**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 $\alpha$ to increase with $\phi$ 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 $\alpha$ 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.**

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

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.**

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 $\phi=1$.**

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 $\alpha$ 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.**

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. 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)**

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.**

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.**

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.**

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.