

Revision of: *The effect of slab touchdown on anticrack arrest in propagation saw tests*

Dear Editors and Reviewers,

Thank you for your thorough review and constructive feedback on our manuscript. We appreciate the opportunity to revise our work based on your comments and suggestions. We carefully considered each point and made corresponding revisions to enhance the clarity, accuracy, and impact of our manuscript.

- 5 In this document, we address each remark (*blue, italic*) point-by-point (black). We hope that these modifications adequately address the concerns raised and improve the manuscript. We are grateful for the guidance that the reviewers' expertise has provided and are confident that these changes have strengthened our submission.

Referee #2

- 10 *The paper is well-written and presents solid research, though its contribution is somewhat incremental compared to existing literature, particularly with respect to previous work by both the authors and other scholars in the field. Despite this, I believe the study introduces enough novel insights to warrant publication in NHESS after a few major revisions detailed below together with more specific minor comments.*

- Remark 2.1** *Novelty and contribution: As briefly mentioned above, although the paper builds upon previous work by the authors and others in the field (especially Benedetti et al.), it does introduce enough novel elements to warrant consideration for publication in NHESS. A significant portion of the mechanical model has already been presented in Weissgraber and Rosendahl (2023) and applied in Bergfeld et al. (2023). Moreover, the concept of touch-down distance in a static slab-weak layer model was introduced by Benedetti et al., although without incorporating slab shear (Euler-Bernoulli beam theory) nor slab layering. The important contribution of this paper lies in combining these two elements, leading to a more robust model, performing detailed sensitivity analysis, validation with FEM and DEM, and applying it to field data. I believe this combination is very solid and represents excellent work. However, I encourage the authors to be more explicit about the overlap with prior work and to clearly articulate the unique contributions of this paper.*

The model is based on the well-validated and established WEAC model. However, it employs a completely new set of modeling techniques to allow for the consideration of slab contact, which was not possible in WEAC. To this end, we extend WEAC by incorporating slab contact.

- 25 We appreciate your suggestion to outline the differences from Benedetti's model more clearly. We will also acknowledge that Benedetti et al. introduced the first mechanical model for this aspect. Our model incorporates several aspects not covered by Benedetti et al., which are known to be critical for the meaningfulness of mechanical analyses:
- We use a first-order shear deformation theory because the beam sections are often too short for the normality assumption of Euler-Bernoulli beam theory. Additionally, stress concentrations in the weak layer lead to localized line loads along the beam length, violating this assumption.
 - Slab layering significantly affects stiffness (bending and extensional) and generally leads to bending-extension coupling. To account for this, we use laminated plate theory to represent slab behavior accurately.

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- The compliance of the weak layer substantially alters the slab-weak-layer system, influencing energy-driven failure processes. To address this, we use the WEAC model, validated with finite element analyses based on weak-interface modeling and Winkler foundations for weak-layer compliance.
 - We use the weak interface model to identify the shear and normal stresses in the weak layer. Here the Benedetti et al work uses a very strong simplification of a rigid beam segment that shows a rigid body movement at the center of gravity of the unsupported slab. This leads to unrealistic, linear stress distributions along the weak layer interface.
 - 40 – Current research has shown that PSTs can be used to identify the fracture energy of a shear crack or anticrack within the weak layer and the concepts of fracture mechanics apply. To study this brittle fracture phenomena, we use the energy release rate at the crack tip (stress intensity factors could also be used, but are not well defined for weak interface models). We compute the energy release rate by using the concepts of weak interfaces as this is also established for the fracture mechanics analysis of adhesive joints. In a full continuum analysis the crack tip would show a stress singularity within the linear-elastic analysis, which would lead to vanishing critical loads (the smallest loading leads to infinite stresses) and hence we see, that a strength-based criterion cannot applied to such strong stress concentrations. Weak interface models are not able to cover the stress singularity but still provide meaningful results with strong local stress concentrations at the end of the intact weak interface but weak interface model still allow for the validated computation of differential energy release rates.
 - 45 – To render the evolving contact of the slab on the substratum we use a separation into stages like Benedetti et al did. The major improvement is that we have used a set of springs to account for the interaction of the unsupported slab with the slab with intact weak interface and with the part of the slab already in contact with the substratum. The spring stiffnesses are obtained by using a full beam model with the above-described features as it is implemented in weac. This represents an important expansion of the weac model.
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55 **Remark 2.2** *Some overstatements and comparison/discussion with Benedetti: I noted a few overstatements throughout the paper that I believe are unnecessary. Additionally, it would be good to add a comparison with Benedetti's "twin" model or at least a deeper discussion. For instance, the paper introduces the different phases during collapse, which were already presented in greater detail (with additional steps) by Benedetti et al. It appears from the methods section that the authors are positioning themselves as the first to introduce these phases, but they were previously discussed by Heierli (though in less detail), Benedetti et al., and more recently by Siron et al., who also included dynamics. While this is briefly acknowledged in the discussion, it should be much clearer earlier in the paper.*

