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A microstructure-based parameterization of the effective, anisotropic elasticity tensor of snow, firn, and bubbly ice.

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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?

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.

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?

4. I understand that the anisotropy is accounted in the $\boldsymbol{P}^{\text{ice}}$ 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...

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

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}.

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.

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?

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?