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Numerical homogenization of the viscoplastic behavior of snow based on X-ray tomography images.

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Response to Andrew Hansen's comments (RC3)

We gratefully acknowledge the reviewer, Andrew Hansen, for his comments to clarify some points and improve the quality of our manuscript.

All the reviewers' comments have been taken into account to provide a revised version of our manuscript. The major modifications of the manuscript consist in:

- an enriched introduction containing an improved state of the art and a clearer statement of the objectives and the scope of our manuscript.
- a more detailed assessment of both the elastic and viscous material parameters on the homogenized viscous behavior of snow. A slight change in the postprocessing procedure has been made by introducing a characteristic time.

1. *I was misled by the title. When I first read it, I thought of a finite (large) strain viscoplastic constitutive model for snow where the important deformation mechanisms include such things as bond fracture, intergranular glide, neck growth, etc. That is clearly not the topic of the article. Perhaps the title could be modified to reflect this point by simply adding words such as small strains or some other appropriate descriptor. I'll note that this point is a recurring theme in my review.*

Reply: We agree that the domain of application of our formulation of the snow viscoplastic behavior was not obvious. This point has been clarified in the introduction of our revised manuscript. Indeed, as our model is formulated in terms of strain rate, there is no need to specify whether it is valid in small strain or finite strain. However, in order to avoid any influence resulting from the change in density of the samples during the numerical simulations, the simulated time was limited to 40, 000 s corresponding to a volumetric strain of roughly 1.2 % under the considered strain rate. Then, the macroscopic law is generalized to finite deformation problems thanks to the use of a collection of 3D snow images exhibiting different microstructures and densities. At the macroscopic scale, the change in density resulting from large deformations is accounted for by the change in the f and c values. It is clear that it supposes that the density changes are sufficient, at the first order, to capture the influence of the complex modifications of the snow microstructure on its macroscopic behavior. This approximation has been extensively used in the past to described the complete densification (large deformation) of granular materials (metallic powders), in the porosity range [0 , 0.4] [1]. In the future, simulations on snow samples with very different microstructures but with similar density would help better evaluate this assumption.

2. *Page 1 lines 15-18 Echoing the point above, lines 15-18 contain a sentence: In practice, a good knowledge of the macroscopic mechanical behavior of snow in a wide range of applied loads, strain rates, and temperatures is of particular interest with respect to avalanche risk forecasting or to determine the forces on avalanche defense structures.*
This sentence led to further confusion, for me, in that I again immediately thought of finite deformation (large strain) problems where constitutive modeling is an enormous and important challenge.
In reality, this paper is concerned with very small strains, and perhaps small strain rates, where bond fracture and intergranular glide do not occur. More specifically, the original ice microstructure must be intact. I believe this constraint is pretty severe for low density snow where bond fracture will surely initiate at very low strains.
I think the reader would be well served if the authors clarify this point by adding a paragraph in the introduction describing the range of applicable strains where they believe the theory is valid. Quantifying such strain levels may be hard to do so an alternate approach would be to simply state that the microstructure is assumed unaltered by bond fracture and further describe the types of problems one might address with the theory. For example, density consolidation under the external body force of gravity seems to me to be the topic of most relevance.

Reply: We agree with the reviewer's comment and we added some precisions in the revised version of the manuscript to stress the fact that the strain levels considered in the homogenization procedure should remain small in order to avoid any bond fracture and intergranular glide and to keep the volumetric strain small.
As pointed out by the reviewer, the main application of the work carried out lies in a better description of the densification of the snowpack under its own weight.

3. *Page 3 line 3 It would be helpful, at least to me, to provide a brief description of isodissipation curves and how they will be utilized. Are these curves analogous to a yield surface in rate-independent plasticity? This is an area of the manuscript where I was a bit lost and I suspect others will suffer a similar plight.*

Reply: Yes, isodissipation curves can be seen as an equivalent of a yield surface. Indeed, if the yield function is replaced by the mechanical dissipation $\mathcal{P}$, an isodissipation curve corresponding to a mechanical dissipation $\mathcal{P}^{\circ}$ is described by the implicit equation $\mathcal{P}(\Sigma) - \mathcal{P}^{\circ} = 0$ in the stress space. Details have been added in the introduction of the revised version of the manuscript.

4. *Page 3 line 30. It would be useful to point out here that* $\mathbf{E}$ *is the small strain tensor at the macroscale. Again, I was initially confused as in finite strain theory,* $\mathbf{E}$ *often refers to the Lagrangian finite strain measure. Moreover, finite strain problems are commonplace in snow mechanics.*

Reply: The reviewer is right and the precision was added in the revised manuscript. We also underlined the fact that uppercase letters systematically refer to macroscale quantities whereas lowercase letters are used for their microscale counterparts.