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We will carefully review the manuscript for potential overstatements and make adjustments where necessary. Additionally, we will explicitly acknowledge and clearly reference the contributions of Heierli, Benedetti et al., and Siron et al. regarding the concept of stages during collapse. While we intended to build upon these prior works, we understand the importance of highlighting their foundational contributions earlier in the manuscript. In the revised document, we will ensure these aspects are presented more transparently and thoroughly.

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70 **Remark 2.3** *Model comparison: I would suggest comparing the current model with Benedetti's analytical model under various configurations (e.g., without layering), particularly regarding unsupported length and touch-down distance. This would better illustrate the necessity of including slab shear. For example, slab stress in Benedetti has characteristics similar to the ERR in your paper, but this trend is not reflected in Benedetti's weak layer stresses. Elaborating on the differences would be great: is the discrepancy due to incorrect assumptions (e.g., lack of slab shear) or something else? The claim that Benedetti's model is not continuous also needs clarification. The issue seems to be with derivatives, which are often discontinuous in contact and friction problems. A more precise statement would help.*

Comparing both approaches shows that Benedetti's model can be considered a special case of the present model, particularly in the first stage, due to similar boundary conditions. Equation (8) of the in our manuscript

$$0 = \frac{a_{AB}^4}{8K_0} + \frac{a_{AB}^3}{2k_R^\circ} + \frac{a_{AB}^2}{2\kappa A_{55}} + \frac{a_{AB}}{k_N^\circ} - \frac{f_c t}{q}, \quad (1)$$

reduces to Benedetti's solution

$$L_{IC} = \sqrt[4]{\frac{8E_s I_s h_w}{q_v}} \quad (2)$$

under the following assumptions:

- 80 – The compliance of the weak layer on which the supported section of the slabs rests is assumed to be infinitely high. No deformation within that section of the weak layer. Both the normal stiffness relevant for the transverse section force in the slab k_N° as well as the rotational stiffness k_R° relevant for the bending moment in the beam are set to infinity.
- The shear stiffness of the slab without foundation κA_{55} is set to infinity as well, allowing for no shear deformation within the slab.
- 85 – For the effective (cylindrical) bending stiffness of the slab $K_0 = D_{11} - B_{11}^2 A_{11}^{-1}$ is obtained under consideration of no bending-extension coupling in the slab (e.g. by a layering which is not symmetric to the mid-line of the slab)
- Further the bending stiffness D_{11} no effect of layering is assumed and a homogeneous, isotropic material is assumed within the slab.
- It assumed that the collapse height equals the thickness of the weak layer.

With these assumptions eq. 8 reduces to the polynomial of L^4 only and the solution by Benedetti et al. is obtained for stage I. The largest difference between the two approaches lies in the weak-layer compliance.

