The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2019-80-RC2, 2019


The Cryosphere
Discussions
Open Access



# *Interactive comment on* "Micromechanical modeling of snow failure" *by* Grégoire Bobillier et al.

**Chris Borstad (Referee)**

christopher.borstad@montana.edu

Received and published: 26 June 2019

This manuscript describes a Discrete Element Model (DEM) study of snow deformation and failure. A commercial DEM software package is used to simulate porous and anisotropic weak layers as well as denser and stronger slab layers. The size and properties of the discrete numerical particles was chosen to represent macroscopic layer properties rather than the size and shape of individual snow grains. Load-controlled numerical simulations were performed on both types of layers, using different loading orientations. The nominal stress-strain response of the simulations is discussed, and a weak layer failure envelope is derived. Comparison is made to the results of three experiments from a cold laboratory study, with generally good agreement between the slope of the stress-strain curves and the failure stress.



Printer-friendly version

Discussion paper

I am encouraged by the prospects for using DEM simulations to study aspects of snow mechanics and slab avalanche triggering, and this work is a welcome contribution. Most of my comments relate to issues of clarity and presentation. There is a lot of detail in the manuscript, but I find some aspects that are unclear or unsubstantiated.

Snow is a highly rate-dependent material, although the DEM model does not take into account rate effects such as sintering or viscous deformation. This is acceptable, although I think some further discussion of rate effects is warranted to place the results in context. A target "high" loading rate of 20 kPa/s is chosen for the simulations, and there is mention that verification was made that varying the loading rate did not affect the results (although this is not shown; why/how then was 20 kPa/s chosen?). However, for placing the simulation results in context with experimental results in the literature, it would be worth discussing what types of experimental loading rates are appropriate for comparing with these simulation results.

The simulations are performed with the layer of interest (slab or weak layer) sitting on a rigid base. However, weak layers typically are sandwiched between deformable layers (slab and base are usually stiffer, but still deformable). The stress measurements from the simulations are derived from results at the interface between the layer and the rigid base. How might your results differ if you had a multi-layer scheme with a weak layer sitting on a stiffer, but deformable foundation? It seems to me that this would be more appropriate physically.

P2, L1: describe in a bit more detail what the PST is here, for the benefit of readers that may not be familiar

P2, L18-21: "too high" computational cost is vague here, and I'm skeptical of this statement without further justification. High Performance Computing (HPC) systems allow very large and costly simulations of things like climate, weather, ice sheet dynamics, astrophysics, etc. I'm quite sure that a slope-scale simulation would be feasible on a suitable HPC computing cluster, so you might just need to say that such a simulation is too costly for a stand-alone personal computer (if indeed this is what you mean), or that the commercial code you're using isn't suited (or licensed) for running on a large cluster.

P3, L1-9: The description of the contact model is a bit vague here. A schematic diagram would be helpful to visualize what the model is simulating at the particle scale and what all these mechanical parameters represent physically. It's okay to direct the reader to previous studies that describe such an approach, to a limit, but there's just not enough information here to adequately understand the contact model.

P3 L14: "highly anisotropic" is vague: what is meant by "highly"? I might suggest removing this and just saying "anisotropic"

P3 L15: "can be modified by homothetic transformation" is vague here. Homothetic transformation should be defined. Is this something that is done in the present study, or just something that "can be" done?

P3 L19-20: I missed what the layer densities were that you simulated, and what particle densities were needed to achieve these layer densities. I suggest an additional table of mechanical properties such as this.

P3 L21: "acceptable computation time" doesn't mean much here without a description of what kind of computer you used (later in the text you mention something about a "standard" personal computer, but this is still too vague).

P3 L29: define "clump theory" and "clump density"

P4 L1: it would be worth justifying why you chose unconfined test conditions rather than confined.

P4 L7: I'm not clear how load-controlled tests were performed by "increasing the actuator layer density." I guess by increasing the density you're increasing the weight/gravitational acceleration that the actuator is applying to the layer? A bit more detail is needed here.

