Review of "Microstructure-based modelling of snow mechanics: experimental evaluation on the cone penetration test" by Herny et al

Main comments

The present paper is a relevant contribution towards understanding cone pentration tests of snow from microstructure based modelling. It includes many novel aspects and warrants publication after a little polish.

Main points:

- What is the influence of averaging the simulation data on fixed 4μm windows? To fully understand the simulations it would be helpful to include also the raw (non-smoothed) force signals alongside with the 4μm-window versions that are finally used to evaluate the statistical descriptors and compared to the experiments. The main question here is: Can the authors confirm that the results reported in l345 (i.e. the "more complex" behavior of the force standard deviation and the correlation lengths (Fig 5b,5c and corresponding figures in the supplement) are not affected by this averaging?

- The paper is rich in details, sentitvity studies, and results which is highly appreciated. On the other hand it not easy for a reader to condense the wealth of findings from the almost 40 figures into a neat summary that reveals the main physics of the simulations. This seems feasible though:

  First, the inspection of the Figures 5, S15, S19, S23 a) suggests that the mean force scales with the Youngs modulus as $\overline{F} \sim E^{-\beta}$ with $\beta \approx 1/2$. The scaling with cohesion follows something like $\overline{F} \sim C^{\alpha}$ where α seems to be around $1 < \alpha < 3/2$. This is consistent for all snow types. Second, the inspection of Figures 2, S16, S20, S24 b) reveals that the slope λ of the broken bonds percentage per unit length in the (bottom) linear regime correlates well with the initial contact density $\nu = z\phi$ (cf. [1]) when the volume fraction ϕ and the coordination number z (evaluated through $z = N_{\mathrm{bonds}}/N_{\mathrm{clumps}}$) is taken from Tab 1. At the same time λ is shown to be, in first approximation independent of the contact law parameters.

  So a simple law that is suggested by these observations, motivated by previous findings, and consistent with dimensional analysis would be

  $$\overline{F} = \mathrm{const}\, DC \left(\frac{C}{E}\right)^{1/2} \nu^{\gamma}$$

  where D is the initial contact area from the contact law (Eq 1 in the paper) and an unknown exponent γ. If therefore the dimensionless variable $F\sqrt{E}D^{-1}C^{-3/2}$ is plotted against ν, it would allow to merge all simulations for all parameters and snow types, including the sensitivity studies into a single figure at a glance while making contact to existing ideas for these elastic-brittle DEM simulations. Absence or presence of data collapse in this figure would greatly help to exploit the results, thus increasing the impact of the study.

Best regards,
Henning Löwe

Minor comments

(**Main text:**):

(l165): I don't understand "in the sense of the power diagram".

(l175): Table 1: Re-format such that units of density and ssa are not split.

(l175): As a quick cross-check from Table 1: Shouldn't be $n_{\mathrm{clumps}} d_{\mathrm{opt}}^3/\rho$ roughly equal to a constant ($\sim$ simulation container volume)? That does seems to work, but only RGlr is an outlier here. Why is that?

(l188): What does "weighting the bond magnitude between grains according to the spheres size" mean? The used D values for each snow type should be included in one of the tables.

(l191): fails → fail

(l191): Explanation not clear, what is meant by "scale the normal stiffness in order that all the sphere-sphere interactions between two grains fails at the same moment"

(l196): Does this mean the cohesive contact fails only in normal direction/tension?

(195): Give K_N here in terms of E and radii here for completeness.

(l198): Unclear: "relative displacement..."

(l203): Sentence unclear.

(l229): Unclear, where is it applied?

(l233): And throughout: mix of italic and roman fonts for variables (like E, C, etc) in equations and in text.

(l265/266): Wouldn't it be way easier to interpret the values if the statistical descriptors were only evaluated for $z > 5$mm where the transients mostly died out? From fig 4 its obvious that in the upper half the statistics is different. Same in Fig S22b. This is stated somewhere later anyways.

(l265/266): Definition of the error metric unclear. Maybe an equation would be easier.

(l295): Fig 2, S6b, S8b, S10b: Could you please also add the non-averaged data for the broken bonds? This is helpful to document absence or presence of intermittency (i.e. bond failure "avalanches").

(Fig 8): Is it possible to put shaded regions as uncertainty?

(l460-466): This argumentation is a bit confusing: Why does the Youngs modulus in YADE does not represent the material? If this was the case, why would the similarity to the ice values then support the actual choice of parameters? MAybe explaining a bit better what the influence of the contact model/grain representation is would help.

(496): Rephrase sentence "The larger...."

(l577): Just asking, but isn't this kind of data management a bit too old-school?

(**Supplement:**):

(Table 1): Is the number of grains "67882" (3rd row from bottom in the DH section) really correct?

(Table 1): headings: Does the term "grains" used here have the meaning as the term "clumps" from Tab 1 in the main paper? If yes, make consistent. In fact, why are the numbers of clumps/ spheres so different? Smaller containers for the sensitivity? Probably stated somewhere but I overread this.

References

[1] Gaume et al 2017, https://doi.org/10.1103/PhysRevE.96.032914