The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2019-86-RC2, 2019

The Cryosphere
Discussions
Open Access

Interactive comment on "Modeling snow slab avalanches caused by weak layer failure – Part I: Slabs on compliant and collapsible weak layers" *by* Philipp L. Rosendahl and Philipp Weißgraeber

Michael Zaiser (Referee)

michael.zaiser@ww.uni-erlangen.de

Received and published: 20 June 2019

The paper revisits the mechanical model for brittle failure of a weak layer under compressive and mixed loads as proposed by Heierli and co-workers. The model shares the conceptual foundation of that model and its limitations (brittle vs quasi –brittle behavior of snow, use of a purely energy based criterion for fracture propagation which does not account for the actual stress state at the crack tip). Within these limitations, the work, which consists of analytical considerations validated by FEM calculations, is solidly done. The main novelty introduced by the authors consists in the inclusion of weak layer elasticity, which is considered in terms of a spring model ('Winkler founda-

tion') commonly used in adhesion mechanics. Methodologically this is fine.

The mathematical results are reasonable. First it is clear that accounting for elastic energy stored in the weak layer is bound to increase the energy release rate in comparison with the results of Heierli et al. who treat the interface layer as stiff. Second, an elementary estimate shows that the relative energy stored in the weak layer and in the slab under a fixed gravitational load of order σ is proportional to

$E \propto h_i \sigma \epsilon_i = \sigma^2 (h_i/E_i)$

where ϵ_i is a measure of the characteristic strain in layer i (slab or weak layer), h_i is the respective layer thickness and E_i is the elastic modulus of the layer. This gives also a rough measure of the respective contribution to the energy release rate. In simple words, weak layer elasticity is relevant if the layer is thick and elastically soft.

Thus, it is in the nature of the case put forward by the authors that they are considering effects that are relevant for thick soft weak layers, and much less so for layers that are thin or elastically similar to the slab. This is borne out by the parameters compiled in Table 1: The weak layer thickness is assumed 5 cm, (thick), the slab thickness is 40 cm (thin), the elastic modulus of the slab is 35 times higher than of the weak layer.

A quantitative comparison with results of the Heierli model is given in Figure 11. For a WL thickness of 3cm, and the other parameters as in Table 1, my simple estimate shows that the energy stored in the WL is about 3 times higher than for the slab, accordingly the overall energy release rate in Figure 11 for that thickness is about 4 times higher than in the Heierli model. The results are thus what one expects. Good.

Thus, the authors show that for some cases the influence of the weak layer on energy release rate may be significant. They in my opinion seriously over-state their case when they imply, by their choice of parameters, that it is always predominant.

(i) First let us note that the elastic modluus values used by the authors are dubious in absolute terms. Elastic moduli of snow can be inferred computationally from FEM on snow microstructures determined by micro-CT, and these calculations can be experimentally validated based on elastic wave propagation data, see [1] Gerling, B., Löwe, H., van Herwijnen, A. (2017). Measuring the elastic modulus of snow. Geophysical Research Letters, 44, 11,088–11,096 and [2] Koechle and Schneebeli, Journal of Glaciology, Vol. 60, No. 222, 2014. The authors should address the discrepancy between those data and the elastic moduli used in their computations.

(ii) Irrespective of absolute numbers, snow elastic moduli are highly density dependent, scaling in approximate proportion with the fourth power of density [1] and following the same density vs modulus curve for both weak layers and bulk snow [2]. Thus, differences in weak layer and slab density of a factor 2 can indeed account for significant differences in modulus. Nevertheless the assumptions of Table 1 seem excessive - to explain the modulus ratio of a factor of 35 assumed by the authors, the weak layer density would need to be around 100 kgm^{-3}. The authors should provide evidence that such huge density differencess between slab and weak layers are indeed common, e.g. in experimental snow density profiles (BTW I have a few counter examples at hand).

Also one may note that weak layer density relates to collapse height. Under the reasonable assumption that the weak layer compacts, during collapse, at least to the density of the overlying slab, a layer of thickness 5cm compacting from 100kgm^{-3} to 240kgm^{2} would entail a collapse height of about 3cm which appears excessive compared with collapse heights observed in field experiments (propagation saw tests) published in the literature.

In summary, it should be clearly explained by the authors that the difference between the present model and the previous model of Heierli et al is contingent on a very significant modulus (density) difference between slab and weak layer, and the authors should discuss, from a snow science perspective and providing appropriate evidence, under which circumstances such modulus/density differences are to be expected. This would help to put the results into context and to illustrate their practical relevance. They

should explicitly relate their parameter assumptions to field data e.g. on propagation saw tests and demonstrate that they are reasonable in view of established relationships between density, modulus, and in view of observed weak layer thicknesses and collapse heights. If the results are thus put into perspective, I think the paper should be published since it sheds light on an aspect of weak layer collapse which, while in real world situations most probably not as dominant as the authors try to suggest, may in some circumstances be of relevance for the interpretation of propagation saw test data and snow stability in general.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-86, 2019.