Swiss Federal Research Institute WSL
Eidg. Forschungsanstalt WSL
Institut fédéral de recherches WSL
Istituto federale di ricerca WSL

The Cryosphere
Editorial Office

Davos, November 27, 2023

WSL Institute for Snow and Avalanche Research SLF
Group Snow Physics
Kavitha Sundu
Phone +41-81-417 02 84
sundu.kavitha@slf.ch

Cover letter

Dear Dr. Kaitlin Keegan,

Enclosed is our revised manuscript, "*A microstructure-based parameterization of the effective, anisotropic elasticity tensor of snow, firn, and bubbly ice,"* with the point-by-point replies to all four reviewers and a complete track-change document for an unambiguous overview of all differences between the original and revised version.
In the response to the reviewers, our proposed modifications and their place in the track change version are displayed in green. Note also that we have corrected some extra typos. They are not discussed in the reviews but appear in the track-change version.
We highlight that based on the reviewers comments we removed Section 4.4 "Elasticity depth profile", an also added of two Appendices. Following a structure change Figures 5 and 6 have been swapped in the manuscript.
Finally, we unfortunately could not perform the simulations on the last four samples as the co-author required to assist with simulations is unavailable for health reasons. Therefore, we use 391 instead of 395 samples (3 of the missing samples correspond to ARC-EGRIP, and one corresponds to a lone specimen from the metamorphism box time series.). The impact on the results is negligible, as these are only a few samples (and share similarities with the other Arc-EGRIP samples and other TS-DH1 samples in the data, meaning they only add a little information). Also, as we mentioned in the rebuttal, we did not consider some load states in the optimization due to filename inconsistencies, which are now corrected. Therefore, the fit parameters slightly changed without any implication on the results.

Finally, the numerous reviewer comments were essential, and the associated improvements in the figures and amendments to method descriptions and the discussion helped to clarify the open points.

Thank you for the editorial support.
Kind regards,
Kavitha Sundu (on behalf of the authors)

Dear Pascal,

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)

Review of « A microstructure-based parameterization of the effective, anisotropic elasticity tensor of snow, firn, and bubbly ice » by Sundu et al. in The Cryosphere

Summary :

The effective stiffness tensor of snow, firn, and bubbly ice is controlled by the density, morphology, and elastic properties of the ice matrix. This control was previously studied and parameterized independently for different ranges of density: for snow (rho in [30, 500] kg/m3), firn (rho in [500, ~800] kg/m3), and bubbly ice (rho in [~800, 915] kg/m3). Here, the authors developed a new parameterization of this control that is valid on the full density range. They use the formal anisotropic Hashin-Shtrikman upper bound as a predictor of the stiffness tensor in empirical fit based on 395 finite-element simulations on tomographic images.

Main comments :

This article constitutes a valuable contribution to The Cryosphere with a sound methodology and interesting results. The paper is fairly well written to follow the work (typos to be corrected through proof review). Even if the new parameterization does not substantially outperform existing parameterizations in their porosity range of validity, it is valid on the full density range from snow to bubbly ice and does not exhibit artificial and arbitrary transition zones with density. In particular, the existing parameterizations for snow predict an effective stiffness larger than the one of ice if applied on high-density samples. Besides, the anisotropy is directly captured by the Eshelby tensor, which does not require additional fitting when using the 2-parameters fit (same beta and eta for all components) for the whole tensor. This new parameterization comes at the cost of a more complex implementation, particularly of the upper bound C_U using the Eshelby tensor derived from the correlation length of the structure. However, its computation expense remains far lower than a full finite-element simulation on the snow microstructure.

However, I have certain comments that would need to be addressed before publication :

1. One goal of the presented work is to provide a new parameterization of the elastic tensor valid from snow to ice. To use it, one must compute the density, the correlation lengths of the given sample microstructure, the associated 4th-order Eshelby tensor, the corresponding Hashin-Shtrikman upper bound, and eventually, the empirical fit, and to juggle between Voigt and tensorial notations. The authors should provide the functions (e.g., Python or Matlab style) so the community can easily re-use this fit. Otherwise, I fear that simple density parameterization will remain the norm. The shown material must be enough to re-implement the fit, but it is prone to errors and headaches.

We fully agree and this was actually planned initially. Upon acceptance, we will provide (on envidat.ch) the python scripts together with the data and the necessary functions to compute the parametrized elasticity tensor as a function of density, anisotropy, shear and bulk modulus of ice for a straightforward adoption of the results in the community.

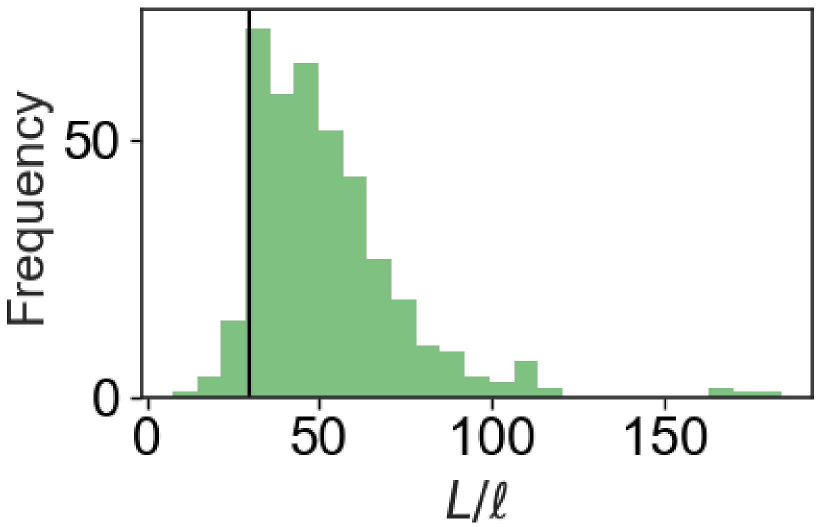
We now provide the code and data. Refer to the code and data availability declaration. Not visible in the track change version.
P27 L533 in the track change version.

2. The computation of the effective isotropic transverse elastic tensor from finite-element simulations is not described in enough detail.
    1. First, what sample size (mm) and boundary conditions are used? Indeed, the convergence of the apparent sample properties into effective material properties with the simulation volume depends on the applied boundary conditions and sample density. In particular, the low-density samples of Alp-DIV likely deviate from the proposed parameterization because of the too-small sample size (Fig. 2). With this information, the robustness of the simulations can be evaluated.

The FEM simulations were performed by employing periodic boundary conditions as originally implemented in the used FE code, which we mentioned now in extended description of the FE method.

"*Finite Elements Method (FEM) simulations were performed using the code from Garboczi (1998) on all the CT images to determine the elasticity tensor of the snow microstructure by employing periodic boundary conditions.***"**
P13 L264 in the track change version.

The sample size vary for each sample and were taken as is from the original data sets. This information is now included in the paper. To assess the impact on the RVE we show the convergence criterion from Wautier et al 2015 in the figure below:



According to their estimates on correlation functions, convergence to an RVE can be assumed when the ratio of linear sample size L and the correlation length l exceeds 30. From distribution of our data shown above (figure not included in the paper) we infer that 92% of all the samples fulfill the requirement of RVE (>30 L/l), while 8 % are below the limit. However, only one sample from Alp-DIV in the low volume fraction range falls into this group, and there is no systematic sub-set of particular samples in these 8%. This information on RVE is now explicitly included in the methods:

"*To assess whether we fulfill the representative volume element (RVE) criterion, we employed the estimate of Wautier et al., (2015), which is based on correlation functions. RVE convergence is deemed to be satisfied when the ratio of linear sample size L (given in Table 1) and the correlation length l ($\sqrt[3]{l_{x}\,l_{y}\,l_{z}}$) exceeds 30. From this, we deduce that 92% of our samples fulfill this requirement, while 8% of the samples do not fulfill it. These latter samples have ice volume fractions ranging from 0.11 to 0.66.***"**
P13 L279 in the track change version.

