Responses to RC 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.

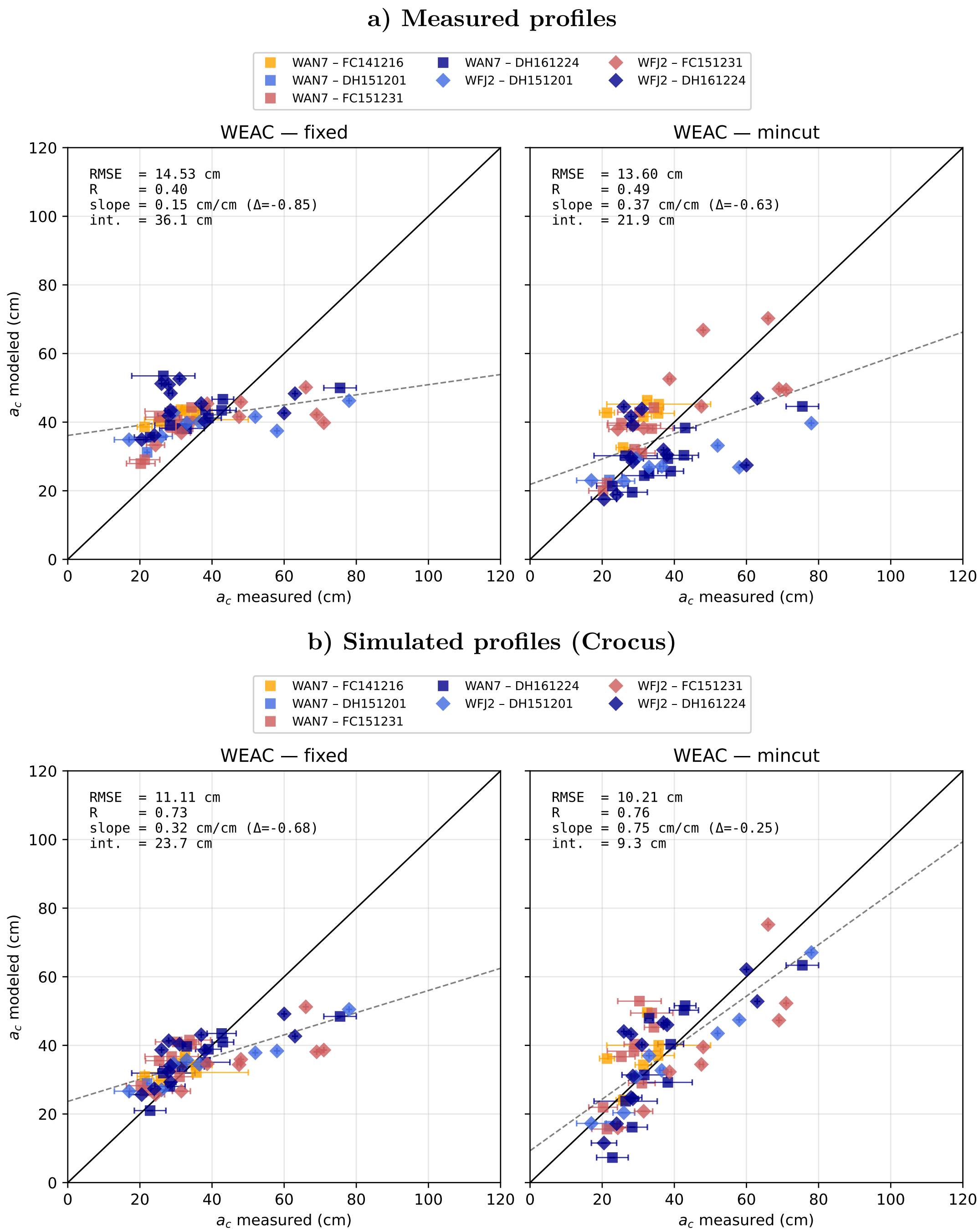

FIGURE 1 – Comparison of the measured and predicted critical cut length a_c for a weak layer fracture energy w_f either constant or parameterized using the min-cut derived parameterization Eq. XX. The comparison is performed using input profiles observed (a) or simulated with Crocus snow model (b). The 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.

