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The Cryosphere
Discussions

Interactive comment on "Microstructure-based modeling of snow mechanics: a discrete element approach" *by* P. Hagenmuller et al.

Anonymous Referee #3

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Review of the paper P. Hagenmuller et al.: "Microstructure-based modeling of snow mechanics: a discrete element approach" The Cryosphere discuss., 9, 1425-1460, 2015

The authors present a discrete element snow model based on 3D tomography images of the snow. The model is capable to deswcribe the rapid and large deformation of the snow dominated by grain rearrangement. The voxels of an upscaled binary X-ray tomography image are replaced with elastic spheres. Snow grains are considered as rigid bodies, and they are represented in the model as clumps of bonded spheres. Bonds between the ice grains are represented by a cohesive contact law. Simulation results of the confined compression of snow samples of different snow types are presented and analysed. Sensitivity analysis of different model parameters is also pre-

sented. Based on the simulations the authors conclude that density alone is a sufficient descriptor of the rapid compression behavior of snow, the microstructure only plays a secondary, practically negligible role.

The authors address an important point in snow mechanics. Namely, the effect of microstructure on the mechanical behavior of snow. X-ray tomography is a modern and popular tool to obtain detailed microstructural information of snow. The authors apply a sophisticated method to convert the tomography data into a discrete element (DE) model. As they correctly point it out, a DE approach is more suitable for the simulation of large deformations than a finite element one. In this respect it is correct to use a DE model to simulate snow compression which is dominated by the large dispalcement and rearrangement of the ice grains in the snow. On the other hand, a microstructure-based DE model consist of a large number of spheres resulting in a long computational time. The authors claim that a DE model is computationally more effective than a finite element simulation. While it is possible that a voxel based finite element model requires more computational time, an adaptive tetrahedral mesh can reduce the computational time considerably. The Young's modulus and tensile strength of a 4 mm x 4 mm x 4 mm snow sample (similar size that is used by the authors in the present paper) can be easily calculated on a common desktop computer with a single processor. I am not sure if this is the case with the simulations presented here. In fact, the authors never mention the computational time and hardware required to run their simulations. The work presented here is an important contribution to out understanding of the mechanical behavior of snow. It is a novel method, and with further development it can be a useful tool to study snow deformation on the microscopic level.

The manuscript is clearly written with high quality figures. It is easy to understand, the relevant works are referenced with one notable exception (see below).

Comments:

1. The most comprehensive discrete element snow model is presented in M.Michael:A

Discrete Approach to Describe the Kinematics between snow and a Tire Tread, PhD thesis, University of Luxembourg, 2014. This must be mentioned and referenced.

2. The bottleneck of microstructure-based snow simulations is the huge computational power required. The authors take steps to reduce the computational time of their simulations by using unrealistic material properties (elastic modulus, density), but they never mention the time and hardware their simulations require. Do they run on a desktop machine with a single processor or they require a supercomputer with 100's of processors?

3. It would be very useful to include the 3D picture of representative samples of the different snow types used in this study (for example s-DF, s-FCDH and S-RG0). Similar to figure 1.

4. In this model the bonds between ice spheres are represented by several sphere-sphere contacts. As with every discretization, there is a minimum number of spheres that can properly represent a continuous contact. Mixed deformation modes like bending (the most common deformation mode of the necks in snow compression) require a fine discretization i.e. a large number of spheres at the contacts. This should be studied by comparison with finite element simulations of bond deformation or at least mentioned in the paper. At a very minimum, the number of spheres at the narrowest necks in the different snow models should be given.

5. The authors write on page 9 line 8:" However, for the parameters considered in this study, the mean effective intergranular friction coefficient remains close to the microscopic friction coefficient defined at the sphere-sphere interaction." It is hard to beleive (and it contradicts to my experience with discrete element modeling) that surfaces consisting of spheres with different size will show similar friction coefficients. What parameters do you refer to?

6. By using a physically unrealistic value for the Young's modulus, E, the contact behavior becomes completely unrealistic. Therefore, a direct comparison with real

snow measurements becomes impossible. This should be emphasised in the paper. It should be also mentioned that changing E will change the point of bond failure.

7. Do you expect your model to describe real snow behavior? Would it be possible to fit the model to real snow measurements? What are the fitting parameters (if there are any) in the model? These points should be discussed in the paper.

8. On page 9 line 22 you write: "the elasticity of the contacts is expected to have little influence on the macroscopic response of the sample." Why do you expect this? In the initial, elastic phase, as well as in the final, dense compaction phase E should have a strong effect. A sensitivity analysis must be done to prove this.

9. What are the typical number of spheres in a model? This should be mentioned.

10. Although it is not realistic, the calculated Young's modulus should be mentioned in paragraph 3.1.1.

11. On page 14 line 16 you write: "It turns out that the value tan(phi) has little effect on the computed mechanical behavior for macroscopic strains in the range [0,0.2]." This contradict to your conclusions in paragraph 3.1.2 where you conclude that in the brittle/frictional phase grain sliding (so the friction between grains) is the dominant mechanism. This requires clarification or further explanation.

12. Looking at figure 7a, it is interesting that tan(phi) has such a high effect in the final, dense compaction phase. This should be mentioned and explained in the text.

13. Figure 7b shows some surprising results. First, the slope of the curve in the initial, elastic phase should not depend on sigma(ice) since practically no bonds are broken yet. Second, in the final, dense compaction phase when most contacts are not cohesive any more, sigma(ice) should not have an effect on the compaction curve. How do you explain these?

14. It is a mistake to discretize the snow geometry using spheres with different diameters in the comparison of the simulated compression behavior of the different snow

types. Especially, since the effective friction between grains can depend on the size of the spheres. 20 micrometers should have been used for all samples. It is in fact a bit suspiciuos that it is exactly those 3 samples that show slightly different behavior that have a sphere size of 20 micrometeres instead of 25.

15. Instead of "equi-temperature methamorphism" write "isothermal methamorphism". page 5 line 13 and page 17 line 4.

Interactive comment on The Cryosphere Discuss., 9, 1425, 2015.