We will make it clearer in the revised document that equation (8) includes the special case of the Benedetti et al. solution.

Remark 2.4 *Crack arrest and model relevance: The link to crack arrest is only introduced late in the discussion, which is somewhat confusing. In addition, while I appreciate the use of fracture mechanics, the paper introduces the critical energy release rate (G_c), essentially stress (σ) divided by weak layer stiffness, adding another layer of uncertainty as one needs to know k_n and k_t as well, properties which are very hard to measure. I recommend instead, or at least additionally, to present results in a stress-strength framework to avoid this additional uncertainty and offer further insights into crack arrest. For instance, including metrics like τ_c/τ_s and σ_c/σ_s , alongside G_c/G_s , would be beneficial, as these are commonly used in engineering and avalanche science to assess stability and also in many strength-of-material oriented models. This would be a very interesting additional outcome that could give insights for researchers using different frameworks (fracture or strength of materials). In fact, the sentence "As shown in Table 3, the quotient of both G_c/G_s is therefore a reasonable metric to map the propensity for widespread crack propagation of a given snowpack/PST" suggests that one can only get this information through toughness. You would essentially get the same type of information by looking at stress and strength. Additionally, this would make a better link to Figure 5 (DEM comparison), which is the only one with stress instead of energy... I think this would be a very valuable and interesting addition.*

105 Thank you for bringing up this important point. It touches upon a common misunderstanding concerning stress behavior at the tips of sharp notches (e.g., V-notches, crack tips, anti-crack tips, etc.). According to elastic continuum mechanics, stresses at these locations are theoretically infinite. However, many models yield finite stress values because they lack the necessary resolution to accurately capture the stress singularity at the crack tip.

110 In weak-interface models like ours, the weak layer of finite thickness is considered as a whole, and the crack itself is not explicitly resolved. In finite-element analyses, the stress at the notch tip increases as the element size decreases around the notch tip, regardless of how small the elements are chosen. This is due to the polynomial basis functions used in these elements. Similarly, discrete-element models employ elements of finite size whose geometries differ significantly from the microstructure of a weak layer. Inter-particle interactions are modeled using spring-type connections between the particles' centers of mass, and decreasing the particle size leads to increased observed stresses.

115 Consequently, the stresses observed at crack tips are influenced by the chosen characteristic length scale (element size, particle size, etc.). While the stress solutions provided by these models are generally correct, they are not accurate in the immediate vicinity of a notch. Therefore, it is not physically appropriate to use such stress values for stability assessments.

To illustrate this, consider two weak layers with identical elastic properties (Young's modulus, Poisson's ratio) and the same cut length but different thicknesses. Although the crack-tip stresses in the thicker weak layer are smaller, its energy release rate is higher, making it the weaker layer! This phenomenon has been demonstrated both theoretically and experimentally, for instance, in studies of adhesive layers, which share similar geometric and fracture characteristics with weak snow layers in terms of elastic contrast and failure localization Stein et al. (2015).

Remark 2.5 *Another important aspect is the lack of discussion regarding the model's relevance to real-scale avalanches. The paper focuses on arrest conditions at the PST scale, not the slope scale. Although arrest can indeed be induced by slab and weak layer properties at the PST scale, as demonstrated here and by others, once crack propagation occurs at the slope scale, touch-down distance does not have the same impact (it actually becomes much larger in a dynamic setting). Arrest at this stage is likely driven by slab tensile failure, spatial variability (see Meloche et al. <https://arxiv.org/abs/2406.01360>), and topography (Gaume et al. 2019 Cold Reg.). This scale is reflected very well in the title of the paper, but the text needs further elaboration in the introduction and discussion sections in my opinion.*

130 We have revised the manuscript to more clearly define the scope of this study in both the Introduction and Discussion sections.