Referee comments : Philipp Rosendahl

This manuscript addresses an important and operationally relevant problem : how to parameterize weak-layer fracture energy for snowpack models from variables that are actually available in Crocus-like systems. I find the overall idea promising. The strongest aspect of the study is the multi-scale workflow : the authors start from min-cut calculations on 271 muCT snow images, derive a densitymorphology parameterization, map this to weak-layer fracture energy through WEAC inversions of PST data, and then test the resulting formulation in both WEAC and a simplified Heierli-type model. This is a sensible and practically useful direction. The reported improvement over a constant wf is real, especially for observed profiles, and the manuscript benefits from explicit reproducibility efforts through shared data/code. I also view the use of WEAC as methodologically appropriate. WeiSSgraeber and Rosendahl (2023) present WEAC as a closed-form layered-slab mechanics framework that delivers slab deformations, weak-layer stresses, and energy release rates in real time, while explicitly not making it itself a crack-growth criterion. In that sense, using WEAC here as an inversion/forward mechanics engine is sound. My main concerns are how strongly the manuscript interprets the resulting parameterization as a physically established fracture law, and how it interprets PST-derived critical cut lengths in light of the recent fracture-mechanics literature.

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We would like to thank Philipp Rosendahl for his thorough review and positive appreciation of the article. In the following section, we provide a point-by-point response to each of his comments.

1 Critical cut length should be treated more carefully as a system response, not as a material property.

Recent work makes this point very clearly. Adam et al. (2024) state that the critical cut length ac marks the onset of unstable crack growth, but it must not be misinterpreted as a material property because it depends on cutting direction, slope angle, slab layering, and load. Bergfeld et al. (2025) further show that PST geometry alone can change measured critical cut lengths substantially, with slope-normal ends yielding up to 50% shorter values than vertical ends. Recent ISSW guidance by Rosendahl et. al (2024) makes the same point even more directly : the measured critical cut length alone does not allow comparison across different experimental conditions and should be used to derive fracture toughness (not vice versa) if comparability is desired. Against that background, I encourage the authors to consistently frame ac as a configuration-dependent system quantity. Within one standardized setup it is a legitimate model target, but it is not itself the weak-layer fracture resistance.

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We fully agree that the critical cut length a_c derived from a propagation saw test should not be interpreted as a material property of the weak layer. It depends on both material and geometric factors, including slab stratification, slope inclination, loading conditions, and cutting direction. Our intention was not to interpret a_c as an intrinsic property of the weak layer, and we are sorry if some statements in the manuscript sounds misleading. In particular, we agree that the statement "A small a_c means that the weak layer is prone to crack propagation" is too vague and may lead to incorrect interpretation, as a_c reflects a system property, and additionally should be precise to characterize the onset only and not the dynamic crack propagation process. Following the reviewers suggestion, the sentence has been removed and we revised the manuscript at multiple places to state more consistently that a_c is a configuration-dependent indicator of crack propagation onset. Please refer to the Specific remarks section, which includes the new sentences and corrections.

2 The manuscript should more clearly separate onset of propagation from sustained propagation, crack arrest, and avalanche release.

The title is appropriately limited to the onset of crack propagation, and that focus is scientifically defensible. However, several passages broaden the interpretation toward general crack propagation propensity and operational hazard monitoring. Literature shows that these are not the same problem. Bergfeld et al. (2023) distinguish onset from the subsequent dynamic propagation phase and separately quantify dynamic fracture and compaction. Rosendahl et al. (2025) then show that slab touchdown can reduce the energy release rate after onset and contribute to crack arrest, so onset and sustained self-propagation are mechanically distinct stages. In my view, the paper would be stronger if the claims were narrowed accordingly : this is a promising parameterization for onset models, but not yet a complete description of crack propagation or release probability.

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The different key mechanical stages of an avalanche release are described in the introduction (l. 37-38), including dynamic propagation. The title clearly specifies the scope of the paper with "onset of crack propagation" which cannot be confused with dynamic propagation. However, to avoid any potential confusion raised by the referee, we will specify everywhere in the text (abstract, introduction, discussion) that we are focusing on the onset of crack propagation and avoid using the ambiguous "crack propagation propensity" wording.

3 The recent mixed-mode literature raises an important limitation of the scalar wf formulation used here.

The manuscript states that WEAC accounts for mixed-mode conditions, but in practice the inversion setup uses upslope PST cuts, no touchdown, and a fixed weak-layer height of 3 cm. Adam et al. (2024) provide a first mixed-mode anticrack interaction law and show that fracture toughness is significantly larger in shear than in collapse. They also note that the historical PST literature contains almost no mode-II-rich data. Conference work by Walet et al. (2024) and Rheinschmidt et al. (2024) points in the same direction : accurate fracture-toughness estimates require better elastic-property retrieval, and fracture resistance is mode-dependent, potentially including mode III for cross-slope propagation. I therefore think the authors should explicitly discuss what their scalar wf represents. As currently formulated, it looks more like an effective PST-based fracture parameter for a limited loading regime than a general weak-layer fracture-energy law. At minimum, the authors should report the mode mixity of their inverted cases and explain why a scalar wf is acceptable for this dataset.

