Responses to CC on manuscript EGUSPHERE-2026-733

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Please Note that the following sections "General comment on public discussion" and "Corrigendum" are repeated in all answers to referees and community comments.

General comment on public discussion We are grateful to see that this topic has generated such interest and a lively scientific debate, highlighting both the relevance of the subject and the remaining challenges. We sincerely thank all contributors who engaged with the study and provided comments. We particularly appreciate how these exchanges have gathered rich scientific discussions by confronting viewpoints emerging from different methodological and modelling frameworks, whether based on Linear Elastic Fracture Mechanics (LEFM) or cohesive zone modelling, as well as from mechanical modelling to more macroscopic snowpack modelling designed for operational applications.

Our aim was to propose and evaluate, using pre-existing datasets, a parameterization of fracture energy, a weakly constrained parameter that remains difficult to measure experimentally, required by energy-based mechanical model to assess the cut length required for the onset of crack propagation within a weak layer in a snowpack profile, in a PST-like configuration. An additional key constraint was that this parameterization should remain applicable within a macroscopic snow model, with the perspective of enabling both climate-oriented applications (large spatial and temporal scales) and operational forecasting purposes.

Ultimately, all comments were carefully reviewed in detail, and most suggestions have been incorporated with the objective of improving the quality and clarity of the originally submitted manuscript. In the following, we prioritize detailed responses to the two referees. While several points raised in the community comments overlap with referee remarks, we address them primarily within the referee responses and complement them in the replies to community comments if necessary.

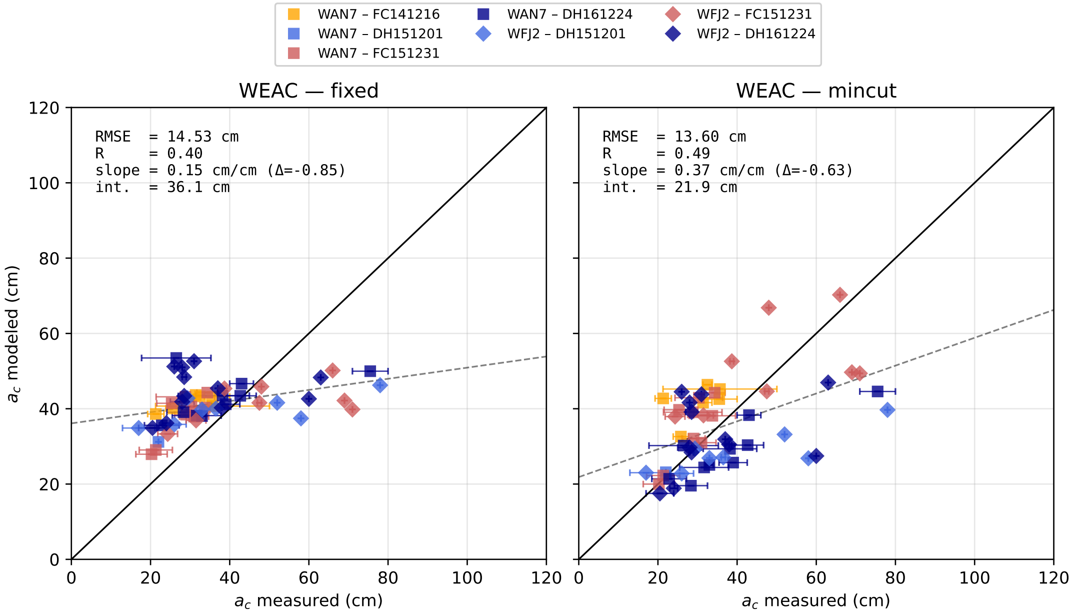
For clarity, please note that reviewers comments appear in dark blue, whilst authors responses are in black with added sentences in the revised manuscript in italic in the following document.

Corrigendum

The comments from the reviewers and public discussion helped us identify an error affecting the scores reported in Figure 7. In the model evaluation based on measured snow profiles, we used by mistake modelled weak-layer density values instead of the corresponding measured weak-layer densities. This mistake led to artificially improved model scores, which decrease when the measured weak-layer densities are used consistently. The revised manuscript has been corrected accordingly, and the associated results and figures have been updated. The new figure 7 is displayed above on Figure 1).

