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.

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.

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.

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.

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.

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.

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.

- 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.

- 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.

- 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.

- 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.

- 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.