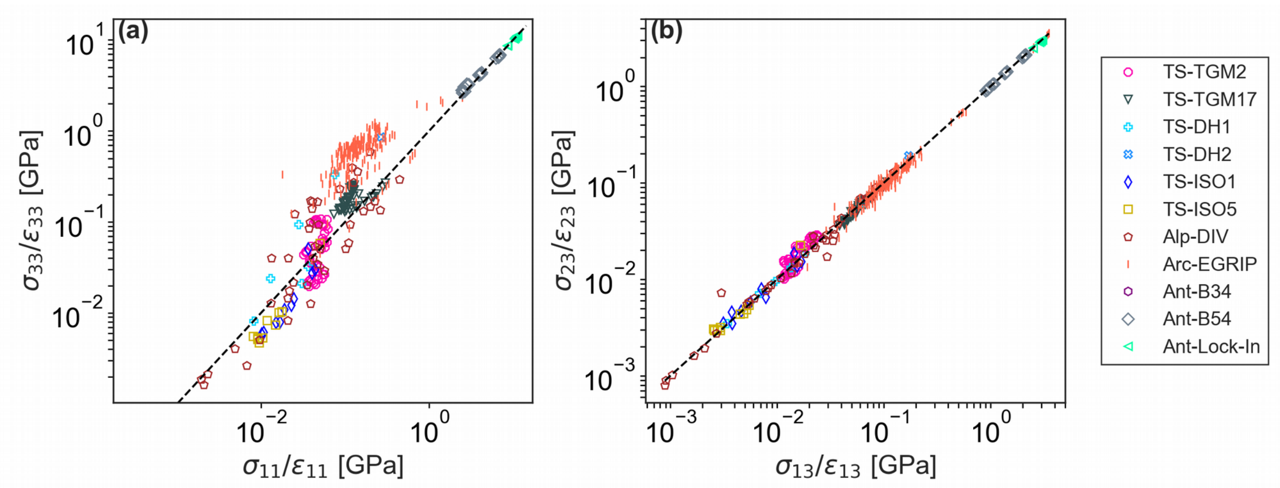
2. The isotropic transverse tensor is estimated from 5 load states (Sec. 3.4) by finding the five independent components of C that minimize the L2-norm of sigma-C:epsilon. The five load states are not described. It is unclear whether a bad choice of these load states may favor better approximations of certain components when approximating the full tensor to the isotropic transverse one. What is the difference between getting the full tensor (21 components) based on 6 unit load cases and taking the theoretically non-zero components under the assumptions of transverse isotropy (e.g., Wautier et al., 2015)? In addition, the assumption of transverse isotropy makes sense for snow (deformation by gravity generally aligned with temperature gradient), but is it relevant for bubbly ice on ice sheets that may also flow in a certain horizontal direction?

We now included the definition of the five load states that were used for the optimization. In principle we are just using the Cartesian basis vectors in 6 dimensional stress space (e_1… e_6). We were combining, though, e_4 and e_5 to a single load state (e_4+e_5) leading to the same equations. We acknowledge and explicitly mention now that the choice of load states naturally implies a different weight for different elasticity components in the least squares optimization (As an example: C33 is only involved in the e_3 load state). The description of the optimization is also extended.

“ We performed FE simulations for five load states derived from Cartesian basis vectors in the six-dimensional deformation space. The deformation ε of the five load states are taken from the set $\{\varepsilon_0 e_{11};\ \varepsilon_0 e_{22};\ \varepsilon_0 e_{33};\ \varepsilon_0(e_{13}+e_{23})\ \varepsilon_0 e_{12};\ \}$, with $\varepsilon_0=0.01$ and with e_{11} to e_{12} being unit vectors in the deformation space. Note that we combined load states 13 and 23 for the fourth deformation state.”

Next, for each sample the five independent components of the elasticity tensor C (see Eq. 2) are estimated by minimizing the L2-norm of $\boldsymbol{\sigma}\text{-}C{:}\boldsymbol{\varepsilon}=0$, where $\boldsymbol{\sigma}$ and $\boldsymbol{\varepsilon}$ are the stress and deformation states from the simulations. The specific choice of load states naturally implies different weights for the elasticity components during the least square optimization, as, for instance, the C^{FEM}_{33} is only involved in the e_{33} load state.”
P13 L267 in the track change version.

To further support the assumption on TI symmetry underlying the work, the following figure (a) compares the ratios sigma11/epsilon11 with sigma33/epsilon33, obtained from the (e_1) and (e_3) load states respectively (without prior assumptions on symmetry). When comparing potential symmetry breaking between x and y (e.g. by plotting sigma13/epsilon13 and sigma23/epsilon23 (see Figure (b)) no significant difference can be observed.



This further indicates that TI symmetry is a reasonable assumption for the present data. We agree though that for bubbly ice, in principle more complex symmetry cases are hypothetically possible as you mentioned. This was however not observed here.

“This optimization strategy ensures the resulting elasticity tensor is transverse isotropic and incompressible. It also ensures that the components are consistently estimated through the several load states in which they play a role in.”

P13 L276 in the track change version.

3. The different models were fitted on the simulation data using a log-transformation of the elastic tensor component with a least squares regression. The density distribution of the samples is not uniform in the full density range. In particular, around 80% (?) of the samples exhibit a density between 250 and 500 kg/m3 (Tab. 1, Fig. 2). Besides, some data are highly correlated because they belong to the same time-series. The collected is already huge and the largest so far to my knowledge; however, could you discuss this point? Can we rely on this parameterization for any collected snow data, or is the fit impacted by the sampling? Moreover, the improvements of the new parameterization do not show up in the regression coefficient (Tab. 2) or the scatter around the predictor (phi or C^U in Fig. 2). Sampling.

   The percentage of samples with density between 250 and 500 kg/m3 is 68%. In this intermediate density range, the most significant influence of structural anisotropy is expected, in contrast to the low and high-density range, at which the structural anisotropy is be less dominant. It is known, that the evolution of structural anisotropy is mainly driven by temperature gradient metamorphism (Leinss et al 2020) and that some lab samples included in our data here have served as an independent validation for the rather strong temperature gradient metamorphism observed in the Arctic tundra (Leinss et al 2020). Therefore, we expect that this parameterization is sufficiently generic to capture typical, anisotropic structures in snow.
   The rather moderate change in the regression coefficient indeed reflects that anisotropy has a only a sub-dominant influence on elasticity, while density is still the main parameter. However, capturing these sub-dominant influences may be very important though for advanced microstructure characterization by alternative means. This is better explained now in the introduction.

   "The proposed empirical parameterization offers a crucial advantage by being applicable across the range of natural ice volume fractions, enabling accurate predictions of the effective elastic modulus (see Fig. 3). This broad range of applicability is supported by the fact that some of the temperature-gradient experiment samples used in this study have been independently compared with natural Arctic snow in terms of geometrical anisotropy (Leins et al., 2020). Furthermore, these anisotropic samples fall into the Intermediate density range (250 kg m^{-3}-500 kg m^{-3}), where geometrical anisotropy exerts a substantial influence, in contrast with the lesser dominance of structural anisotropy at low and high densities. Therefore, we expect that our parameterization is sufficiently generic to capture typical anisotropic structures in snow. Furthermore, the samples used to derive the parametrization are diverse regarding their conditions of formation. Consequently, we expect this parameterization to yield reasonably accurate predictions of elastic properties for the whole range of natural porous snow, firn, and ice formations."
   P22 L375 in the track change version.

   "Leins et al. (2016) *show that an electromagnetic inversion model could be exploited to retrieve the geometrical anisotropy of snow, and this despite a sub-dominant impact of the*

geometrical anisotropy on the effective permittivity tensor. A better understanding of the link between geometrical and elastic anisotropy would thus enable the use of a similar technique to retrieve the geometrical anisotropy of snow from seismic surveys."
P2 L50 in the track change version.

"The relatively moderate change in the regression coefficient of our C_{33}^{PW} in comparison to previous parameterization C_{33}^{G} and C_{33}^{PW} (see Fig. 3) reflects that anisotropy only has a sub-dominant influence on elasticity, while density remains the main parameter. However, capturing these sub-dominant influences may be very important for advanced microstructure characterization by alternative means, such as capturing macroscopic physical properties remotely (Leins et al., 2016)."
P23 L394 in the track change version.

4. The authors state that « the limit of $\varphi \rightarrow 1$ the microstructure must tend to an isotropic state » (l.160-161). I disagree with the statement or I have not understood it. Bubbles in ice may be very flat and tend to, for instance, horizontal micro-cracks (porosity tends to zero, but anisotropy can remain constantly high). This point motivated the choice of the HS bound as a predictor but there is no prior reason for that. It appears that the collected samples (Fig. 6) of high density (phi > 0.7) are also characterized, but the sampling may be too limited to draw definite conclusions on the structure anisotropy at high density. Moreover, Fig. 6 is based on this specific feature of the HS bounds. It shows that the anisotropy of the bubbles does not affect the anisotropy of the elastic tensor. I am not convinced this is sound. Please clarify.

This was not well formulated: We meant the effective *elastic* behavior of the microstructure must tend to an isotropic state for $\varphi \rightarrow 1$, i.e. the elastic anisotropy must obviously vanish for zero-porosity, polycrystalline ice. It is true that the geometrical anisotropy may remain non-zero for $\varphi \rightarrow 1$. This is somewhat visible in our bubbly ice samples where a slight geometrical anisotropy remains, even for the highest densities. But in this density range our data may not be exhaustive enough to see if the slight vertical anisotropy is generalizable to other sites. Due to the processes in polar firn, the remaining geometrical anisotropy for $\varphi \rightarrow 1$ must be weak and much lower as for intermediate densities. The effective elastic behavior involves both, density and geometrical anisotropy, and the combined effect must converge to an elastically isotropic elastic state for vanishing volume fraction of the inclusions. From our understanding this limiting behavior is strict, and automatically accounted for through the HS bound. We reformulated the sentence to make this clearer.

