Review of "Modeling of crack propagation in weak snowpack layers using the discrete element method" by Gaume et al.

This paper details the use of a commercial discrete element software package to model a particular snow stability test that is the closest known proxy for slab avalanche behaviour. A cohesive snow slab and an underlying weak snow layer are modeled using assemblies of discrete spherical particles that are governed by a contact law and a cohesive law. The slab layer is undercut by progressively knocking out the weak layer support until the system loses stability and an unstable fracture propagates through the weak layer. This fracture process is studied numerically to determine the mechanical and structural influences on crack propagation speed, propagation distance, and fracture arrest. The paper is generally well written and organized.

The experimental design is questionable, however, and little attention is paid to the assumptions and motivations behind the particular numerical setup chosen. Very little mention is made of the physical snow properties that govern actual avalanches or even the stability test (Propagation Saw Test or PST) that is being modeled. For example, it is well known that thick persistent weak layers are strongly anisotropic, with much weaker shear strength than compressive strength (*Reiweger and Schweizer*, 2010, 2013). It is also well known that there is very often a distinct difference in both hardness and grain size between the slab and the weak layer (e.g. *Schweizer and Jamieson*, 2007; *DeVito et al.*, 2013), with the weak layer often having larger grains–the opposite of what was modeled. These important physical foundations of the avalanche problem seem to have not been considered in the numerical study, though they could have and indeed should have for such a discrete modeling exercise. No mention is made of why the particular triangular stacking scheme for building the weak layer was chosen, nor for whether this gives the weak layer any anisotropy. Indeed, the word "anisotropy" does not appear even once in the manuscript, yet this is a fundamental characteristic of avalanche weak layers. I am not convinced that any of the physical insights gleaned from the numerical sensitivity study are meaningful because little attention was given to the physical foundation of the model setup.

Regarding the model formulation (Section 2.2.2.): the spherical grains used in the slab (1 cm) are about an order of magnitude larger than the typical snow grains in a natural snow slab. Although you can "tune" the density in the model to match the bulk density that you are interested in, this is only relevant for getting the overall gravitational loading correct. This completely ignores the number and strength of bonds and contacts in the discrete slab assembly, which is what is actually important from the perspective of slab stiffness, bulk strength, and the ability to propagate a stress wave. Instead of focusing solely on getting the bulk density correct, and then assuming that the model is "valid," there are a number of other ways that you could set up and validate such a model. As mentioned above, a numerical hardness test should give similar values as in a real slab. Since you're interested in slab fracture as a mechanism of fracture arrest, then you should also conduct some bulk strength tests to confirm that the tensile strength of the slab as a whole (not the individual bonds) corresponds with published strength values (e.g. the actual scale of beam experiments such as *Sigrist et al.* (2005) could be simulated to validate your model). Although widely used, the density is not an appropriate proxy for the strength and microstructure of snow, a fact that has been recognized for decades (*Mellor*, 1975; *Shapiro et al.*, 1997). It's a shame not to recognize this given that this sort of discrete model setup holds such promise for moving beyond the limitations of density as the sole and predominant proxy measure in snow mechanics.

Figure 12 makes my point here. You are modeling a slab with a bulk density of 250 $\mathrm{kg\,m^{-3}}$, for which Eq. 4 suggests you should have a tensile strength of around 10 kPa. Of course, this equation came from *bulk* snow strength measurements from beam bending experiments, but you are using it for an ice grain contact law. In any case, the tensile fracture in your experiment, which originates at the top of the beam somewhere between the 5th and 6th panel in the figure, occurs when the tensile stress at the top of the beam is on the order of 100 kPa. So your bulk slab strength is an order of magnitude too high. You should be constructing the model such that your bulk model results agree with bulk snow measurements, which is not the approach you have taken here. In the absence of any such rigorous validation exercise, the results of the present study amount to the use of a highly tunable model to reproduce a particular phenomenon, but not necessarily for the right physical reasons.

