Review of:

Numerical homogenization of the viscoplastic behavior of snow

based on X-ray tomography images

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The authors of this paper develop a nonlinear viscoplastic constitutive law for snow that is the result of viscoplastic behavior of the ice constituent. The process involves what I'll refer to as a finite element micromechanics model of a representative elementary volume (REV) whose microstructure is determined from snow samples using X-ray tomography. The micro stress-strain fields are then appropriately homogenized to aid in the development of a macroscale viscoplastic stress-strain relation. The resulting macroscale constitutive law appears relative straightforward to implement and contains a modest number of empirical parameters—as does any nonlinear constitutive model of interest.

I believe the authors have addressed a very challenging problem and produced a constitutive model for snow that is of interest. The paper is well written and I was able to follow the majority of the work. Admittedly, there were also areas of the article where I simply did not have the expertise to follow.

I believe the article is worthy of publication although I'll provide several comments and/or suggestions for revisions in the interest of improving the manuscript and perhaps expanding the readership. I'll also provide some minor editorial comments.

Comments/Suggestions:

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.

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 <u>very</u> 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.

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.

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

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.

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.

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

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.

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

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.

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 $E_{\theta\theta} = E_{rr}$? 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.

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.

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:

a. Further validation of the theory with experimental data would certainly be a worthy endeavor. Once that is complete, applying the theory to a 2-D boundary value problem of natural consolidation (at the macroscale) would be of genuine interest. Even a field study comparison is possible.

b. Of course, consolidation (densification) in snow will also occur due to sintering, particularly in equitemperature environments. It would be of interest to explore the significance of these two phenomena, particularly as a function of snow depth and temperature.

c. Extending this work to finite deformation by allowing for microstructural evolution of key state variables such as grain size, intergranular glide paths, neck growth, etc. In this vein, I would point the authors to some of the pioneering work of R. L. Brown on viscoplastic behavior of snow. One particular reference of interest:

Brown, R.L., "A volumetric constitutive law for snow based on a neck growth model" **Journal of Applied Physics**, Volume 51, 1980

Abstract
A volumetric constitutive law for snow is developed by considering the deformation of the ice grains and grain bonds which form the porous material. The equations of equilibrium and mass conservation are applied to both the grain body and neck regions to calculate the rate of change of grain geometry and neck geometry. The matrix material, ice, is assumed to be a nonlinear viscoplastic material. Comparison with data shows excellent agreement for a wide range of initial densities and for large volumetric deformations. Calculations are also made to evaluate grain and neck deformation during compaction. The model can be applied to porous metals and foams, although the constitutive law for the matrix material would have to be altered.

It seems to me that an application of Brown's neck growth theory, or some derivative of it, would be of keen interest today given the incredible tools of X-ray tomography—something that Brown did not have the luxury of having at the time of publication.

If the authors are determined to extend their theory to anisotropic behavior, I would offer the cautionary note that the degree of complexity probably far exceeds any reward. For instance, if one assumes transverse isotropy (reasonable I think), the limited number of coordinate rotations leads to five stress and strain invariants instead of the normal three. In that spirit, I might suggest the following article is of interest.

A.C. Hansen, D.M. Blackketter, and D.E. Walrath, "An invariant based flow rule for anisotropic plasticity applied to composite materials," **Journal of Applied Mechanics**, Vol. 58, 1991.

The similarities of the referenced work with the present manuscript are striking. Finite element micromechanics of a unit cell are utilized with homogenization to produce a macroscale anisotropic plastic constitutive law for composite materials. The additional invariants play a major role in the flow rule and replace the conventional effective stress measure of classical J_2 plasticity theory.

At the end of the day, though, my opinion is that the degree of anisotropy of snow is mild enough such that an anisotropic development is unwarranted for mechanical problems of this nature.

Minor Editorial Suggestions:

1. Page 2 line 10 The phrase "*finite elements techniques*" might better read as "*finite element techniques*".

2. Page 2 line 13 Should the word "*bound*" be "*bond*"?

3. Page 3 line 1 In the first sentence, I believe the word "*follow*" might better read as "*follows*".

4. Page 3 Line 2 "*The section 3 presents*" might better read as "*Section 3 presents*". There are other instances of this style as well.

5. Page 3 line 16 Should "*in an homogenization*" read as "*in a homogenization*"?

6. Page 3 line 16 The last word "*introduces*" should be singular.

7. Page 5 line 16 I believe the word "*efficiency*" should be "*efficacy*".

8. Page 10 line 7 The phrase "*initial an final*" should read "*initial and final*".

Respectfully submitted.
Andrew C. Hansen