Remark 2.6 *Crack arrest explanation: Moreover, the current paper does not completely explain why crack arrest occurs; it suggests that it could be related to G_c/G_s , but the ratios remain below one, meaning energy release still exceeds toughness. This is fine, but I think it should be further discussed. Additional research is needed to fully understand this process, especially at the slope scale, where other factors like slab tensile failure, spatial variability, and topography play crucial roles.*

135 We completely agree with this observation. While we aimed to uncover a conclusive explanation for why crack arrest occurs, our findings only provided preliminary insights. This underscores the need for further studies, which we have now emphasized in the revised Discussion section.

Remark 2.7 *Frictional sliding: It is both surprising and a bit disappointing that the current study does not account for frictional sliding, an essential factor, particularly for cases on steep slopes. Why was this omitted? Including friction seems quite straightforward and would provide a more comprehensive understanding of the crack arrest phenomena. I understand that at a scale of a classical PST (1m) it won't affect the results much, but at the larger scale, it will have crucial consequences on the results. There must be a clear and objective rationale for excluding it, though I find it difficult to grasp the complexity behind this decision, especially since similar modeling has included friction before. How complex is it to add the friction term into the equations?*

145 Incorporating friction into the model introduces physical nonlinearity, necessitating an iterative solution scheme. While this is beyond the scope of the current study, we plan to include it in a future iteration of the model.

Remark 2.8 *Equations: I suggest adding more details on the stress calculations.*

The limited emphasis on stresses was a deliberate choice. As explained in our response to R2.4, stresses are not directly relevant in the context of crack propagation problems, such as those studied in propagation saw tests.

150 **Remark 2.9** *Experimental validation: At the moment, the validation is performed using DEM and FEM, and the model is then applied to experimental cases. I wonder if it would be feasible to extend the validation to a much broader database of PSTs, which include snowpack information and PST outcomes, even if this validation is at a lower level of detail. One does not always have such precise data as those presented here, but I still think such data could give important general insights into the model applicability. For instance, one could use a threshold-based approach to check whether you are able to "predict" ARR and END cases.*

155 Based on your suggestion, we analyzed all PSTs with complete snow profiles available in the SnowPilot database (2500 data points). However, we identified a significant bias in PST datasets: nearly all PSTs in the database are 1 m long, which means crack arrest is only observed for PSTs with very short cut lengths, i.e., when the weak-layer fracture toughness is exceptionally low. This introduces an unrealistic bias, and a substantial number of longer PSTs would be required to draw reliable conclusions from the proposed analysis.

Remark 2.10 *Suggested limitations and outlook section: A section on limitations and outlook would greatly enhance the paper. It could include topics such as the effects of slab fracture (which could be modeled similarly to Benedetti but here including layering model) and discuss skier triggering, spatial variability, and slab fracture dynamics, especially since slab fracture may be difficult to observe in PSTs.*

165 Slab fracture can be calculated using the stress solutions provided in Weißgraeber and Rosendahl (2023). However, this requires careful consideration, as slab fractures can initiate either from the top or the bottom of the slab.

170 Skier triggering is a nucleation phenomenon governed simultaneously by stress and toughness, necessitating a dedicated modeling approach. In this context, slab touchdown plays a significant role in large-scale crack propagation but is less critical during the initiation phase. To address this in the future, we are developing an experimental setup to validate skier-triggering models against empirical observations. However, this effort is beyond the scope of the current paper.

The characteristics of spatial variability discussed in prior publications also apply here. Unfortunately, we do not have touchdown PST data available to provide additional insights into this aspect.

References

- 175 Stein, N., Weißgraeber, P., and Becker, W.: A model for brittle failure in adhesive lap joints of arbitrary joint configuration, *Composite Structures*, 133, 707–718, 2015.
- Weißgraeber, P. and Rosendahl, P. L.: A closed-form model for layered snow slabs, *The Cryosphere*, 17, 1475–1496, <https://doi.org/10.5194/tc-17-1475-2023>, <https://tc.copernicus.org/articles/17/1475/2023/>, 2023.