The Cryosphere Discuss., 9, C55–C63, 2015
www.the-cryosphere-discuss.net/9/C55/2015/

The Cryosphere Discussions
Open Access

Interactive comment on "Modeling of crack propagation in weak snowpack layers using the discrete element method" *by* J. Gaume et al.

P. Hagenmuller (Referee)

pascal.hagenmuller@meteo.fr

Received and published: 12 February 2015

1 General comments

1.1 Summary

The authors present numerical experiments of the propagation saw test (PST), using the discrete element method. The PST combined with particle tracking velocimetry, has become a standard field-method to evaluate the critical crack length required for self-propagation in the weak layer, and the propensity of fracture arrest due tensile failure of the slab. Theoretical and numerical models (e.g. Heierli et al., 2008) were developed to understand how the slab and weak layer properties affect the critical crack length.

However, our understanding of the crack propagation (speed and arrest) is still limited. The goal of this paper is to investigate, through a numerical model, the influence of slab properties (Young's modulus E, tensile strength σ_t, thickness D, density ρ), weak layer thickness D_{WL} and slope angle ϕ on the crack propagation speed c and the total crack propagation length l^*.

The 2D numerical model is based on the discrete element method. The slab is decomposed with spheres of radius $r = 0.01$ m on a regular grid aligned with the slope. The weak layer and its high porosity (responsible for the weak layer collapse) is reproduced by spheres of radius $r/2$ arranged in collapsible triangular shapes. The contact law between the spheres (slab and weak layer) is cohesive (visco-elastic and brittle). The bottom of the weak layer is fixed and the system is subjected to gravity and an "advancing" saw in the weak layer at constant speed. The propagation speed c and propagation length l^* are derived automatically from the displacement of the discrete elements of the slab.

The authors analyze the sensitivity to single system parameters of the computed propagation speed c and length l^*. This parametric study reveals that c increases with slab mass, Young's modulus and slope angle. It is not affected by the weak layer thickness. The propagation distance appears to increase with the slab tensile resistance force (strength and thickness), to decrease with weak layer thickness, slab Young's modulus and density. To account for the inter-dependence between Young's modulus, slab tensile strength and density, E and σ_t are then considered as function of ρ. This parameterization is used to compare the numerical results to PST field experiments. The conditions of field experiments (snow stratigraphy, slope angle, etc.) are globally simplified to functions of density and a constant slope angle. The agreement between model and experiments is fair, showing a good agreement in the order of magnitude of c and I^* and a coherent increase of c and l^* with ρ. Lastly, to have an insight of the failure mechanisms that lead to failure of the slab in tension, the authors investigate the development of tensile stresses in the slab during the propagation of the weak layer

TCD
9, C55–C63, 2015

crack. They compare the computed tensile stress to the one predicted by the beam theory and show that inertia (not quasi-static equilibrium) effects cannot be neglected to understand crack propagation for slab density higher than 180 kg m^{-3}.

1.2 Overall scientific and presentation quality

I feel that the approach presented in this paper is very interesting and worth publishing in the Cryosphere Discussions. Indeed, the model is able to account for inertia effects which appear to be critical to understand the fracture arrest propensity. To my opinion, the main contribution of this article is to explicitly reveal the fact that the weak layer collapse is not instantaneous, cannot be neglected to predict the development of tensile stresses in the slab, which ultimately leads to fracture arrest.

However, the presentation of these results requires major revisions and some clarifications are needed. I have listed specific comments below.

2 Specific comments

1. The abstract is written as an introduction and contains almost the same information as the introduction of the paper. Please consider significantly reducing the introduction part of the abstract (p610 l1-15) and adding method description and the key results.