P4 L9-10: Related to comment above, here you specify a loading rate. Is this a target loading rate that you achieve by increasing the actuator density?

P4 L19-20: Is “A” the nominal/total area or some measure of the contact area between the particles that represent the layer and the particles that represent the base?

P4 L22-23: At what point along the stress-strain curve is this tangent modulus calculated? This is a common problem with using a tangent modulus to calculate the elastic modulus, because the stress-strain curve is not usually linear all the way to the peak stress. You mention several times that these curves are linear, but I'm skeptical that this is the case. The stress-strain curves in Figures 4, 5, 6, 7, and 10 seem to show some nonlinearity right before the peak (which is to be expected, and is commonly found in experimental data; I recommend zooming in on these peaks in the figures to show any nonlinearity and bond breakage, even if minimal). Thus it really matters where you calculate a tangent modulus, and it is thus common to use something like a secant modulus at the elastic limit (something like 95% of the peak stress) for determining a more robust elastic modulus.

Equation 2: I think the comma between the “i” and “j” subscripts shouldn't be there in C_ij. A comma typically signifies differentiation in standard summation convention (e.g. C_i,j = d/dj C_i), but this is a tensor product of unit normal vectors.

P5 Laboratory experiments: here you chose three experiments from the Capelli study. The Capellis study looked at rate effects, and used three different loading rates. You have chosen results from their intermediate loading rate. Why? How would your results compare to their experimental results at different loading rates? Contact tensors: I'd like to see what the slab and weak layers look like in detail. It's encouraging to see that the weak layer shows transverse isotropy. I think a figure showing the slab and weak layer assemblies in detail would be a nice addition, perhaps even with some unit normal vectors drawn in to show how you're getting these contact tensor results.

P7 L8-9: I think there is some (slight) nonlinearity right before peak, and the step in bond breaking ratio confirms this. Even a small amount of nonlinearity is important, as it indicates some damage accumulation prior to failure (and this is again why it's important where you calculate the tangent modulus...).

P7 L24: What is "Acc" and "Bond_breaking"? This seems to be new terminology. I'm also confused as to why you have focused so much on acceleration here. What exactly is the acceleration showing? You previously discuss that your results are not sensitive to loading rate variation, but wouldn't you expect some change in these acceleration curves with different loading rates? Even if the stress-strain curves don't change much?

P8 L5-6: What do you mean by "critical" bond breaking here? The bond breaking curve in Figure 7b is obstructed by the normal strain curve, but I'm again inclined to think that there seems to be some nonlinearity/bond breaking right before failure. I would zoom in on the peaks of the stress/strain curves, perhaps in a subset of these figures.

P8 L13: unclear how you're defining "loading angles" here. Another example of where some schematic diagrams would be helpful.

P8 L19: The polynomial fit represented by Eq. 9 indeed looks good, but a goodness-of-fit measure like Rˆ2 is not (in general) applicable for a nonlinear model unless a constant mean function can be embedded in the nonlinear model. It's worth checking how the Rˆ2 value is calculated here, since it's not going to be the same definition for a goodness-of-fit as in a linear regression model.

P9 L3: another reference to loading angles here, but the coordinate system for defining the angle hasn't been defined (some additional schematic diagrams will alleviate many of these kinds of comments)

P9 L10: "standard personal computer" should be defined more specifically: what kind of processor, how many cores, what type/amount of memory

P9 L14: The experiments of Sigrist were in bending (to induce tensile failure), and predominantly showed quasi-brittle behaviour with clear nonlinear stress-strain (or loaddisplacement) response prior to failure.

P9 L21-22: I can see a better agreement with the cam clay model, but less so with the Mohr-Coulomb-Cap model proposed by Reiweger, which has a linear portion corresponding to the Mohr-Coulomb criterion which is not present in your results. Perhaps worth discussing in a bit more detail, or justifying why you think there is good agreement here?

---

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-80, 2019.