Figure 1 corresponds to the revised version of the figure presenting a_c modeled versus measured displayed with a new slope scores.

a) Measured profiles



b) Simulated profiles (Crocus)

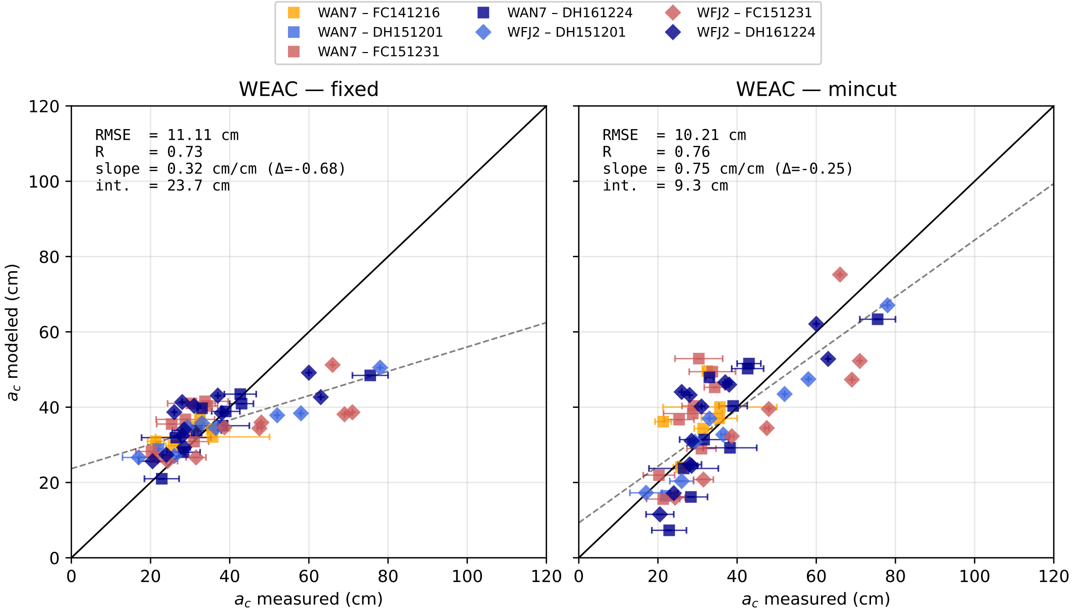


FIGURE 1 – Comparison of the measured and predicted critical cut length a_c for a weak layer fracture energy w_f either constant or parameterized using the min-cut derived parameterization Eq. XX. The comparison is performed using input profiles observed (a) or simulated with Crocus snow model (b). The WEAC model is used to derive a_c. Uncertainties in measured critical cut length are displayed as horizontal error bars, vertical error bars corresponding to the range of a_c values obtained using the intervals of sphericity and SSA (reported in table XX in appendix) assigned to observed weak layers.

Community comments : Ron Simenhois

1 Overall assessment

As an operational avalanche forecaster and part-time researcher with some experience in snowpack modeling, avalanche release research, and avalanche hazard assessment, I offer the following perspective on this manuscript and the associated public discussion. This paper addresses a genuine and longstanding gap : the weak-layer fracture energy wf has remained a poorly constrained free parameter in crack-propagation models, typically back-calculated from PST data rather than independently estimated from snowpack state variables. The proposed parameterization, linking wf to density, grain sphericity, and SSA through a min-cut proxy derived from X-ray microtomography, is a meaningful step toward making crack propagation indicators fully prognostic from snowpack model outputs. From a practitioner's standpoint, programs already running Crocus or SNOWPACK operationally can implement this with modest effort and obtain more physically consistent crack-propagation estimates than a constant wf provides. That is a real and useful contribution. Beyond its immediate application to crack-propagation onset, the continuously applicable wf parameterization developed here serves as a foundational input for future work on dynamic crack propagation and fracture arrest at the slope scale. Coupling spatially varying wf fields derived from distributed snowpack simulations to dynamic propagation frameworks would be a natural and productive next step, crucial to operational avalanche programs, and this paper removes a key obstacle to that work.

