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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

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Prof. Michael Zaiser has provided a detailed analysis of our model and has raised concerns regarding our choice of the parameters. He argues that for other parameter sets the effect of the weak layer would be less pronounced.

We agree that our weak layer modulus and thickness assumptions can be improved to better agree with field observations. We have, therefore, rerun our calculations with new parameters for the weak layer. The difference to the model of Heierli and Zaiser [1] (that assumes a rigid foundation) is now smaller but still very pronounced. We further clarify other differences between our model and that of Heierli and Zaiser [1].

We thank Prof. Zaiser for the detailed review. We have update the figures corresponding to parameter studies in our manuscript and have extended the discussion considering this input parameter dependence.

Reviewer comments

(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.

We derive the slab's elastic modulus from its density using a power law fit to Scapozza's [2] data (Eq. (29) in our manuscript). We use this equation to compute data that can be compared to the results of many other analyses, also very recent works, that use the same concept [3–11].

The equation provided by Gerling et al. [12] is cross-validated using two different experimental methods, therefore, likely more reliable and should perhaps be used for future works. However, given its large variability (E_slab between $7\,\text{MPa}$ and $110\,\text{MPa}$ for our assumption of $\rho_\text{slab} = 240\,\text{kg/m}^3$), using Eq. (29) seems reasonable as well. We added the following paragraph to our manuscript:

> *Note that Gerling et al. [12] provide a different equation that is cross-validated using two different experimental methods and, therefore, likely more reliable. However, we chose Eq. (29) for comparability with previously published models.*

Our choice of weak layer Young's modulus is based on Köchle and Schneebeli's statement "*the weak layer was, on average, about half as dense as the layers above and below*" [13]. Hence, our assumption of a slab density of $240\,\mathrm{kg/m^3}$ corresponds a weak layer density of approximately $120\,\mathrm{kg/m^3}$. Then, the power law fit to Scapozza's [2] data yields $E_\mathrm{slab} = 5.23\,\mathrm{MPa}$ and $E_\mathrm{weak} = 0.16\,\mathrm{MPa}$. Although it is not clear whether Eq. (29) is valid for weak layers, this seemed a reasonable first guess.

However, Köchle and Schneebeli also point out that "*the elastic modulus was much higher (on average 20 times) in the layers above and below*" [13]. With $E_\mathrm{slab} = 5.23\,\mathrm{MPa}$ this yields $E_\mathrm{weak} \approx 0.25\,\mathrm{MPa}$. Such values and corresponding densities are sensible as discussed in the next point. In order to avoid extreme assumptions and to show that our model does not require very large elastic contrasts, we used a weak layer Young's modulus of $E_\mathrm{weak} = 0.25\,\mathrm{MPa}$ to recompute relevant parametric studies of the present work and updated our figures accordingly. Doing so did not change any statement qualitatively and all conclusions in the manuscript remain valid. Table 1, all figures and concerned figure captions are updated accordingly. We added a brief statement of our reasoning concerning parameter choices to the manuscript:

> *A weak layer Young's modulus of $E_\mathrm{weak} = 0.25$ MPa is chosen based on the findings of Köchle and Schneebeli [13] who report an average ratio of weak layer to slab Young's modulus $E_\mathrm{weak}/E_\mathrm{slab} = 1/20$.*

(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 $100\,\mathrm{kgm^{-3}}$. 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 reason- able assumption that the weak layer compacts, during collapse, at least to the density of the overlying slab, a layer of thickness 5cm compacting from $100\,\mathrm{kgm^{-3}}$ to $240\,\mathrm{kgm^{-3}}$ 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.

As pointed out in our answer to remark (i), our choice of modulus ratio was indeed based on estimates of density differences (by Köchle and Schneebeli [13]). We cannot say whether such large density differences are common, however, a number of authors points at weak layer densities much lower than the one we initially assumed ($120\,\mathrm{kg/m^3}$). For instance, using two different measurement techniques, Föhn [14] reports densities of surface hoar layers i) between 44 and $215\,\mathrm{kg/m^3}$ with a mean of $102.5\,\mathrm{kg/m^3}$ and ii) between 75 and $252\,\mathrm{kg/m^3}$ with a mean of $132.4\,\mathrm{kg/m^3}$. Horton et al. [15] even measure densities as low as $\rho = 30\,\mathrm{kg/m^3}$. As pointed out above, we changed the weak layer Young's modulus in an effort to avoid polarizing assumptions. According to Eq. (29) of our manuscript, the new weak layer modulus corresponds to a weak layer density of $\rho_\mathrm{weak} \approx 135\,\mathrm{kg/m^3}$.