"In addition, ZC shows an influence of geometrical anisotropy that increases monotonically with ice volume fraction, which is also nonphysical since in the limit of $\varphi \rightarrow 1$ the elastic anisotropy behavior of the microstructure must tend to an isotropic state."
P8 L203 in the track change version.

5. The elastic tensor depends on density as a power law with an exponent in [3, 5]. An error of 5% on density may cause an error of 15% to 25% on the elasticity components. Measuring

density, even with tomography, is subjected to errors in this order of magnitude (e.g., Proksch et al., 2015; Hagenmuller et al., 2016). The « relative » error due to anisotropy should be discussed with respect to the errors on density and not shown as the main source of uncertainty.

This is a very good suggestion, we included the comparison in the discussion. To this end we considered the extreme anisotropy and density cases in our data and computed the propagated uncertainty on the elastic constants ΔC from our parametrization, for both, namely i) having a density error of 5% or ii) neglecting anisotropy. The results are:

Case 1: Highest anisotropy in our data
α = 1.87, φ = 0.39
ΔC_α = 88.7%
ΔC_φ = 18.18%

Case 2: Lowest anisotropy in our data
α = 0.45, φ = 0.66
ΔC_α = 58.45%
ΔC_φ = 28.55%

These values confirm that neglecting anisotropy may lead to considerably larger errors than a typical error in the density measurement.

We have added a paragraph in Section"4.4 Relative influence of geometrical anisotropy and density".

"Figure 5 shows that the structural anisotropy α is an important component of the parametrization proposed in this work. However, as it is not straight-forward to measure the structural anisotropy and as elastic is highly sensitive to density, one may wonder how the errors induced by neglecting anisotropy compare to typical errors due to uncertainties on the density measurement. To answer this question, we compared the impact of neglecting anisotropy (that is to say assuming α=1) to that of a typical 5\% uncertainty when measuring density using µCT (Proksch et al., 2015, Hagenmuller et al., 2016). Concretely, we applied our parametrization of the C_{33} component to three cases: case (a) corresponds to the ideal case of taking into account geometrical anisotropy (α!=1) and assuming no uncertainty on density, case (b) corresponds to a case where geometrical anisotropy is accounted for (α!=1) but with a 5\% uncertainty on density, and case (c) corresponds to the case where geometrical anisotropy is neglected (α=1) but without density uncertainty. These three cases are applied to the Arc-EGRIP samples (0.45<α<1.87 and 0.24<φ<0.66), which underwent TGM under natural conditions, and to the TS-TGM17 samples (0.9<α<1.15 and 0.30<φ<0.32), which in contrast underwent TGM in controlled conditions. They are visible in Fig.6 alongside estimation of the C_{33} component directly derived from the FE simulations, which serves as a reference. Neglecting anisotropy (case 3) leads to average errors of 39.8% and 21.7% for the Arc-EGRIP and TS-TGM17 samples, respectively. A 5% percent error on density, while taking into account anisotropy (case 2), yields average errors of 23% and 11.96% for the Arc-EGRIP and TS-TGM17 samples, respectively. This is to be compared with average errors of 14.56% and 11.96% when anisotropy is considered and when there is no error on density."

P20 L350 in the track change version.

Minor comments :

l11 : « the crystallographic anisotropy » -> « to the maximal theoretical crystallographic anisotropy .» Indeed, your estimation of crystallographic anisotropy is very rough.

Changed accordingly.

"*Finally, we used the Thomsen parameter to compare the geometrical anisotropy to the maximal theoretical crystallographic anisotropy in bubbly ice.***"**

P1 L11 in the track change version.

l22 : « the last example … » -> « Schlegel et al. have stressed ».

Changed accordingly.

"*The work of Schlegel et al. (2019) emphasized the role of elastic anisotropy.***"**

P2 L23 in the track change version.

l24-26 : « ice matrix geometry … crystallographic orientation ». There are references for geometrical anisotropy but no for crystalline anisotropy.

Agreed. References included.

"*Snow and firn are however known to be anisotropic due to both the ice matrix geometry (e.g., Loewe et al., 2013, Calonne et al. 2015, Leinss et al. 2016, Moser et al. 2020, Montagnat et al. 2020), and the crystallographic orientations of the ice crystals (e.g., Diez et al. 2015, Petrenko 1999).***"**

P2 L25 in the track change version.

l26-28: « fabric is low/high ». What does it mean? Anisotropy is high /low?

Changed to "strong" (this is how it is commonly referred to) and "anisotropy" added in brackets.

"While the geometrical fabric in firn is strong (leading to a strong geometrical elastic anisotropy) near the surface due to temperature gradient metamorphism (Montagnat et al. 2020) and decays with depth (Fujita et al 2014), the crystallographic fabric is weak near the surface (thus yielding a weak crystallographic elastic anisotropy) but increases with depth under densification and flow \ (e.g., Montagnat et al 2014, Saruya et al 2022)."

P2 L31 in the track change version.

l29: « recent work wave propagations » -> ? « Hellmann et al. (2021) measured wave propagation on glacier ice and suggested … »

Reformulated

“Recent work by \cite{hellmann_2020} on measuring wave propagation in glacier ice suggests that even at low porosity (< 1%), the effective elastic (crystallographic) anisotropy of polycrystalline ice is influenced by the geometrical effects of the porosity. ”

P2 L35 in the track change version.

Figure 1: The range of density on which the existing parameterizations are supposed to work (according to their respective authors) is never shown in Figure 1 or explained in the text (e.g. Section 2.2). Add this info.

This information has been added in the Sec 2.2.

P5 L140, P6 L143, P7 L169, Caption of Fig1 P9 in the track change version.

l34: « elasticity ». Delete word.

Deleted

P2 L49 in the track change version.

l34: « for retrieving sub-surface density and anisotropy ». In general, it is unclear to me if the parameterization is bijective, i.e., is there one unique anisotropy tensor and density for a given elasticity tensor?

We guess no. But in geophysics (like the work cited here) retrievals are rarely based on exact inversions, but rather on suitably constrained optimizations of (strictly) ill-defined problems by exploiting the properties of the forward model. A potential elastic inversion is somewhat similar to the electromagnetic inversion put forward in (Leinss et al 2016) where also a very small impact of the structural anisotropy on the effective permittivity tensor (with known anisotropic forward model) could be exploited to retrieve the geometrical anisotropy of snow. This is elaborated a bit further in the extended version of the introduction.

“Leins et al. (2016) *show that an electromagnetic inversion model could be exploited to retrieve the geometrical anisotropy of snow, and this despite a sub-dominant impact of the geometrical anisotropy on the effective permittivity tensor. A better understanding of the link between geometrical and elastic anisotropy would thus enable the use of a similar technique to retrieve the geometrical anisotropy of snow from seismic surveys.”*

P2 L50 in the track change version.

l57: « Section 2 gives a theoretical overview of the elasticity tensor » -> « Section 2 gives the background of the elasticity theory ».

Changed accordingly.

“Section 2 gives the background on the elasticity theory, examines the limitation of existing parameterizations, and motivates the methodological idea that underlies the proposed parameterization for the elasticity tensor.”

P3 L78 in the track change version.

l.66: « Where the » -> « whose »

Reformulated.

P3 L88 in the track change version.

Eqn. 1: Give the assumption underlying this equation (Hill's lemma).

Hill's lemma stated now.

"The effective (fourth order) elasticity tensor C of a statistically homogenous two-phase composite material is defined by Hooke's law, using Hill's lemma, of elasticity as"

P4 L91 in the track change version.

l72-73: Explain what is « transversely isotropic » and that z is vertical (?).

The coordinate system is now properly defined at the beginning of the section.

"We consider snow to be a transversely isotropic (TI) material, where the axis of transverse symmetry is chosen as the vertical z- axis perpendicular to the horizontal isotropic xy- plane."

P4 L98 in the track change version.

Eqn. 2: Report also sigma and epsilon (as in Eqn. 1), so that the Voigt notation is explicit (there may be some variations with some 1/2, 2 coeff.).

Voigt notation is explicitly defined now.

"that relates the volume averaged second-order stress $<\sigma>$ *and strain tensors* $<\varepsilon>$*, given in Voigt notation as* $[\sigma_{11},\sigma_{22},\sigma_{33},\sigma_{13},\sigma_{23},\sigma_{12}]^T$ *and* $[\varepsilon_{11}, \varepsilon_{22}, \varepsilon_{33}, 0.5\varepsilon_{13}, 0.5\varepsilon_{23}, 0.5\varepsilon_{12}]^T$*, respectively."*

P4 L94 in the track change version.

l77: « common relations ». It would be convenient to have these relations in the appendix. Indeed, the paper change from one notation to another (C_ij, Lamé, bulk modulus, etc.) and it is sometimes difficult to follow.