5. *Page 3 lines 16-27. The discussion of the various types of boundary conditions is excellent and I completely agree with the authors. I suspect a formal study of the differences between periodic boundary conditions and kinematically uniform boundary conditions would show very little difference in a homogenization process. The volume averaging process is very forgiving.*

Reply: Thank you for pointing this out! Whatever the boundary conditions used, they introduce errors in the volume averaging process. But they eventually vanish in the volume averaging process as the resulting errors are proportional to the boundary surface.

6. *Page 7 line 3. The focus of this paper is on assumed isotropic behavior (I believe that is a good thing!). Indeed, on line 1 of page 8 this is explicitly stated. Hence, on page 7, line 3 it might be useful to state that, for an isotropic material, the stiffness tensor can be obtained with a single simulation of an REV. Six simulations are sufficient to fully characterize orthotropic behavior.*

Reply: We totally agree with this comment. Considering anisotropic behavior will increase significantly the number of invariants required to formulate the macroscopic behavior. However, before page 8, this assumption is not needed in the theoretical development presented.

7. *Page 7 lines 12-13. I was completely lost by the phrase unit sphere in the second order tensor space.*

Reply: In the vector space composed of the second order tensors, it is possible to define a norm (for instance $||\mathbf{E}|| = \sqrt{\mathbf{E} : \mathbf{E}}$). Because of the homogeneity property written in equation (11), the mechanical response $\mathbf{\Sigma}(\mathbf{E})$ can be deduced from the mechanical response associated with $\mathbf{E}/||\mathbf{E}||$, which is a second order tensor belonging to the unit sphere. Details have been added in the revised version of the manuscript.

8. *Page 9, lines 16-17. I don't believe this sentence is properly expressed. While there is certainly an elastic strain and a viscous strain, there is only one stress. That is, we do not have an elastic stress and a viscous stress. The equation for stress is correct in these lines, the wording is not.*

Reply: We agree with the reviewer and we modified the sentence accordingly.

9. *Page 15 line 8. The ability of snow to dissipate some energy . Perhaps it would be better to say: The ability of snow to dissipate significant energy . A minor point perhaps but a reflection of my background.*

Reply: We agree with the reviewer and we modified the sentence accordingly.

10. *Page 18 lines 11-14. I agree with the components identified in the strain tensor, all zero except E_{zz}. However, I am confused by the stress tensor. Specifically, the authors show $\Sigma_{\theta\theta} = \Sigma_{rr}$. While this may be correct, it is not readily apparent to me. Perhaps the authors could show this point, at least in their review response if not the manuscript.*

Reply: This is due to the mechanical equilibrium $\mathrm{div}(\boldsymbol{\Sigma}) = 0$. The explanation has been added in the revised version of the manuscript.

11. *Page 20 lines 1-4. Similar comments to the previous point. In the case of stress, it is unclear why $\Sigma_{\theta\theta} = \Sigma_{rr}$. For example in a thick walled pressure vessel $\Sigma_{\theta\theta}$ is vastly different than Σ_{rr}. Moreover, why is $\mathrm{E}_{\theta\theta} = \mathrm{E}_{rr}$? My instincts tell me that this problem is over-constrained and that these two conditions involving stress and strain cannot be met simultaneously. Again, perhaps I am wrong but additional clarity is needed.*

Reply: For the stress components, the answer is the same as above while the condition for the strain comes from the constitutive equation (32). The explanation has been added in the revised version of the manuscript.

12. *Page 23 lines 16-18. This comment is important and follows back to my earliest comments about limitations of the theory to very small strains. The authors use the phrase few percents of deformation. I'm guessing even strains of this low magnitude may be too high to preclude bond breaking, particularly at low densities.*

Reply: The homogenization approach proposed in our paper relies upon an incremental approach. Indeed, given a snow sample, the incremental visco-plastic behavior is computed thanks to numerical simulations using very small strain increments (indeed 4.10^{-3}) while the finite strain problem can be addressed by changing the reference tomography image according to the density evolution. This comes back to our response to your first comment about the title of our manuscript. However, as far as the mechanical of Bartelt and von Moos is concerned, we agree with the reviewer: the microstructure evolutions might exceed the implicit changes in the microstructure taken into account in our model.

13. *Page 24 lines 8-11 I respectfully disagree with the authors as to the importance of this study being extended to anisotropic snow types. In my view, there are so many more important avenues of discovery. I'll offer up a few suggestions of personal interest:*

Reply: We agree with the suggestions made by the reviewers and we added some of them as outlooks in our conclusion.

The minor editorial suggestions were taken into account in the revised version of the manuscript.

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References

[1] L. Sanchez, E Ouedraogo, L. Federzoni, and P. Stutz. New viscoplastic model to simulate hot isostatic pressing. *Powder metallurgy*, 45:329–334, 2002.