We would like to thank Ron Simenhois for taking an interest in and reading the manuscript in detail. We appreciate his contribution to the scientific debate sparked by the article through open discussion, and thank him for his positive assessment of our work, particularly his recognition of its usefulness in the operational context of avalanche hazard forecasting.

2 On the nature of ac

The public discussion has generated considerable debate over whether the critical crack length ac is a valid indicator of crack-propagation propensity. We want to be precise here. Calling ac a "geometrical measure" understates its physical content ; it integrates slab density, elastic modulus, height, weak layer fracture energy, slope angle, and cutting direction through a mechanical model. It is more accurately described as a system property : a configuration-dependent indicator of the onset of propagation for a given slab-weak layer configuration. I agree with the second reviewer's framing, and the impact pressure analogy is apt. Recent work by Adam et al. (2024) and Bergfeld et al. (2025) makes explicit that PST-derived critical cut lengths are not transferable across experimental configurations and that they are most appropriately used to infer material properties, such as fracture toughness, rather than treated as portable physical properties themselves. The paper should reflect this framing consistently. That said, the configuration-dependence of ac does raise a legitimate methodological concern that the paper should address more explicitly. Comparing ac values across different snowpack configurations without adequately controlling for the dominant non-wf drivers, primarily slab density and height, makes it difficult to isolate the contribution of the wf parameterization. The slab stiffness gradient with depth dominates the profile-wide ac signal in Figure 9, and the cross-sectional comparison in Figure 7 conflates variance from slab properties with variance from wf. The more controlled and compelling validation is actually the temporal evolution analysis in Figure 8, where the same weak layer is tracked through a season with consistently modeled slab evolution. This result deserves greater prominence : it is both scientifically cleaner and directly relevant to operational use, where practitioners need to track weak-layer stabilization over a season.

This comment is directly related to comment no. 1 from reviewer P. Rosendahl, it has already been adressed in reply to comment no. 1 of P. Rosendahl, please refer to it for details.

3 On the completeness of the physical framework

The first public comment implies that the absence of a coupled stress-energy initiation criterion represents a fundamental conceptual gap. I disagree with this framing. Avalanche release is a chain of distinct necessary conditions : failure initiation, onset of crack propagation, dynamic crack propagation across the slope, crown fracture, and slab sliding. This paper addresses one link in that chain. No single contribution is expected to resolve the full sequence, and evaluating this paper against that standard is not appropriate. The relevant avalanche mechanics literature, including Reuter and Schweizer (2018), has explicitly separated failure initiation from crack propagation propensity precisely because these are distinct problems requiring distinct treatments. It is equally important to distinguish the onset of crack propagation, which this paper models, from sustained dynamic propagation and fracture arrest, which are mechanically distinct subsequent stages. Bergfeld et al. (2023) separately quantify dynamic fracture and compaction following onset, while Rosendahl et al. (2025) show that slab touchdown can reduce the energy release rate after onset and contribute to crack arrest. The paper's title appropriately limits scope to the onset, but several passages broaden the interpretation toward general crack-propagation propensity and operational hazard monitoring. The claims should be narrowed accordingly : this is a promising parameterization for onset models, not yet a complete description of crack propagation or release probability. The practical implication is not that the paper's output is invalid ; rather, it is that ac should be interpreted in combination with failure initiation indicators, which is already standard practice in operational programs that use multiple stability indices. A weak layer with low ac but low failure initiation likelihood is not operationally dangerous, and practitioners understand this. The paper acknowledges this briefly but could be more explicit about it as an operational caveat.

This comment is directly related to comment no. 2 from reviewer P. Rosendahl. We already reply to it in our answer to P. Rosendahl, please refer to it for details.

4 Scientific concerns worth addressing

Setting aside the conceptual debate, several methodological concerns merit attention. The most significant is the potential circularity between calibration and evaluation. The Richter et al. (2019) PST dataset is used both to establish the linear wf-to-min-cut relationship and to evaluate how well the resulting parameterization predicts ac. The demonstrated improvement over a constant wf is at least partially guaranteed by construction. A leave-one-weak-layer-out, leave-one-season-out, or leave-one-site-out cross-validation would materially strengthen the validation. This matters especially because the performance gain in the operationally relevant Crocus case, the configuration closest to actual forecasting use, is modest : R = 0.76 versus 0.73 and RMSE 10.1 versus 11.1 cm compared to constant wf. The larger gains appear in the observed-profile case, which is less operationally relevant. The paper should be transparent about this asymmetry rather than leading with the more favorable numbers.