Relations are stated in the appendix now.

P26 L536 in the track change version.

l80-83: Only one Thomsen parameter is used after. Only present this one and explain in a few words what it represents.

We followed the suggestion.

P4 L107 in the track change version.

l94 : « 33 component » -> « the component C_33 »

Reformulated.

P5 L128 in the track change version.

l122: « elasiticity » -> « elasticity ». Check the orthograph in the whole paper with dedicated software to avoid typos.

Changed and spell check carried out.

P6 L160 in the track change version.

l132: « HS bounds predict the effective elastic properties ». No, they are bounds (with one equal to zero).

From our perspective, bounds still *predict* the effective elastic properties. The HS bounds are even realized for specific microstructures. This implies, that the prediction can be even exact. So in some cases a *prediction* via bounds is very good, in other cases less good. We therefore keep our formulation.

l160: « influence of anisotropy increases monotonically ». Clarify if its relative anisotropy.

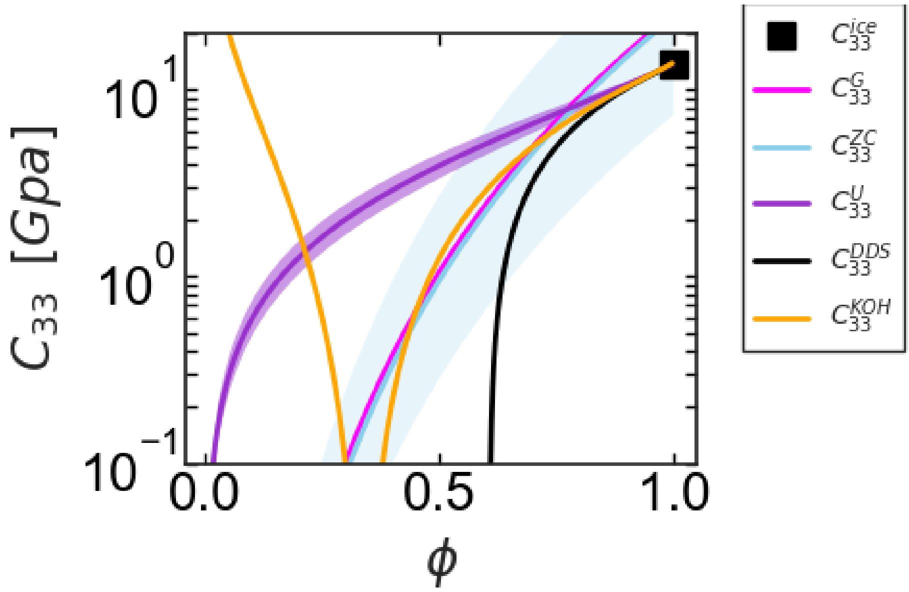
Formulation changed.

"In addition, ZC shows an influence of geometrical anisotropy that increases monotonically with ice volume fraction, which is also nonphysical since in the limit of φ → 1 the elastic anisotropy behavior of the microstructure must tend to an isotropic state."

P8 L203 in the track change version.

Fig. 1: show in log scale to be consistent with the rest of the paper. Show the expected range of validity of the models.

We tested this as shown in the Fig below. However, we believe that at this point of schematic introduction, the log scale is more confusing to the readers. Hence, we prefer the non-log scale.



« Illustration » -> « Evolution »

Changed.

P9 in the track change version.

Table 1 : « Isothtermal » -> « Isothermal »

Corrected.

P11 in the track change version.

Section 3.4. Give reference to the choice of the ice properties.

Reference included.

P13 L267 in the track change version.

Fig. 2: Are the first row and last column really necessary? You could gain space to make the subfigures larger.

We think they are illustrative (also in view of another reviewer's comment): The three columns progressively shows how the data collapse is attained by 1/ including the correct symmetry/anisotropy 2/ rescaling by the underlying ice parameters (in particular through the first row). This now better described. But we also increased the space for the subfigures by relocating the legend.

"*Figure 2 shows an overview of all results by plotting the simulated elasticity components C^{FEM}_{ij} (different rows) against ice volume fraction (column 1), the HS upper bound (column 2) and the normalized representation from Eq.11. In the first top row (Fig.2(a)-(c)), all elasticity components from all the samples are represented with different colors depending on the component of the elasticity tensor. In contrast, in the rest of the rows, only one component is represented at a time and the colors and symbols highlight the different samples, as defined in Table 1. The figure shows that the scatter of the simulated elasticity tensor components (C^{FEM}_{ij}) is maximal when plotted as a function of the ice volume fraction φ (left column), and that this scatter is reduced when plotted as a function of the HS upper bound C^{U}_{ij} instead (middle column).*

Next, we use the improved correlation between C^{FEM}_{ij} and C^{U}_{ij} to derive the parameterization for each component according to Eq.~\eqref{eq:11}, shown as the black curves (right column). **"**

P13 L291 in the track change version.

Fig. 3: comparing C_FEM to C_G_33 (power law) is somehow unfair (scatter due to the fact that, e.g. C12 != C11). Indeed refits of the power law on each component show very little scatter (Tab. 2).

There was a typo in the x-label of the Figure 3d subplot. We now corrected from C_G_33 to C_G_ij. Indeed, also for (e) or (f) we have C12!=C11. But here the data collapse is attained even without fitting components individually. This illustrates that the underlying symmetry supplied by the fabric/Eshelby tensor is the relevant ingredient (see comment above)

P16 in the track change version.

Fig. 6b: I am not sure this figure makes any sense. Anisotropy at high density affects elasticity anisotropy, but it appears that porous ice is not anisotropic (due to ice physics). See main comments. Can you make the same figure but with the FEM as the ground truth?

This is also related to your comment further above: At high density, the elastic anisotropy due to geometry must vanish (as shown by the figure) and the difference between the anisotropic and the isotropic formulation tends to zero. The geometrical anisotropy in porous ice (at least for the samples analysed here) remains very weak, but still visible. This is exactly how you expected it to be in your comment above. The figure cannot be done using FEM data since the data does not fill the plane continuously.

Sect. 4.6: This is not clear to me why epsilon_cryst should decay with increasing porosity. For sure, it cannot go above the value for a single mono-crystal. Moreover, you do not need this decay to draw your conclusion (geometric anisotropy is dominant for most of the densities). Simplify.

Assume a volume-filling monocrystal with zero porosity, which is represented by epsilon_cryst as the the maximally, possible anisotropy. Now add a mechanically isotropic inclusion phase (air). As a result, the elastic anisotropy must decay. We still believe that this schematic is illustrative since it re-emphasizes the necessity of revisiting the dominant anisotropy for very high density. This is maybe less of a concern for snow, but this is very important for fabric analysis of ice. We changed the text to make this clearer.

“To understand this phenomenon, one can assume a volume-filling monocrystal, with a Thomsen parameter $\epsilon_{\mathrm{cryst}}$ that corresponds the maximum possible crystallographic anisotropy. Now, if this volume is gradually filled with an isotropic inclusion of air, the anisotropic behavior of the hollowed mono-crystal decays. This behavior is shown as a schematic line in the inset of Fig.~\ref{fig:07} and highlights the importance of consideration of both kinds of anisotropies for very high density.”

P26 L495 in the track change version.

l287 : significantly

Changed accordingly.

P22 L386 in the track change version.

l340-342: You discuss here possible improvements. Does it really make sense with the given current performance and the uncertainty on the measurements? Delete paragraph?

We agree, paragraph deleted.

P24 L453 in the track change version.

l388: « The new parameterization constitutes a significant simplification ». I would not say it is simple but rather, « it is a crucial tool »

Reformulated.

P27 L524 in the track change version.

Reference :

- Hagenmuller, P., Matzl, M., Chambon, G., Schneebeli, M., 2016. Sensitivity of snow density and specific surface area measured by microtomography to different image processing algorithms. The Cryosphere 10, 1039–1054. https://doi.org/10.5194/tc-10-1039-2016

- Proksch, M., Löwe, H., Schneebeli, M., 2015. Density, specific surface area and correlation length of snow measured by high-resolution penetrometry. Journal of Geophysical Research: Earth Surface 120, 346–362. https://doi.org/10.1002/2014JF003266

References added.

Not visible in the track change version.

