changed.

I278: "...we show the geometrical Thomsen parameter \epsilon_geom (see Eq.3) in Fig. 7." Referencing this figure does not assess the geometrical vs crystallographic anisotropy in your calculations, or at least it is not clear what you mean by this. Consider "... we plot the geometrical Thomsen parameter, obtained from Eq. 3, against the porosity, the output of which is given in Figure 7."

We agree this is not clear. We reformulated the description.

I295-296: No parentheses are needed around the authors names.

Corrected.

I299: "In contrast... are explicit formulas." to "In contrast, the limiting behavior of the Hashin-Shtrikman bounds can provide an explicit formula for effective moduli."

Corrected accordingly.

In Fig. 7. Make sure the symbols you are using are consistent. For example, varepsilon is used to label the vertical axis, but epsilon is used in the text.

Notation corrected.

I303: "...collapsing onto" to "...collapse onto"

Correted.

I304: "...as a function of normalized HS upper..." to "... as a function of the normalized HS upper bound…"

Corrected.

I305: "...parameterization Srivastava et al..." to "parameterization used in…"

Corrected.

I317: "...yields a eigenvalue zero..." to "yields zero eigenvalue…"

Corrected.

I323: "...where MIL resulted circle with no signatures of anisotropy." It is not clear what you mean by this. Please explain the outputs from the cited text more clearly. For example, "...observed by Klatt et al. (2017), in which a Boolean model of MIL with arbitrary rank fabric tensors, produced figures of circles when evaluated on Reuleaux triangles. Moreover, the MIL analysis used in that model was insufficient in detecting interfacial anisotropy."

This was badly written. Reformulated.

I327: "...overall our parameterization shows" to "overall, the parameterization used in the present work (C_{ij}^{PW}, given by Eq. (11)-(12)), had excellent agreement (R^2 = 0.99) when fit to all components…"

Wording corrected.

I335: "... evident for temperature gradient time series (TGM2 and MMTO17) from Fig. 6 (a)...". Which one is MMTO17? This timeseries is not listed in the referenced figure (or, at least, it is not clear which time series you mean). Also, without a description of the temperature timeseries in the body of this work, the dependence of the elastic modulus used in the PW model (and even \alpha) on temperature is not entirely clear, other than vertically oriented structures being favored at high temperature gradients.

This was a typo, we meant TS-TGM17. As detailed above for the other two comments on this, we elaborated now on the role of temperature in the parameterization.

I371-375: "in principle... However, typos..." These sentences are not needed. Unless you plan to also compute crystallographic fabric at low porosity (such as in future work), with remedied typos from the referenced text, it detracts from the overall discussion.

We agree. This has been reformulated.