This comment is directly related to comment no. 5 from reviewer P. Rosendahl, it has already been adressed in reply to comment no. 5 of P. Rosendahl, please refer to it for details. Furthermore, the wording of the abstract and main conclusion has been amended to better reflect the true added value of our parameterisation using both the measured and modelled snow profiles.

The question of whether the min-cut is doing genuinely independent work beyond density deserves a direct test. The paper itself acknowledges that density is the primary driver of min-cut, and density also enters the WEAC model through the Gerling elastic modulus formulation at a steep power law (^{5}.13). Without a density-only baseline, and ideally a density-plus-discrete-grain-type baseline, it is impossible to quantify the contribution of the morphology term, as measured by sphericity and SSA. The current comparison with only a constant wf is necessary but insufficient to support the paper's main claim.

This comment is directly related to comment no. 5 from reviewer P. Rosendahl, it has already been adressed in reply to comment no.5 of P. Rosendahl, please refer to it for details. In brief, using the mincut as an intermediate variable between the layer density ϕ + grain morphology (SSA, sphericity) and the pseudo-measured weak layer fracture energy w_f reduces the dimensionality of the initial problem (useful because the number of measured w_f remains small, and we only fit one parameter in the linear relationship between the mincut and w_f) and provides pathways to extend the proposed parameterization to other snow types (indeed the PST data only includes DH and FC weak layers, while the relation between density + grain morphology to mincut is obtained on a larger tomographic dataset).

A related concern is mode mixity. The manuscript states that WEAC accounts for mixed-mode conditions, but the inversion setup uses upslope PST cuts, no touchdown, and a fixed weak layer height of 3 cm. Adam et al. (2024) show that fracture toughness is substantially larger in shear than in collapse, and that the historical PST literature contains almost no mode-II-rich data. As currently formulated, wf looks more like an effective PST-based fracture parameter for a mode-I-dominated loading regime than a general weak-layer fracture energy law. The authors should report the mode-I/mode-II share of the energy release rate for the inverted WEAC cases and explain explicitly why a scalar wf is acceptable for this dataset and its intended application.

This comment is directly related to comment no. 3 from reviewer P. Rosendahl, it has already been adressed in reply to comment no. 3 of P. Rosendahl, please refer to it for details. We have now clarified (title + text) that w_f is mode-I clearly dominated. In addition, we discuss that the variation of fracture energy for different mode mixity relevant for avalanches and PSTs appears small compared to the one related to different weak layers.

A more technical concern relates to the fitted percolation threshold $\phi_t = 0.04$, corresponding to a density of approximately 37 $kg\ m^3$. The dataset does not include samples with $\phi <$ approximately 0.08 (75 $kg\ m^3$ for precipitation particles), so this parameter is extrapolated below the calibration data range. The physical interpretation of a percolation threshold for ice bond network connectivity in very low-density fresh snow is not straightforward, and to my knowledge, the appropriate literature, modern snow micro-tomography work on structural connectivity, does not clearly support a value this low. The sensitivity of the parameterization to this fitted threshold should be reported, and its physical basis discussed more carefully.

We agree with the reviewer that the fitted percolation threshold (ϕ_t=0.04) can be considered relatively low and that it lies outside the range of densities represented in our calibration dataset. To the best of our knowledge, there is currently no established reference value for a percolation threshold specifically associated with the min-cut in snow microstructures. In our interpretation, such a threshold would correspond to a state where the ice-bond network becomes effectively disconnected across the min-cut surface, implying a min-cut of 0. From a physical perspective, this situation appears unlikely as long as a coherent snow microstructure still exists.

Importantly, natural snow layers with very low densities, approaching or even below 40 $kg\ m^3$, have been reported in the literature, particularly for fresh precipitation particles and low-density new snow (e.g., freshly fallen dendritic snow can exhibit densities on the order of 30-40 $kg\ m^3$; (Helfricht et al., 2018)). Therefore, while the fitted threshold lies below the calibration range of our dataset, it remains within the broader range of physically observed snow densities.