2. The way the propagation speed is computed from field and numerical PST should be explicitly presented. This is described in the quoted reference (van Herwijnen and Jamieson, 2005). However, Figure 6 shows the temporal evolution of the computed vertical displacements and does not clearly reveal a "time delay between the onset of movement between markers proportional to the distance between the markers" (p613, l2-3). Consider adding some clarifications. If the

TCD
9, C55–C63, 2015

definition of c is somehow ambiguous, it would be appropriate to focus only on l^*, which is, in my opinion, much more important in the context of avalanche forecasting and already discussed deeper in the paper.

3. p616, l4-9. The link between microscopic contact law parameters and macroscopic mechanical parameters is missing. The DE model is, for instance, described in terms of k_n, k_n^b and the results are described in terms of E. Note that this correspondence can be derived analytically without bi-axial tests. The reference to one non-reviewed proceeding is not sufficient.

4. section 3.2. The parametric study is done by changing a single variable. The complete parametric study by the authors is very interesting. As described by the authors, some parameters have the same effects on the computed PST. If possible, a parametric study with a few dimensionless numbers derived from a dimensional analysis would be welcome. With this method, the competition between the different mechanisms would appear clearly. If not possible, consider formulating explicitly the key results of this parametric analysis in conclusion, which is now missing. Explain why the parametric study does not consider the influence of the weak layer strength. Indeed, the authors suggest that the propagation speed is "mostly influenced by the load due to the slab and WL strength" (p618, l24-25).

5. Dynamic effects are shown to induce a transient loss of support of the slab. These dynamic effects might be sensible to the chosen time step and discretization scale (sphere radius). It is important to provide the order of magnitude of the speed of elastic waves (dependent on r) in the sample and to compare it to the crack propagation speed. The time step used in the DE simulation is also missing. Moreover, the parametric analysis do not include a sensitivity analysis to the sphere radius r. If r does not affect the simulation results, add this information briefly in the text (as it is done for the restitution coefficient, p615, l9-12).

TCD

9, C55–C63, 2015

Interactive Comment

6. Comparison with field PST. The choices made to compare the numerical and experimental PST (Section 3.3, first paragraph) are unclear to me. I don't understand why two cases are distinguished. The size of the PST block, the slope angle, the slab density and weak layer thickness are variables which are certainly measured during the field experiments ("manual snow profile", p612, l15-17). The only missing parameter is the weak layer strength. But this variable can be derived from the measured critical length, as done by the authors in case 2. So all input parameters of the DE model, E and σ_t being function of ρ, seem to be available. I don't understand the choices made in case 1 and case 2. The point of the authors is not clear and requires major clarifications.

7. A few months ago, some of the authors have published an article in The Cryosphere Discussions entitled "Influence of weak layer heterogeneity and slab properties on slab failure propensity and avalanche release area" (December 2014). On contrary to the present paper, no dynamic effects and weak layer normal collapse are considered in the mechanical model (tell me if I am wrong). As noted (p628, l24-28), some of the numerical results in the two papers are in good agreement. I think it would be valuable if the authors could comment on this agreement, even the underlying implemented mechanisms are very different.

3 Minor comments

I have listed some minor comments. But I encourage all authors to make a in-depth check and correction of the wording and the organization of the text.

1. p610, l15: all "ff" appears in a strange way on my printed version of the paper. Check with editing service whether it is normal.

2. p610, l19-24, "Then, the relation ... in PSTs". Awkward sentence. Reword. For

instance, "In order to compare the numerical and experimental results, the slab mechanical properties (Young's modulus and strength) which are not measured in the field, were derived from slab density. The simulations are shown to fairly reproduce the field PSTs."

3. p611, l4, "if its size exceeds a critical length or if the load exceeds a critical value". The critical length already depends the applied load, doesn't it?

4. p611, introduction. A brief description of the PST would be welcome in introduction.

5. p611, l19-21: "For instance, it is not uncommon to perform PST field measurements with widespread crack propagation on one day, while a few days later, with seemingly very little changes in snowpack properties, cracks will no longer propagate.". Quote appropriately.

6. p613, subsection 2.2.1. This subsection should be incorporated to the global introduction.

7. p614, l10: "completely rigid". Do you mean "fixed"?