Pascal Hagenmuller

Dear Antoine,

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 this paper, the authors propose to use finite element simulations conducted on X-ray tomography images (395 images in total) to compute the homogenized elastic behavior of snow, firn and bubbly ice. The resulting behavior is modeled as transversely isotropic, which corresponds to 5 independent material parameters. Homogenizing the elastic properties of snow from X-ray tomography images is not new and several authors (cited in the paper) have already proposed such a procedure in the last decade. And some of them have already used a transversely isotropic model for snow. The contribution from the authors to the state of the art is to propose a fit over the whole range of porosity with the combination of a power law and the theoretical Hashin-Shtrikman bound (equations (9), (11) and (12)) to respect the fact the ice properties are recovered for a solid fraction of 1. The fit explicitly accounts for both density and geometrical anisotropy (estimated as the ratio of autocorrelation lengths in the vertical and horizontal directions). They show that their fit enable to achieve a higher precision than previous fit proposed in the literature.

Then, the authors discuss the relative contribution of geometrical anisotropy for different porosity values. The authors also assess the relative contribution of geometrical and crystallographic anisotropy on the elastic properties of snow, firn and bubbly ice. They show that the influence of anisotropy decreases with the decrease in porosity. They also show that geometrical anisotropy is dominant over crystallographic anisotropy up to a volume fraction of 0.7.

Even if the contribution to the state of the art is a little bit incremental on some aspects, I would suggest publication, provided the authors clarifies the following points. On the form, the paper is globally well written but the main story line is sometimes a little bit difficult to follow.

1. In Fig. 6. the authors explicitly show the relative contribution of geometrical anisotropy for different porosity. The authors could comment a little bit more this central Figure in their paper. For instance, there seems to be a tendency for α to increase with ϕ for low porosity values on the data set considered in Fig. 6.(a). Is there any physical explanation for that? In Fig. 6.(b) the two squares show that the larger over and under estimation zones are indeed not observed in the data set. Could the authors therefore comment on the maximum over and under estimations that one could get by not accounted for α for different snow densities? How does such uncertainties compare with uncertainties related to density

estimations?

The first part is mainly explained in the discussion in Sec. 5.4: Snow typically undergoes temperature gradient metamorphism (TGM) at these low to intermediate densities. TGM on average leaves the density invariant while it increases the anisotropy. This happens in alpine snow but also in polar snow. In polar snow (e.g. EGRIP), the density is typically higher, but also the exposure to temperature gradients is longer, leading to even higher alpha values. On average for the whole data, this yields this apparent increase of alpha with density, which reflects an average behavior of phi(alpha) when combining natural snow and firn samples from different locations. We elaborate on this now when discussing Fig 6.

(We also refer to the track change document (page 24, line 472-484) that shows the changes in the context of the manuscript)

Comparing the uncertainties due to anisotropy with those due to density were also raised by Pascal Hagenmuller and are now included in the discussion.

"Figure 5 shows that the structural anisotropy α is an important component of the parametrization proposed in this work. However, as it is not straight-forward to measure the structural anisotropy and as elastic is highly sensitive to density, one may wonder how the errors induced by neglecting anisotropy compare to typical errors due to uncertainties on the density measurement. To answer this question, we compared the impact of neglecting anisotropy (that is to say assuming α=1) to that of a typical 5\% uncertainty when measuring density using µCT (Proksch et al., 2015, Hagenmuller et al., 2016). Concretely, we applied our parametrization of the C_{33} component to three cases: case (a) corresponds to the ideal case of taking into account geometrical anisotropy (α!=1) and assuming no uncertainty on density, case (b) corresponds to a case where geometrical anisotropy is accounted for (α!=1) but with a 5\% uncertainty on density, and case (c) corresponds to the case where geometrical anisotropy is neglected (α=1) but without density uncertainty. These three cases are applied to the Arc-EGRIP samples ($0.45<\alpha<1.87$ and $0.24<\varphi<0.66$), which underwent TGM under natural conditions, and to the TS-TGM17 samples ($0.9<\alpha<1.15$ and $0.30<\varphi<0.32$), which in contrast underwent TGM in controlled conditions. They are visible in Fig.6 alongside estimation of the C_{33} component directly derived from the FE simulations, which serves as a reference. Neglecting anisotropy (case 3) leads to average errors of 39.8% and 21.7% for the Arc-EGRIP and TS-TGM17 samples, respectively. A 5% percent error on density, while taking into account anisotropy (case 2), yields average errors of 23% and 11.96% for the Arc-EGRIP and TS-TGM17 samples, respectively. This is to be compared with average errors of 14.56% and 11.96% when anisotropy is considered and when there is no error on density."

P20 L350 in the track change version.

2. Time series of snow metamorphism are considered in the data base. In these time series (especially temperature gradient experiments), anisotropy develops. It could be interesting to

show on some specific time series, how the fit propose by the authors enable to accurately capture the anisotropic evolution of the mechanical properties.

We extended Figure 5 to also show one example of a temperature gradient time series. In both parts of Figure 5 (profile and time series) we highlight now the impact of accounting for anisotropy (or not) in the PW model, following your comment 8 below.

P21 Fig6 in the track change version.

3. Section 4.3 may possibly benefit from some clarifications. I understand that Kohnen parametrization is valid at high ice volume fraction only. This could be stated explicitly in section 2.2.2. Then, why not having presented the results in the same form as in Fig. 3 with correlations between the different models and the FEM predictions?

The (density) range of validity for all parameterization is now explicitly included. We chose a different presentation in this figure because we also wanted to highlight the behavior of the parameterizations, not only where FEM results are available, i.e. close to $\phi=1$.

P5 L140, P6 L143, P7 L169, P9 caption of Fig1 in the track change version.

4. I understand that the anisotropy is accounted in the $\boldsymbol{P}^\mathrm{ice}$ tensor in equation (9) which is related to the Eshelby tensor $\boldsymbol{S}$ recalled in Appendix A that depends the ratio α between the vertical and horizontal corelation lengths. Therefore, I do not understand why the tensors $\boldsymbol{M}$ and $\boldsymbol{M}*$ are introduced in section 3.3...

For the Hashin-Shtrikman bounds, the fourth-order Eshelby tensor $\boldsymbol{S}$ is required which is later converted to a 6x6 tensor in Voigt notation. In contrast, the Zysset-Curnier formulation is based on the orientation/fabric tensor $\boldsymbol{M}$ (which is represented by a 3x3 matrix here). Our goal here was to restate the existing results as-is from the original formulation. This is why another tensor was required here.

"Finally, we note that while both the fabric tensor M in the ZC formulation and the Eshelby tensor S^{ice} *in the HS formulation are used to described structural anisotropy, they cannot be used interchangeably, notably as M is second rank tensor whereas* S^{ice} *is a fourth rank tensor."*
P7 L192 in the track change version.

5. More details on the FEM simulations should be given. For instance, what are the boundary conditions?

This was also requested by other reviewers. More technical details on the method, and the boundary condition are now included in the FEM methods section.

“Finite Elements Method (FEM) simulations were performed using the code from Garboczi (1998) on all the CT images to determine the elasticity tensor of the snow microstructure by employing periodic boundary conditions.”
P13 L264 in the track change version.

We also extended the description of the optimization with the involved load states and comment on the fulfilment of RVE (cf. comment and figure included in the reply from Pascal Hagenmuller)

“To assess whether we fulfill the representative volume element (RVE) criterion, we employed the estimate of Wautier et al., (2015), which is based on correlation functions. RVE convergence is deemed to be satisfied when the ratio of linear sample size L (given in Table 1) and the correlation length l ($\sqrt[3]{l_{x}\,l_{y}\,l_{z}}$) exceeds 30. From this, we deduce that 92% of our samples fulfill this requirement, while 8% of the samples do not fulfill it. These last samples have ice volume fractions ranging from 0.11to 0.66.”
P13 L279 in the track change version.

(We also refer to the track change document (page 12, line 272-276) that shows the changes in the context of the manuscript)

6. In Fig. 2, when confronting the predictions of FEM against the U model, it could be nice to display the 1:1 line as done in Fig. 3. For the right graphs, the units (GPa) should be corrected as dimensionless quantities are plotted. Can the authors give more explicitly what is the expression of the fit curve? Does it refer to one of the specific models presented before? Interpreting the data in terms of Young or Bulk moduli could ease the physical interpretation of the parametrization. Instead, the authors simply refer to Torquato (2002a) to find the equivalences with respect to the coefficients C_{ij}.

Regarding the 1:1 line for the upper bound: For none of the sub-figures one expects that the 1:1 line is actually attained, so therefore we do not include the 1:1 here.
Regarding the units: Corrected.
Regarding the fit curve: The fit function is explicitly derived in Eq. 11/12, the description in the text for the figure has been adapted to make this clearer.
Regarding the choice of the base moduli: We added now the conversion from C_ij to Young and Bulk modulus in the appendix. We prefer though to not formulate the entire elasticity tensor and the results in terms of longitudinal and transversal Young moduli and Poisson ratios in the first place.