In the absence of stronger physical constraints from the literature, we chose to treat ϕ_t as a free fitting parameter, constrained only to remain within a physically realistic range for snow densities. Under this framework, ϕ_t=0.04 corresponds to the parameter value yielding the best minimization of the cost function. While this constitutes an extrapolation relative to the available data, we do not consider it physically unrealistic based on the arguments above.

We further note that the practical impact of this threshold on the resulting modelled w_f values remains relatively limited. Increasing the threshold (e.g., to 0.06 or 0.08) primarily degrades the quality of the fit for low-ϕ values, and consequently for low min-cut values but not for w_f or a_c.

Nevertheless, the fitting procedure has been partially revised. In the original manuscript, the fit relied on ordinary least squares, which implicitly placed substantially lower weight on low min-cut values. The revised fit now better accounts for this issue, and additional discussion regarding the fitted parameter values and their interpretation has been added to the manuscript.

In the discussion section 4.1, we have added the following paragraph : *"The fitting procedure, a weighted least squares, yields a percolation threshold* $\phi_t = 0.04$*, corresponding to a snow density of approximately*

$35\,\mathrm{kg\,m^{-3}}$. This value lies slightly below the range of the snow dataset used, yet remains close to the density range of naturally deposited fresh snow (30 to $40\,\mathrm{kg\,m^{-3}}$, Helfricht et al. 2018), for which a non-zero min-cut is expected."

The finding that modeled Crocus profiles outperform directly observed profiles is operationally encouraging but deserves careful interpretation alongside the known density bias in Crocus (systematic underestimation of slab density, RMSE 58.8 $kg\ m^3$). The authors suggest this reflects measurement noise in density cutters, which is plausible. However, it also reflects that the parameterization was calibrated within Crocus's representational space, where density, SSA, and sphericity evolve with internal consistency. The possibility that the parameterization is better tuned to the model world than to physical reality should be acknowledged explicitly.

We agree with the reviewer that the improved performance obtained using Crocus-modelled profiles should be interpreted cautiously, particularly in light of the documented systematic underestimation of slab density by Crocus. We also agree that internal consistency between prognostic variables within the model likely contributes to the improved performance.

However, we would like to nuance the interpretation that the parameterization is primarily over-calibrated to the Crocus representational space, at least with respect to density. The calibration linking min-cut to density and microstructural descriptors was performed using densities measured from X-ray tomography, rather than Crocus-modelled densities. Therefore, the density dependence of the parameterization is not directly inherited from Crocus. Nonetheless, it is true that the SSA and sphericity relationships are derived from Crocus grain-type representations. In practice, however, these variables mainly influence the inflection and modulation of the fit, rather than the overall density-driven structure of the parameterization itself. Nevertheless, we agree that this point deserves explicit discussion. As suggested by the reviewer, we have clarified in the revised manuscript that transferring this parameterization to other snowpack models would likely require additional tuning to account for differences in prognostic variables, and the plausible ranges of snow properties represented within a given model.

Accordingly, we added and modified a paragraph in discussion section 4.1 : *"It should also be noted that the characterisation of different min-cut between grain types (for a given density) is captured in the fit using prognostic variables specific to the Crocus model, namely SSA and sphericity. This part of the parameterisation is therefore model-dependent and would need to be recalibrated using the corresponding variables and their associated value ranges for any other snowpack model to which it is transferred. The power-law component (i.e. density dependency), by contrast, is model-independent. Ultimately, this parameterisation may be applied consistently across all layers of a snow profile (whether field-measured or simulated with Crocus) to compute w_f. This value can then be incorporated into a slab model to estimate the critical cut length."*

5 From a practitioner and operational perspective

Both Crocus and SNOWPACK have known limitations in representing actual snowpack states, density biases, sensitivity of weak layer formation to temperature-gradient parameterization, and layering resolution constraints. These challenges are larger in many operational contexts than the specific gap this paper addresses. But that context favors this paper's contribution : it removes one source of indeterminacy in the crack-propagation modeling chain without requiring new field measurements or model restructuring. The validation dataset being drawn entirely from two instrumented research sites 3 km apart above Davos, dominated by DH and FC grain types in an Alpine climate regime, limits statements about portability. The single surface hoar layer in the dataset was not reproduced by Crocus, and Adam et al. (2024) derived their mixed-mode interaction law specifically on surface hoar weak layers, while Schöttner et al. (2026) report distinct scaling behavior across FC/DH, DF/RG, and surface hoar categories. What the paper supports most defensibly is an effective parameterization for PST-onset calculations in persistent FC/DH-type weak layers under the specific modeling assumptions used. The conclusions should reflect this scope rather than claiming generality across snow fracture energy broadly. Maritime snowpacks and thin continental snowpacks are unrepresented, and portability to other climate regimes is an important open question that should be stated explicitly as a priority for future work.