Concerning weak layer thickness and collapse heights, we analyzed the data set provided by Gaume et al. [8] and used the data set's mean weak layer thickness ($48\,\mathrm{mm} \approx 50\,\mathrm{mm}$) for our parametric studies. In view of other publications such as the work of Jamieson and Schweizer [16], who report weak layer thicknesses between 2 and $30\,\mathrm{mm}$, however, we agree that our initial assumption of $5\,\mathrm{cm}$ may seem excessive. Again, in order to avoid extreme assumptions, we have changed the weak layer thickness to $t = 2\,\mathrm{cm}$ for our parametric studies and updated Table 1, figures and corresponding figure captions. We have included the above arguments in our text:

> *Assuming Eq. (29) to be applicable to weak layers as well,* $E_{\text{weak}} = 0.25$ *MPa corresponds to a weak layer density of* $\rho_{\text{weak}} \approx 135\,\mathrm{kg/m^3}$*. This agrees with density measurements of surface hoar layers by Föhn [14] who reports densities i) between* 44 *and* $215\,\mathrm{kg/m^3}$ *with a mean of* $102.5\,\mathrm{kg/m^3}$ *and ii) between* 75 *and* $252\,\mathrm{kg/m^3}$ *with a mean of* $132.4\,\mathrm{kg/m^3}$ *using two different measurement techniques.*
>
> *With reference to Jamieson and Schweizer [16] who report weak layer thicknesses between* 0.2 *and* 3 *cm, we chose* $t = 2$ *cm. Further parameter choices are summarized in Table 1.*

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.

It is our intention to provide a model of the mechanical behavior of skier loaded slabs on porous and collapsible weak layers. The weak layer's porosity that is required for its collapse implies a certain elastic contrast between slab and weak layer. Slab avalanche release owing to other failure mechanisms such as time-dependent damage accumulation are likely better captured using (gradient) plasticity approaches and such.

We hope that our assumptions and results are put into context with the above arguments and changes to our assumptions. However, we disagree with the statement 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*".

In order to illustrate this, consider the attached Figure 1 where we have recomputed the results shown in Fig. 11 of our manuscript using different weak layer moduli. The graph shows that even when the elastic moduli of weak layer and slab are very similar (ratio 1:2.5 at $E_{\text{weak}} = 2.0\,\mathrm{MPa}$), there is a significant difference between Heierli's and the present model. The difference originates from the elastic energy of the slab that is still supported by the weak layer. Because Heierli models only the unsupported section of the slab, which can be thought of as a rigid weak layer, this energy contribution is neglected (Figure 1).

Aside from the improved accuracy of the energy release rate of cracks, the contribution of the present model is significant because it provides weak layer stresses in the same analysis. One might again argue that while slab and weak layer are rather homogeneous, the elastic halfplane solution shown by Föhn's [17] suffices. However, i) weak layers that are softer than a bonded slab are characteristic for skier-triggered avalanche events [18,19,20] and ii) the presented modeling strategy allow for considering arbitrarily layered slabs instead of just homogeneous ones. That is, it is capable of providing analytical expression for both weak layer stress and the energy release rates of cracks in stratified snowpacks. This is important because, for instance, melt-freeze crusts can render slabs stiff in bending yet soft in tension depending on their location within the snowpack. A corresponding follow-up work is already in preparation.

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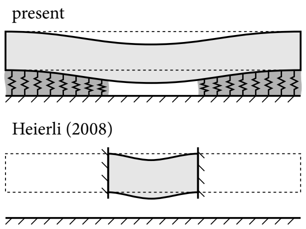

Fig. 1. Impact of model assumptions on slab deformations that directly affect the stored energy.

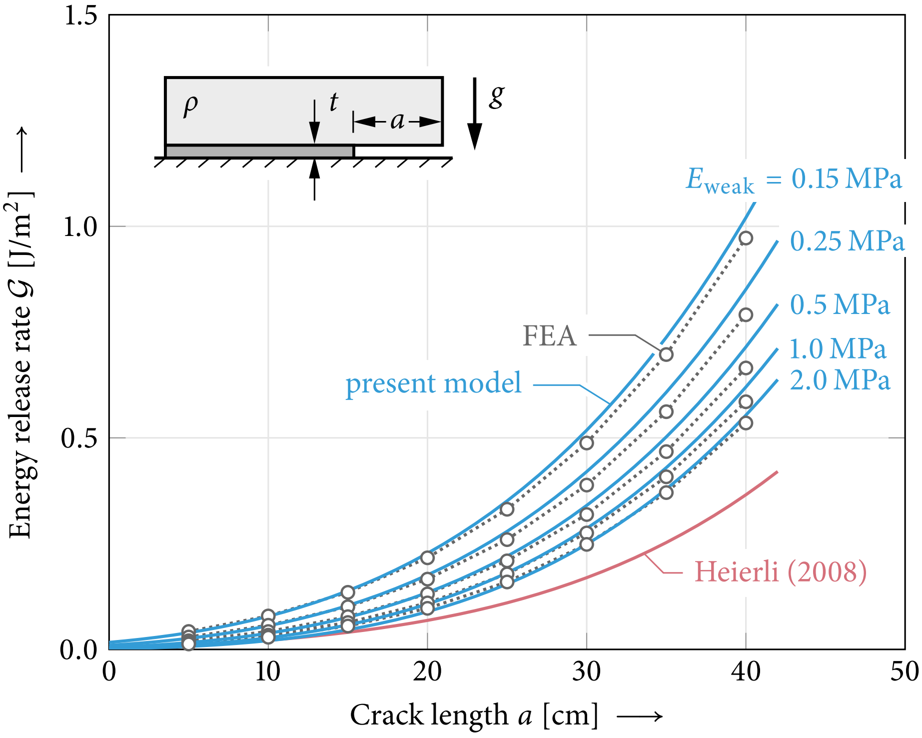

Fig. 2. Fig. 11 of the manuscript recomputed with slab Young's modulus E_slab = 5.23 MPa, weak layer thickness t = 1 cm and different weak layer Young's moduli E_weak.