P15 Right column of Fig 2, New Appendix P27 in the track change version.

7. In table 2, the formal expressions for the different models could be recalled or at least the number of the corresponding equations in the paper.

We agree. The reference to the defining equations has been included.

P17 Table 2 in the track change version.

8. Fig. 5 is not very clear and do not bring much added value compared with Fig. 3... From Fig. 3 the authors have proved that their fit perform better than the other models. Why not using this depth profile to highlight the impact of accounting for the anisotropy or not in the PW model?

We agree. We modified Fig 5 for showing the impact of accounting for anisotropy (or not) in the PW model, once for the depth profile and once for the time series.

P21 Fig 6 the track change version with accompanying text

"Figure 5 shows that the structural anisotropy α is an important component of the parametrization proposed in this work. However, as it is not straight-forward to measure the structural anisotropy and as elastic is highly sensitive to density, one may wonder how the errors induced by neglecting anisotropy compare to typical errors due to uncertainties on the density measurement. To answer this question, we compared the impact of neglecting anisotropy (that is to say assuming α=1) to that of a typical 5\% uncertainty when measuring density using µCT (Proksch et al., 2015, Hagenmuller et al., 2016). Concretely, we applied our parametrization of the C_{33} component to three cases: case (a) corresponds to the ideal case of taking into account geometrical anisotropy (α!=1) and assuming no uncertainty on density, case (b) corresponds to a case where geometrical anisotropy is accounted for (α!=1) but with a 5\% uncertainty on density, and case (c) corresponds to the case where geometrical anisotropy is neglected (α=1) but without density uncertainty. These three cases are applied to the Arc-EGRIP samples (0.45<α<1.87 and 0.24<φ<0.66), which underwent TGM under natural conditions, and to the TS-TGM17 samples (0.9<α<1.15 and 0.30<φ<0.32), which in contrast underwent TGM in controlled conditions. They are visible in Fig.6 alongside estimation of the C_{33} component directly derived from the FE simulations, which serves as a reference. Neglecting anisotropy (case 3) leads to average errors of 39.8% and 21.7% for the Arc-EGRIP and TS-TGM17 samples, respectively. A 5% percent error on density, while taking into account anisotropy (case 2), yields average errors of 23% and 11.96% for the Arc-EGRIP and TS-TGM17 samples, respectively. This is to be compared with average errors of 14.56% and 11.96% when anisotropy is considered and when there is no error on density."

P20 L350 in the track change version.

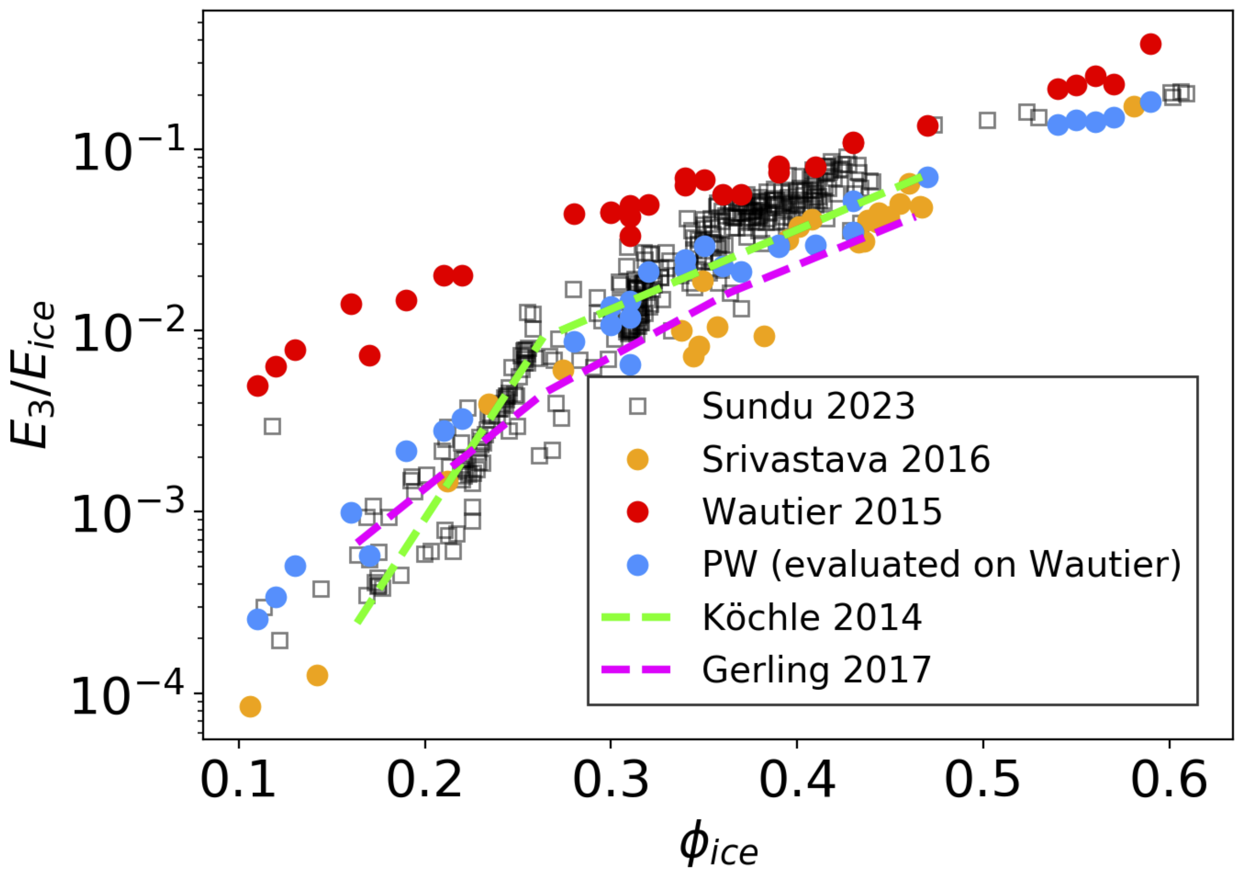
9. The data from Wautier et al. (2015) where snow is modeled with the same transverse isotropic behavior is available in the supporting information. Correlation lengths are also given. Maybe the authors could consider testing their fit on these data points?

We were actually not aware of the amount of details given in the supporting information in Wautier 2015. So we carried out the comparison as requested including other FEM based estimates. In the Figure below we included the FEM results from your paper, the FEM results from Srivastava 2016, our FEM results (Sundu) and the FEM based parameterizations from Köchle and Gerling. And we added the prediction of our PW parameterization when evaluated on the correlation lengths and the density given in the supplement of your paper.

Despite the scatter, this overview rather leads to the conclusion that your FEM results seem to differ from all the others. Therefore the PW evaluation on your correlation lengths and density differs too. We therefore acknowledge that differences between different FEM-based results exist and include this in the discussion.

"To further test the performance of our parameterization, we considered data (ice volume fraction and correlation functions) provided by Wautier et la. (2015). The data display values of α ranging from 0.65 to 1.26, and of φ ranging from 0.10 to 0.59. We applied our parameterization on these data using Eq.12 and compared the obtained results to the elastic stiffness tensor computed from FE simulations of Wautier et la. (2015), Srivastava et al. 2016 and from the present work. We also added the other parameterizations derived from FE simulations (namely Kochle et al 2014, Gerling 2017), with φ ranging from 0.10 to 0.59. We found that PW parameterization applied to the data of Wautier et la. (2015) differs from the Wautier et la. (2015) simulation results. However, despite the scatter, both our FEM simulations and PW parameterization lie within the range of FE results from Srivastava et al. 2016, Kochle et al 2014 and Gerling 2017."

P24 L457 in the track change version.



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 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.

P5 L126, P7 L167 in the track change version.

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.

"The estimation of the geometrical anisotropy usually relies on advanced microstructural characterization, such as the estimation correlation lengths (Krol, 2016). Despite its complexity, this microstructural characterization of snow, firn and ice has become a standard worldwide in the last decade thanks to the development of micro-computed tomography (µCT) in the US (Baker et al 2019), Japan (Ishimoto et al 2018), India (Srivastava et al. 2016), Norway (Salomon et al 2022), Germany (Freitag et al 2004), France (Wautier et al 2015) and Switzerland (Kochle et al 2014). The increasing role played by the microstructural characterization of snow and firn, fostered by µCT, led to the development of alternative retrieval methods, such as the characterization of anisotropy from radar (Leinss et al 2016)."

P2 L39 in the track change version.

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.