Some more specific comments for reference

- A distinction should be made between thick, collapsible weak layers of the type considered in this study and weak *interfaces* between adjacent layers, that may not be "collapsible" beyond grain scale effects that are relatively unimportant in the energy balance. The latter type is also responsible for slab avalanche activity, and this type of weakness is usually not amenable to performing the PST. The results of the present study, and any conclusions you attempt to draw about avalanches in general from this sort of exercise, are limited to the former case. This should be explicitly stated.
- Why the focus on crack speed and propagation distance in the paper? I don't understand the motivation for choosing these two metrics. What about the cut length? The cut length is the primary measurement in the field test, so why not more discussion of this in the model? Especially important would be any slope angle dependence on the cut length.
- It would actually be possible, and very interesting, to characterize the hardness of the discrete slab and weak layer assemblies by performing proxy numerical measures of cone (*Johnson and Schneebeli*, 1999; *Schneebeli et al.*, 1999, e.g.) or blade penetration (*Borstad and McClung*, 2011) resistance. This seems obvious to me since you are essentially performing the same kind of experiment by indenting your weak layer with a "numerical saw." Can you report the reaction force encountered by this saw in the model? This sort of metric would help to test and calibrate the method of choosing the discrete grain size and contact/cohesive law parameters.
- In the PST, slab fractures and the associated arrest are almost always within several slab thicknesses ahead of the saw, which is precisely the location of the maximum tensile stress at the surface of the slab (which can be shown via the same beam theories that you reference, but you see the same thing in Figure 7). This indicates that the fracture arrest is rather a *structural* arrest caused by the bending induced by the saw cut, that in turn fractures the slab. In other words, it is an artefact of the test itself, and may not be fundamental in actual avalanches (the "en-echelon" style of fracturing may be a specialized exception). This is also important because the collapse occurs after the propagation of the initial localized fracture (*Reiweger et al.*, 2015; *McClung and Borstad*, 2012). For these reasons I think it is important to also report the propagation distance beyond the end of the saw, instead of (or in addition to) your new definition of propagation distance.
- p 613, line 19: these do not appear to be snow dynamics references, yet are mentioned as such
- p 614, line 5: were the tests actually performed, or will they be in the future? confusing use of tense here
- a better schematic diagram is needed for the contact and cohesive law used in the model
- p 615, line 12: does this mean viscous effects are minimal? The discussion of the restitution coefficient, and the lack of sensitivity, are a bit confusing. If indeed this has to do with the influence of viscous effects on the timescale of the simulated/actual PST, then this should probably be discussed further.
- Section 2.2.3, first paragraph: this is not a very clear description, and the reference to the protocol is not peer reviewed. Perhaps some of this addresses my concerns about model setup, but this cannot be judged from this short and confusing paragraph. The rest of the paragraph discusses how various parameters are determined for the model (tensile strength, Young's modulus), but the references are for *bulk* snow measurements, whereas it seems that a discrete model that explicitly simulates ice bonds should be using parameters for pure ice.

- p 620, lines 20-22: the stress in a bending slab or beam depends on *where* within the beam you are interested. I understand from the context that you are interested in the maximum tensile stress at the tensile face of the beam, but you need to explicitly state this for the reader.
- p 629, 3rd paragraph: this is far too late in the paper to introduce the weak layer structure used in the model. This should come in the model description/setup, along with a justfication for how and why you build the granular weak layer as you do.
- p 630, line 2: if the analysis is preliminary and incomplete, then leave it out.
- Table 2: it seems that a DEM setup should use ice parameters for strength and stiffness rather than bulk snow values, such that the bulk model setup should reproduce the bulk snow parameters. Putting in bulk snow values for the ice bonds and trusting the bulk response of the model seems like a backward approach. Finally, how can you specify the tensile strength of the weak layer (1.6 kPa) to such precision? This seems arbitrarily precise. How sensitive is the model to this parameter?
- Table 3: which tensile strength is varied in the sensitivity analysis? Slab or weak layer, or both?
- Figure 1: this figure has been published already many times. Are there not any other surface hoar pictures available?
- Figure 3: this is for an actual PST measurement in the field? This is not clear from the caption. More descriptive detail is needed.
- Figure 4: the contact bond model schematic needs explanation. What is A and B? Are you really using spheres of different size? If not, this is misleading. What is the shaded area, and the dashed lines?
- What are the properties of the foundation below the weak layer? A completely rigid foundation should lead to different response than a deformable foundation. This could also be explored in a sensitivity analysis, and might be revealing.
- Figure 12: what are the dashed vertical lines and tick marks in the panels? These are not labeled or explained, yet presumably they represent something...

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