This comment is directly related to comment no. 7 from reviewer P. Rosendahl, it has already been adressed and taken into account in reply to comment no. 7 of P. Rosendahl, please refer to it for details.

The following paragraph has been added in a new discussion section : *" We also note that the PST dataset covers only a limited range of weak layer types, namely faceted crystals (FC) and depth hoar (DH). The fact that surface hoar (SH) is not represented in Crocus limits the validation of the parameterization for this grain type, and no weak layers composed of fresh snow, such as precipitation particles (PP) or decomposing and fragmented particles (DF), are included in the dataset. It should further be noted that the two study sites, located approximately 3 km apart in Switzerland, are representative of Alpine snowpacks, which may differ substantially from other snowpack types, such as oceanic snowpacks found in parts of the Rocky Mountains of North America, or continental and Arctic snowpacks characteristic of higher-latitude environments."*

A subtler but equally important limitation is that neither of these study sites, nor most sites where operational snowpack models are routinely validated, are located in actual avalanche start zones. Snowpack models are typically run at weather station sites on flat or low-angle terrain, and their outputs are extrapolated to release zone terrain. The snowpack in a 35-40ř start zone differs from a research plot in its wind redistribution history, slope-parallel settlement, radiation geometry, and spatial variability. Whether the parameterization behaves consistently when applied to start zone terrain, where the slab-weak layer system properties are what actually matter for avalanche release, has not been investigated. This is a recognized gap across the broader snowpack modeling literature, not specific to this paper, but it deserves explicit acknowledgment in the context of operational claims.

This comment is in line with the previous one and with comment no. 7 by reviewer P. Rosendahl. It once again highlights significant limitations, which we acknowledge, and helps identify interesting directions for future work. These have been incorporated, along with the previous remarks, into a new "Discussion" section devoted to the limitations inherent in the article, as well as to the various perspectives and challenges that remain.

As a consequence, this paragraph has been added in a new discussion section : *"Nonetheless, the snowpack in a 30 to 40°actual avalanche start zone may differ from a research site in terms of snow stratigraphy (i.e. due to wind redistribution, settlement, radiation, and spatial variability). This represents a limitation, although it is common in most research on snowpack stability, due to the difficult access and monitoring conditions in these areas."*

6 Conclusion

This paper makes a genuine and implementable contribution to operational avalanche forecasting. It narrows a specific gap, the wf free parameter problem, that has limited the utility of crack propagation indicators in operational snowpack modeling. The scope is best understood as an effective parameterization for PST-based onset calculations in persistent FC/DH weak layers, rather than a general snow fracture energy law, and the conclusions and title should reflect this more carefully. The public debate around the paper has, in places, conflated legitimate methodological concerns with broader disciplinary arguments about fracture mechanics frameworks that do not directly bear on the paper's contribution. The second reviewer's careful response to these comments is well-reasoned, and I support its framing. With revisions addressing the calibration circularity, the density-alone and density-plus-grain-type baselines, the mode mixity characterization, the percolation threshold extrapolation, the modest performance gain in the operationally relevant Crocus case, and clearer framing of wf as an effective PST-based parameter rather than an intrinsic material property, this paper would represent a meaningful and operationally relevant advance.

We would like to thank Ron Simenhois once again for his positive feedback on our work. All comments have been addressed above, either directly or indirectly through our response to the reviewers comments.

Références

Helfricht K, Hartl L, Koch R, Marty C, Olefs M (2018) Obtaining sub-daily new snow density from automated measurements in high mountain regions. Hydrology and Earth System Sciences 22(5) :2655–2668, DOI 10.5194/hess-22-2655-2018, URL `https://hess.copernicus.org/articles/22/2655/2018/`