P21 Fig 6 in the track change version with accompanying text

“Figure 5 shows that the structural anisotropy α is an important component of the parametrization proposed in this work. However, as it is not straight-forward to measure the structural anisotropy and as elastic is highly sensitive to density, one may wonder how the errors induced by neglecting anisotropy compare to typical errors due to uncertainties on the density measurement. To answer this question, we compared the impact of neglecting anisotropy (that is to say assuming α=1) to that of a typical 5\% uncertainty when measuring density using µCT (Proksch et al., 2015, Hagenmuller et al., 2016). Concretely, we applied our parametrization of the C_{33} component to three cases: case (a) corresponds to the ideal case of taking into account geometrical anisotropy (α!=1) and assuming no uncertainty on density, case (b) corresponds to a case where geometrical anisotropy is accounted for (α!=1) but with a 5\% uncertainty on density, and case (c) corresponds to the case where geometrical anisotropy is neglected (α=1) but without density uncertainty. These three cases are applied to the Arc-EGRIP samples (0.45<α<1.87 and 0.24<φ<0.66), which underwent TGM under natural conditions, and to the TS-TGM17 samples (0.9<α<1.15 and 0.30<φ<0.32), which in contrast underwent TGM in controlled conditions. They are visible in Fig.6 alongside estimation of the C_{33} component directly derived from the FE simulations, which serves as a reference. Neglecting anisotropy (case 3) leads to average errors of 39.8% and 21.7% for the Arc-EGRIP and TS-TGM17 samples, respectively. A 5% percent error on density, while taking into account anisotropy (case 2), yields average errors of 23% and 11.96% for the Arc-EGRIP and TS-TGM17 samples, respectively. This is to be compared with average errors of 14.56% and 11.96% when anisotropy is considered and when there is no error on density.””

P19 L350 in the track change version.

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.

“Also, while the parameterization of the elasticity tensor was derived using the elastic properties of ice at -16 °C, one can directly transpose the parameterization to a different temperature. This is

readily done by taking into account the temperature dependence of the ice elastic properties that appear in the parameterization."
P26 L511 in the track change version.

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.

"Snow is a heterogeneous and porous material with an ice volume fraction φ (defined as the ratio between the volume occupied by the ice phase over that of the sample), […] "
P3 L88

"We consider snow to be a transversely isotropic (TI) material, where the axis of transverse symmetry is chosen as the vertical z- axis perpendicular to the horizontal isotropic xy- plane."

P4 L98 in the track change version.

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.

"This power-law form derives from a polynomial expansion of the elasticity tensor expression in terms of the fabric tensor eigenvalues (Zysset 2003)."

P6 L164 in the track change version.

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.

"Figure. 3(d) shows that the Gerling (2017) density-based parameterization yields the best prediction for the component C_{44} when derived by fitting all components. This is because the C_{44} component values lie in between the diagonal component values C_{11} and C_{33} (typically higher values) and off-diagonal component values C_{12} and C_{13} (typically lower values)."

P24 L440 in the track change version.

(We also refer to the track change document (page 15: Figure 3, page 23: line 430-433) that shows the changes in the context of the manuscript)

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.

"Finally, we note that while both the fabric tensor M in the ZC formulation and the Eshelby tensor S^{ice} in the HS formulation are used to described structural anisotropy, they cannot be used interchangeably, notably as M is second rank tensor whereas S^{ice} is a fourth rank tensor."
P7 L192 in the track change version.

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.

New Appendix P27 in the track change version.

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.

P20 New Fig 5 in the track change version.

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.

“*The microstructure of snow and ice can be characterized by different types of fabrics (crystallographic, geometrical), which gives rise to macroscopically anisotropic elastic behavior.***”**

P1 L3 in the track change version.

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.

“*The elastic modulus can be used to represent the mechanical property of snow, firn or ice and the knowledge of the effective elasticity tensor plays a crucial role in a variety of applications throughout the field of cryospheric sciences.***”**

P1 L17 in the track change version.

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

“*The work of Schlegel et al. (2019) emphasized the role of elastic anisotropy. Specifically, the retrieval of elasticity profiles of snow, firn, and ice through seismic waves usually relies on the assumption of isotropy which constitutes an uncertainty in the inversion method.***”**

P2 L23 in the track change version.

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.

"*Snow and firn are however known to be anisotropic due to both the ice matrix geometry (e.g., Loewe et al., 2013, Calonne et al. 2015, Leinss et al. 2016, Moser et al. 2020, Montagnat et al. 2020), and the crystallographic orientations of the ice crystals (e.g., Diez et al. 2015, Petrenko 1999).***"**

P2 L25 in the track change version.

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.

"Recent work by \cite{hellmann_2020} on measuring wave propagation in glacier ice suggests that even at low porosity (< 1%), the effective elastic (crystallographic) anisotropy of polycrystalline ice is influenced by the geometrical effects of the porosity."

P2 L35 in the track change version.

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.

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

P2 L55 in the track change version.

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.

P3 L68 in the track change version.

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.

"*The present work aims to derive a parameterization of the effective elasticity tensor of snow, firn, and bubbly ice based on volume fraction and structural anisotropy and that can be consistently applied to the entire range of volume fractions. We achieve this by taking the anisotropic HS bounds (without free parameter) as the functional starting point and by using an empirical* *transformation (containing two fit parameters per tensor component). The proposed fitting function matches observed characteristic features, namely the power-law increase of the moduli for high porosities (for snow) and the asymptotic behavior of dilute sphere dispersions (for bubbly ice) in the limit of low porosities.***"**

P3 L71 in the track change version.

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.

"*In Sect. 4 we show performance of new parameterization, by comparing it with previous work in which these parameters are not captured.***"**

P3 L82 in the track change version.

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

Changed accordingly.

"The effective (fourth order) elasticity tensor C of a statistically homogenous two-phase composite material is defined by Hooke's law, using Hill's lemma, of elasticity as"

P4 L91 in the track change version.

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.

"Angular brackets denote volume averaging over a statistically homogeneous region of interest and makes the connection between the volume averaged strain energy of a heterogeneous material at the microscopic scale to that of a macroscopically heterogeneous material under uniform strain. The operator : denotes double contraction (Torquato 1997)."

P4 L95 in the track change version.

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

See comment above.

"Snow is a heterogeneous and porous material with an ice volume fraction φ (defined as the ratio between the volume occupied by the ice phase over that of the sample), […] "

P3 L88

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

Changed.

P5 L118 in the track change version.

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.

P6 L145 in the track change version.

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.
P7 L167 in the track change version.

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.

P7 L173 in the track change version.

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

P7 L172 in the track change version.

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.

"Finally, we note that while both the fabric tensor M in the ZC formulation and the Eshelby tensor S^{ice} *in the HS formulation are used to described structural anisotropy, they cannot be used interchangeably, notably as M is second rank tensor whereas* S^{ice} *is a fourth rank tensor."*
P7 L192 in the track change version.

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

P8 L199 in the track change version.

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 $\phi\to1$.

"In addition, ZC shows an influence of geometrical anisotropy that increases monotonically with ice volume fraction, which is also nonphysical since in the limit of $\varphi \rightarrow 1$ the elastic anisotropy behavior of the microstructure must tend to an isotropic state."
P8 L203 in the track change version.

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

P8 L206 in the track change version.

In Fig.1. Gpa should be GPa
Corrected.

P9 Fig 1 in the track change version.

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

"….this can be achieved by using a transformation in the following form, in which HS bound C^U_{ij} is normalized by C^{ice}_{ij} as.. "

P9 L215 in the track change version.

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.

"Throughout this work, we consider the elastic properties of snow/firn at a given instant in time, where the microstructure gives pointwise information of ice and air. We do not consider any

underlying time-dependent process that would result in the evolution of the microstructure (such as metamorphism)."

P4 L110 in the track change version.

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

The definition of Q has been included in the Appendix.

P12 L261 and new Appendix P27 in the track change version.

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.

"Also, while the parameterization of the elasticity tensor was derived using the elastic properties of ice at -16 °C, one can directly transpose the parameterization to a different temperature. This is readily done by taking into account the temperature dependence of the ice elastic properties that appear in the parameterization."
P26 L511 in the track change version.

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.

P17 Table 2 in the track change version.

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.

P13 L 292, caption of Fig 2 P15 and caption of Fig 3 P16 in the track change version.

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.

"Figure. 3(d) shows that the Gerling (2017) density-based parameterization yields the best prediction for the component C_{44} when derived by fitting all components. This is because the C_{44}

component values lie in between the diagonal component values C_{11} and C_{33} (typically higher values) and off-diagonal component values C_{12} and C_{13} (typically lower values).”

P24 L440 in the track change version.

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

Done.

P17 L315 in the track change version.

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

Specifications have been added.

P17 L317 in the track change version.

In Fig 4. "Gpa" to "Gpa"

Corrected.

P18 Fig 4 in the track change version.

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

Reformulated. The entire paragraph changed due to the modification of the figure.

P18-19 L323-339 in the track change version.

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.

P18-19 L323-339 in the track change version.

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

Corrected.

P18-19 L323-339 in the track change version.

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.