8. p614, l10: "simulations" -> "simulations (see Figure 4a)"

9. p614, l12: "cubic" -> "square"

10. p614, l15: "triangular form". It is impossible from the provided figures to see the exact form of the weak layer elements (nb spheres, angle). Moreover, it is unclear how the thickness of the weak layer is changed in the parametric study. I suggest deleting Figure 4b, which is useless in this form, and to replace it with a zoom on the weak layer structure.

11. p615: I don't understand the role of the k_n and k_n^b. If the bond is cohesive does the spring between grains play a role? Or is it a serie of linear springs? Why is the value of k_n in N/m while k_n^b is in Pa/m?.

12. p616,l18-20: "The only difference with the procedure for field measurements is that with DE we do not need markers since we have access to the displacement of every grain of the system.". Delete.

13. p618, l16 "from almost zero". Give value.

14. p620,l1: "Hence, fracture arrest propensity decreases with slope angle.". From Figures 9c1, 9c2, this conclusion does not appear as clear as stated. Be more precise on the cases where this conclusion applies.

15. p620, l6-8: "This interpretation is supported by the observation that the tensile opening of the crack always starts from the top surface of the slab in both DE simulations (Fig. 7) and in field PSTs.". I do not agree. This observation only confirms that tension is due to slab weight projected along the slope AND bending. But it does not tell which mechanism is the more important.

16. p621, l21-23: I don't understand why different densities (240 and 250 kg/m3) are used? It is not very important for the comparison but it would make the presentation clearer. The comparison between Figure 3 and 6 would be also easier if the computed and measured displacements would be plotted on the same figure with the exact same marker position.

17. p622, l19-22 "The slight overestimation for low densities might be due to the fact that, to compute the propagation speed, the slab was considered as purely elastic and possible plastic effects in the slab that might induce energy dissipation were disregarded.". I suggest to keep this idea for the discussion section.

18. p622, l25-28 "This is not the case for the experiments for which the critical length generally increases with increasing density due to the associated increase of Young's modulus and a strengthening of the WL (Zeidler and Jamieson, 2006a, b; Szabo and Schneebeli,2007; Podolskiy et al., 2014)". I don't understand what you mean.

19. p623, l3-6 "Furthermore, for case 1,..., the better quantitative agreement with the experiments.". Awkward sentence. Reword.

20. p623, l22-24 "However, we argue that, as soon as fracture arrest occurs within the beam, the crack propagation distance is almost independent of the beam length.". I agree with your assumption. Since one of your conclusion (see abstract) is about column length, I suggest that you rapidly check your assumption with the model which is directly designed to do so.

21. p624, l5: σ_{xx} is not necessary tension. Moreover, indicate how the stress tensor is calculated from microscopic contact forces.

22. p624, l18: "right side" -> "above the undamaged weak layer (right side in Figure 12).

23. p626, l3: "strength leading" -> "strength, leading"

24. p626, l9: "where stresses do not have time to establish". Do you mean that the displacement of the slab due to gravity is too slow to establish a mechanical equilibrium between bending and gravity?

25. p626, l14-28: In my opinion, the fit of the maximum tensile stress for density above 180 kg/m3 is not necessary and does not give additional information on the underlying mechanism. Delete and reword section appropriately.

26. Figure 2. Add scale in the figure on the right.

27. Figure 4. Replace subfigure 4b by a zoom on the WL structure
28. Figure 8. m/s -> m s^{-1}, kg/m3 -> kg m^{-3}
29. Figure 9. Explain what is a_c in the caption of the figure.
30. Figure 10. a) Explain to what correspond the boxplot (max, 75%, median, ... ??) b) Report the median value on the subfigure.
31. Figure 12. Explain that σ_{xx} was linearly (?) interpolated between grains. The tensile stresses before failure appears to be very large (500 kPa » 10 kPa) ???

Interactive comment on The Cryosphere Discuss., 9, 609, 2015.