The Cryosphere Discussions

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Reply on RC2

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Author comment on "A closed-form model for layered snow slabs" by Philipp Weißgraeber and Philipp L. Rosendahl, The Cryosphere Discuss.,
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Dear reviewer, thank you for your comments and suggestions, which help us to improve our paper. Please find our response to each concern and remark of the review below:

This study develops an analytical model for the investigation of mechanical behavior of a stratified snow cover over a weak layer. The snow cover is considered as an arbitrarily layered beam solved by laminate mechanics, while the weak layer is modeled as a set of springs attached to the bottom of the snow cover. The model is firstly verified with the experimental data and the numerical results from finite element modeling, and then adopted to investigate the factors affecting slab release, including layering, bridging and slope angle. The key novelty of this study to the reviewer is the consideration of the layered snow, compared to the previous study conducted by the authors. The outcomes from this study could offer useful information for understanding the failure behavior of stratified snow over a weak layer, and thus give relevant information on slab avalanche release. However, there are some concerns to be clarified as detailed below.

1) Consideration of the crack: As described in the methodology, the model considers the weak layer as springs with normal and shear stiffnesses, and can handle different scenarios such as the ones with partially collapsed weak layer in Fig. 4. But it is not clear how the crack in the weak layer is considered and whether the scenarios in Fig. 4 are predefined. If the scenarios are predefined, how to determine these different initial conditions in practice? If not, please clarify the triggering of the crack. For example, what is the criteria to trigger the crack? Do the springs have certain shear and normal strength, above which they break? Please also discuss the propagation of the crack with time.

The model can be used to study both slabs on intact weak layers and slabs where the weak layer has partially failed. The weak layers are modeled with a so-called weak-interface approach as it has been used in many works to study structural situation with stiffer structures being supported by foundations of significantly lower stiffness [Het1946]. Such weak-interface models can also be used to study locally failed foundations by removing the support effect of the interface [Kre1992, Len2001]. The present model can then be used to establish failure models that require the stress distribution in the weak interface and the energy release rate of crack configurations [Ste2015]. We have proposed a mixed-mode finite fracture mechanics approach for non-layered slabs in an

earlier work [Ros2020b] and we will incorporate the present model for layered slabs in this framework in the future.

We thank the reviewer for pointing out that the description of the boundary conditions in both scenarios (slabs on intact weak layers and slabs where the weak layer has partially failed) should be improved. We will revise this section to make it more clear.

2) Comparison with the results from a homogeneous equivalent layer: In the results 4.1, the current model has been compared with the homogeneous model by Monti et al. (2015). It is stated that "Both concepts are benchmarked against the stiffnesses computed using finite element analyses", please clarify whether "both concepts" are the current model and the model by Monti et al. (2015). If yes, as they have been benchmarked already, why the model by Monti et al. (2015) does not have consistent stiffness with the FEA in Fig. 10? In addition, please clarify that if the correct equivalent stiffnesses are implemented to a homogeneous model such as that by Monti et al. (2015) or the previous model by the authors (Rosendahl and Weißgraeber, 2020a), will the homogeneous models give good prediction on the mechanical behavior of the slab?

Thank you for pointing out this ambiguity. What we mean is that we compare the model by Monti et al. against finite element analyses and also compare the present model against FE analyses. We will improve the formulation in the revised manuscript to make this more clear.

Monti et al. have not focused on stiffnesses but instead looked at stresses within the snowpack, where the agreement was not 100% but deemed satisfying by the authors. We can only speculate that they were not aware of the stiffness discrepancies.

If slabs are layered, there is no one equivalent stiffness. Instead, layered slabs always respond differently to bending or tension. When there are only small differences in the stiffness of all layers of the slab, the use of an equivalent stiffness can provide satisfactory results because the error from neglecting layering effects is small [Ber2023]. We will include this point in our revised manuscript.

References used in the response:

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