P18-19 L323-339 in the track change version.

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.

P18-19 L323-339 in the track change version.

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.

P18-19 L323-339 in the track change version.

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

Formulation changed.

P19 L347 in the track change version.

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.

P21 L369 in the track change version.

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

Corrected.

P23 L401 in the track change version.

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.

P23 L405 in the track change version.

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.

P22 Fig 7 in the track change version.

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

Correted.

P23 L411 in the track change version.

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

Corrected.

P23 L412 in the track change version.

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

Corrected.

P23 L414 in the track change version.

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

Corrected.

P23 L425 in the track change version.

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."used

This was badly written. Reformulated.

"Similar results were also observed by Klatt et al. (2017), when MIL analysis was performed on a Boolean model with aligned Reuleaux triangles, it resulted in circles."

P24 L431 in the track change version.

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.

P24 L436 in the track change version.

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.

P24 L448 in the track change version.

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.

“To understand this phenomenon, one can assume a volume-filling monocrystal, with a Thomsen parameter $\epsilon_{\mathrm{cryst}}$ that corresponds the maximum possible crystallographic anisotropy. Now, if this volume is gradually filled with an isotropic inclusion of air, the anisotropic behavior of the hollowed mono-crystal decays. This behavior is shown as a schematic line in the inset of Fig.~\ref{fig:07} and highlights the importance of consideration of both kinds of anisotropies for very high density.”

P25 L495 in the track change version.

Dear Reviewer,

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)

This study derives a new parameterization for the effective elasticity tensor that is valid for the full range of volume fractions (i.e. for snow, firn, and bubbly ice). The authors compare this new parameterization to existing parameterizations valid for certain ranges of volume fractions, and identify the potential importance of geometrical anisotropy (in comparison to density and crystallographic anisotropy) in controlling elasticity.

The science and methodology appears sound and the results are interesting. My main comments are about the presentation of the material; in some cases I found the takeaways and the specific novel contributions of the work difficult to pull out of the descriptions. I would also recommend more description of some specific methods (possibly at the cost of some of the background material, which is quite extensive). Besides these recommendations, I would support publication.

Organization and Presentation

In general, I found the balance between the background/"literature review" section of the paper and the methods/results to be a bit off – there was quite a bit of background information, which in some cases was useful (it is helpful to know where the individual models come from and what assumptions they include) but the length and amount of information made it difficult to parse what novel contribution this study was providing. Further, as discussed further below, the background seems to come at the cost of description of methods, which I believe are important to include.

We agree. In the revision, we clarified in the theoretical background what was taken as is from previous work and what is already novel here. In addition, the methods section was extended, also in view of the other reviewer's comments.

P5 L126-129, P6 L164-169, P12 L262-282 in the track change version.

A section in the paper or an appendix that discusses what it takes to apply or use this tensor would be helpful. Similarly, I was left with questions about how generalizable this tensor is – the authors do a good job of explaining its generality in terms of volume fraction, but because the parameterization is based on empirically-found parameters, I believe it would be helpful to know two things:

- What are the conditions that these parameters are found in? What sizes of samples, grain sizes, temperatures, etc.?
- How well will this tensor generalize to different temperatures, grain sizes, etc.?

This would be useful in knowing how to apply this new parameterization.

Regarding the applicability: The tensor and the associated functions will now be published alongside the key data on envidat upon acceptance of the manuscript. This will make the application of the work straightforward.

"To ease the applicability of the present parameterization, we provide the Python scripts with the data and the necessary functions to compute the parametrized elasticity tensor as a function of a sample's density and anisotropy and of the shear and bulk modulus of ice. "

P26 L509 in the track change version.

Regarding generalizability: We expect anisotropy as the main secondary microstructural impact next to density (which clearly dominates the behavior). Since our microstructure data (and anisotropy) is diverse in terms of geographical locations we actually expect this parameterization to be reasonably accurate for for arbitrary natural porous snow/firn/ice.

"The proposed empirical parameterization offers a crucial advantage by being applicable across the range of natural ice volume fractions, enabling accurate predictions of the effective elastic modulus (see Fig. 3). This broad range of applicability is supported by the fact that some of the temperature-gradient experiment samples used in this study have been independently compared with natural Arctic snow in terms of geometrical anisotropy (Leins et al., 2020). Furthermore, these anisotropic samples fall into the intermediate density range (250 kg m^{-3}-500 kg m^{-3}), where geometrical anisotropy exerts a substantial influence, in contrast with the lesser dominance of structural anisotropy at low and high densities. Therefore, we expect that our parameterization is sufficiently generic to capture typical anisotropic structures in snow. Furthermore, the samples used to derive the parametrization are diverse regarding their conditions of formation. Consequently, we expect this parameterization to yield reasonably accurate predictions of elastic properties for the whole range of natural porous snow, firn, and ice formations."
P22 L375 in the track change version.

Regarding the impact of temperature: This was also raised by another reviewer. In a nutshell: Our parameterization explicitly contains the elastic constants of ice as parameters. For the comparison to FEM carried out here 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.

"Also, while the parameterization of the elasticity tensor was derived using the elastic properties of ice at -16 °C, one can directly transpose the parameterization to a different temperature. This is readily done by taking into account the temperature dependence of the ice elastic properties that appear in the parameterization."
P26 L511 in the track change version.

Regarding the impact of grain size: The purely elastic behavior of a porous material cannot depend on grain size explicitly. It only depends on microstructural shape, which included via the Eshelby tensor.

“Finally, as the purely elastic behavior of a porous material does not depend on grain size explicitly but only on its microstructural shape (as seen in the Eshelby tensor described in Appendix B), the proposed parameterization is applicable regardless of the grain size of the considered sample. “

P26 L513 in the track change version.

In summary, the free parameters involved in the parameterization should neither depend on temperature or grain size. We elaborate now in greater extend on these aspects in the discussion.

Methods

I believe the paper would benefit from more detailed outlines of the methods used, particularly with respect to the X-ray tomography (how are the samples found/made? What conditions are they made/found in?) and the FE simulations (what is the resolution of the simulations? What are the assumptions underlying these simulations). Similarly, it would be helpful to have more information about the EGRIP samples – what is the specific variable identified in these samples (crystallographic anisotropy?).

Overall, the methods section has been significantly extended, in particular the description of the FE simulations. For the tomography data (including EGRIP), additional information has been included which is essential for the method (e.g. sample sizes, affecting the RVE). For experimental/field details about the acquisition we refer to the respective papers though.

(We also refer to the track change document (page 11: line 256-273, page 10: Table 1) that shows the changes in the context of the manuscript)

Other Comments

- It would be potentially helpful to clearly define “geometrical anisotropy” up front before using the term. It is an important concept for the paper and for some audiences (including myself, since I do not study porous materials) the term is not obvious
  We agree. This is taken into account in the revised introduction.

  “Snow and firn are however known to be anisotropic due to both the ice matrix geometry (e.g., Loewe et al., 2013, Calonne et al. 2015, Leinss et al. 2016, Moser et al. 2020, Montagnat et al. 2020), and the crystallographic orientations of the ice crystals (e.g., Diez et al. 2015, Petrenko 1999). The geometrical anisotropy arises from the geometrical orientation of the structure that constitutes the ice matrix in snow (for instance if it is predominantly orientated towards the vertical direction), while the crystallographic anisotropy is an inherent characteristic of the ice crystals themselves.”
  P2 L25 in the track change version.

- Figure 1: it would be helpful to include a more descriptive legend to remind the readers which tensor is meant to be valid for which ranges of volume fraction
  We agree. We now mention the range of densities for which the previous parameterizations have been derived.

P5 L140, P6 L143, P7 L169, P9 caption of Fig1 in the track change version.

- Equation 12: what is beta?

The parameter beta was explained before the equation and reflects the power law behavior of the modulus. The parameter beta is a free parameter of the proposed parameterization eq 12 that is later found by optimization. Description adapted.

"The first free parameter β ensures that at low volume fraction the modulus increases as a power law of the ice volume fraction. The second free parameter ξ acts, on the one hand, as a modification of the prefactor in the power law and, on the other hand, as the transition scale that controls the crossover to f(x)~ x."
P10 L224 in the track change version.

- Table 1: it would be helpful to have another column that included the region that each sample was obtained from (or if it was a laboratory sample)

We agree the information on location/lab was added to the table.
P11 Table 1 in the track change version.

- Figure 2: Why are there two legends? What is the difference between a-I and j-r? This information would be helpful in the caption

We agree, this is difficult to read. The figure has been adapted.
P13 L292, P15 Fig2 caption in the track change version.

- Line 255: For some reason, I struggled to parse the sentence "Another view…our data", which seemed important to understand what Figure 5 is showing.

The entire section was modified to comply with the other reviewers' comments. The sentence dropped out.

P18-19 L323-339 in the track change version.