Nat. Hazards Earth Syst. Sci. Discuss., 2, C1737–C1740, 2014
www.nat-hazards-earth-syst-sci-discuss.net/2/C1737/2014/

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Evaluating snow weak-layer rupture parameters through inverse Finite Element modeling of shaking-platform experiments" *by* E. A. Podolskiy et al.

A. van Herwijnen (Referee)

vanherwijnen@slf.ch

Received and published: 29 July 2014

General comments:

In the paper, a method is presented to evaluate weak layer constitutive parameters based on a combination of snow fracture experiments and finite element modeling. The approach presented in the paper is interesting and provides information which would not be available solely based on experimental results, and the paper addresses the important issue of how to model weak layer failure, which, despite various studies, is still mostly an unresolved issue. As such, the paper should be considered for publi-

cation in NHESS. However, major revisions are required before it can be accepted for publication. Overall, the paper requires extensive restructuring and rewriting to make the paper more readable, and the authors need to do a better job to justify some of the assumptions made in the model.

Specific comments:

First and foremost, the authors should use the term fracture throughout the paper and not rupture. Rupture is for soft materials - a rubber hose or a blood vessel can rupture - while fracture is for brittle materials.

Several assumption were made to build the FEM model and the authors should clearly state what these are and why they were made. Some of the assumptions are based on experimental results. Specifically, based on high-speed video recordings of the experiments the authors state that there was no discernible collapse of the weak layer during fracture and that the snow below the weak layer behaves like a rigid oscillator. Both these assumptions are crucial as they allow the authors to omit the lower block from the FEM simulations and treat the weak layer as an interface. I find it hard to believe that a 1-2 cm thick artificial weak layer consisting of dendritic snow with a density of 100 kg/m3 would not collapse during fracture, especially since collapse in low density storm snow has been documented in field experiments (Bair et al., 2012). Given the importance of these assumptions, it is somewhat surprising that no quantitative experimental results are shown to substantiate these claims, especially since those results are also not presented in Podolskiy et al. (2010). At the very least, the authors should provide one or two videos as supplemental online material. However, I would strongly urge the authors to include image correlation analysis to provide experimental evidence for their claims.

In the FEM model, the weak layer is modelled as an interface with zero thickness, characterized by a normal and shear stiffness and obeying a Mohr-Coulomb failure criterion. As the oscillation frequency increases, stresses within the sample increase and

progressively more nodes satisfy the failure criterion (Figure 7). Sample failure is then defined as the instant when the stress in all nodes exceeds the strength. However, prior to this instant, when the stresses are removed, no flaw remains. This assumption is not clearly stated in the methods section and only first addressed in the results section (page 4547). Furthermore, this assumption is not justified as it is well known that snow behaves as strain-softening material, as also assumed in Gaume et al. (2013). Thus, the authors need to provide more convincing arguments for their assumption, or show that including a more realistic behavior for the weak layer does not influence the results in any appreciably manner. For instance, I would expect that if nodes which fulfill the failure criterion are allowed to fail, and stress redistribution is taking place, the elastic properties of the snow block above the weak layer would have greater influence on the obtained results.

Overall, the paper is also a pretty hard read. It is rather lengthy, sometimes repetitive and somewhat scattered. The authors should restructure the paper as well as shorten some sections. For instance, section 2 describes the objectives and the scope of the study, while section 3.3 again describes the scope of the study. Clearly, the writing can be more to the point and compact.

Technical corrections:

I have provided more detailed comments in a separate annotated pdf file.

Bair, E.H., Simenhois, R., van Herwijnen, A., Birkeland, K. and Dozier, J., 2012. Storm snow avalanches: characteristics and forecasting, International Snow Science Workshop ISSW 2012, Anchorage AK, U.S.A., 16-21 September 2012, pp. 111-114.

Gaume, J., Chambon, G., Eckert, N. and Naaim, M., 2013. Influence of weak-layer heterogeneity on snow slab avalanche release: application to the evaluation of avalanche release depths. Journal of Glaciology, 59(215): 423-437.

Podolskiy, E.A., Nishimura, K., Abe, O. and Chernous, P.A., 2010. Earthquake-induced

snow avalanches: II. Experimental study. Journal of Glaciology, 56(197): 447-458.

Please also note the supplement to this comment:
http://www.nat-hazards-earth-syst-sci-discuss.net/2/C1737/2014/nhessd-2-C1737-2014-supplement.pdf

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 2, 4525, 2014.