We thank Antoine Wautier for reviewing the manuscript and his constructive remarks. Please find below our point by point response. The comments of the referee are shown in blue and our corresponding responses in black below. Proposed modifications to the manuscript are provided as highlighted text with the lines corresponding the submitted manuscript.

In this paper, the authors present significant contributions to the homogenization of the viscous behavior of snow and firn. They perform finite element simulations of the mechanical behavior of snow and firn in oedometer conditions based on X-ray micro-tomography images. They compare the homogenized viscous behavior to experimental results to back analyze the micro origin of the viscous behavior. In particular, they discuss in details the modeling of the ice matrix as a poly-crystal in case of firn (isotropic behavior) and as a mono-crystal in case of snow (anisotropic behavior). This is done by considering a sensitivity analysis on different ice rheologies.
The paper is well written, easy to follow with a rather clear three dimensional formulation of the viscous behavior of ice and snow. I recommend publication subjected to the minor following comments.

1. In the simulations of the mechanical response of snow and firn samples, did the authors model the transient elasto-visco-plastic regime? How did they isolate the visous response?
In this article we consider a purely visco-plastic material, without an elastic component. This way we directly obtain the steady-state macroscopic viscous response without the need to model a potential transient elastic response.

The fact that we do not need to isolate the viscous response will be specified in the "Finite element solution" Section **L232**:
"As we consider a purely viscous material without elasticity, we do not need to isolate the viscous response from an elastic part (as for instance done in Wautier et al., 2017)."

2. Does the local anisotropy of the ice behavior reflect on the macroscopic behavior, or does the local fluctuations in the directions of ice anisotropy cancel out at the macroscale?
In our simulation framework we have only considered the vertical response, as it is the relevant direction for natural snowpacks and firn columns. We thus did not attempt to estimate the mechanical anisotropy of the material. This limitation will be mentioned in the manuscript **L117**:
"Also, this study is limited to the investigation of vertical compaction in snowpacks/firn columns and does not consider other directions of deformation (e.g. lateral compaction). Therefore, we do not quantify the potential anisotropy of the compactive viscosity."

As hinted by the referee, since we are considering isotropic crystallographic textures in this article, the fluctuations due to the crystallographic orientations should cancel out as there is no preferential direction for the c-axis. The study of a texture-induced anisotropy, and its interaction with the anisotropic microstructure of snow and firn is an interesting path of investigation, with applications in the case where snow and firn are subjected to more than pure oedometric vertical strain. We however feel that this path is beyond the scope of our article as it would first require to determine the relevant ice deformation mechanism in snow before performing a large number of specific numerical simulations. We propose to mention in the text that the study of the interaction between textural and structural anisotropies could be worthy of investigation in the future **L422**:
"A deeper understanding of the influence of the snow texture on its mechanical properties would enable the study of the interaction between structural and textural anisotropies."

3. Complementary to the given reference (Tsuda et al. 2010), I would like to underline a few theoretical references showing that the exponent n of the viscous behavior of a porous material is preserved in the up-scaling process. The authors could also refer to Auriault et al., 1992; Suquet, 1993; Orgéas et al., 2007.
We thank the referee for providing these references. They will be added to the manuscript **L104**.

4. As a curiosity, the authors could include some explanations on how the ice matrix switches from mono to poly-crystals when snow transforms into firn.
We propose to extend the paragraph **L353** to specify how and why the ice rheology transitions from mono to polycristalline:

"There thus would be a transition in the ice rheology from snow, characterized by freely-deforming mono-crystals, to firn, characterized by the interaction of incompatibly orientated crystals (i.e. polycrystalline ice), as the microstructure becomes denser and the crystals start blocking one another."

5. In addition to the given references, the anisotropic formulation of the viscous behavior of the ice behavior (which relies on the form of the fourth order tensor a) could be included explicitly in the text to have a self-supporting paper. In the mono-crystal model, what are the conditions applied on the interfaces between two crystals?
We will move the anisotropic formulation Appendix into the main part of the paper, in Section 3.2.2 "Finite element solution".

For the interface between two crystals we use the "natural" condition that directly follow from the FEM formulation, i.e. continuity of displacement rates. These corresponds to the condition found in a homogeneous material.
We note that it is because of this continuity that we cannot naturally model interface effects, such as grain boundary sliding. The continuity condition between crystals will be mentioned in the text **L253**:
"In the anisotropic simulations, the condition at the interface between monocrystals is characterized by displacement rates continuity."

6. When referring to the segmentation of the ice matrix into mono-crystals (l.215), the authors could refer more explicitly to the images obtained using diffraction X-ray micro-tomography.
We will mention that the crystal segmentation could be experimentally measured using X-ray diffraction tomography. If applicable to sufficiently large samples, this would remove the uncertainty associated with the geometrical segmentation used in our study.

We will add **L221** that the texture of our snow sample could potentially be experimentally measured though XDT:
"We note that while such technique was not available for our study, the crystallographic orientation in snow could also be experimental determined through X-ray diffraction tomography (Roscoat et al., 2011, Reischig et al., 2013, Granger et al., 2021)."

We will also mention it when discussing the need for texture measurments in Section 4.4 **L420**:
"Concurrent measurements of the texture are then unavoidable, either through snow thin-sections (Riche et al., 2013, Montagnat et al., 2020) or through X-ray diffraction tomography (Roscoat et al., 2011, Reischig et al., 2013, Granger et al., 2021)."

7. Maybe the authors could consider moving Section 3.3 ”testing the finite element setup” in an appendix.

If the editor and referee agree, we would prefer to keep Section 3.3 in the main part of the manuscript, as it also illustrates some of the points presented in the Theoritical background Section (the preservation of the non-linear exponent and the influence of an anisotropic ice material). Also, as the appendix of the previous manuscript was moved to the main part of the article, keeping Section 3.3 in the main part of the manuscript would allow a single streamline article.