**

We agree that the scalar w_f used in the present study and retrieved by WEAC model inversion using observed and modeled profiles along with PST measurements should not be interpreted as a general law applicable across all loading conditions. Rather, within the framework of our dataset and inversion setup, it is more appropriate to interpret it as an effective PST-based fracture parameter representative of a restricted loading regime, predominantly dominated by mode-I.

Indeed, all propagation saw tests considered in this study were conducted on flat terrain, which strongly limits the contribution of shear loading and results in loading conditions that are largely dominated by mode I. We quantified with WEAC the mode mixity for all inverted PSTs (Fig. 2) : the considered PSTs are dominated by mode I, with contributions typically ranging between 85 and 100% (one exception at 77%). In addition, it has to be noted that on slopes between 0 and 45 degrees, where most avalanches are typically triggered (including "remotely") (Schweizer et al., 2003), the up-slope and down-slope onsets (again, we speak here about the onset not dynamic crack propagation) of crack propagation are dominated by mode I (approx. > 75%, (Adam et al., 2024)). Moreover, (Adam et al., 2024) showed that the difference in fracture toughness between mode I and II remains limited (difference around 35%). This is relatively small compared to the differences of toughness between weak layers (more than a factor of 5 in this study). To date, we cannot conclude on how the presence of mode III (cross-slope) onset of crack propagation may limit the application to avalanches of mode I dominated fracture toughness.

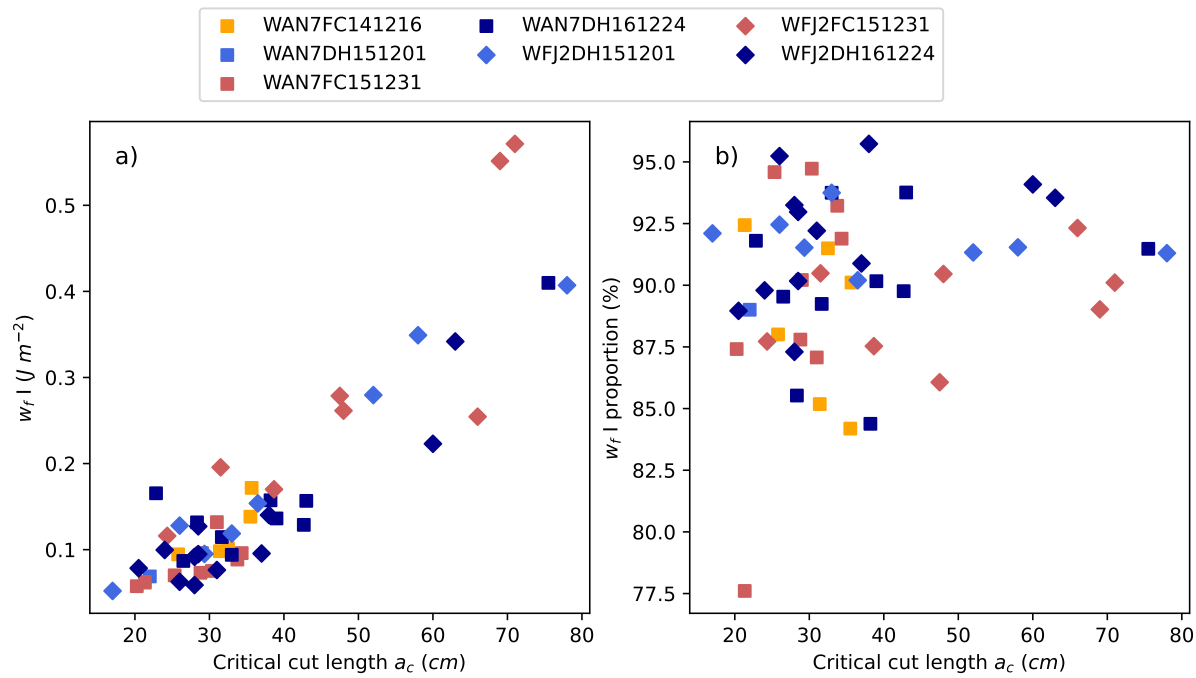

FIGURE 2 – a) Mode I w_f ($J\ m^{-2}$) as a function of critical cut length for all layers and sites. b) Proportion of mode I w_f as a function of critical cut length for all layers and sites.

Touch-down was enabled in the WEAC model, but never occurred for any of the PSTs considered in this study, as the cut lengths ranging between 20 and 90 cm remain below the touch-down distance computed from the measured or simulated stratigraphy in all cases.

Therefore, we will now clearly state that our w_f mainly quantifies mode I critical release rate and add the mode mixity of the used PSTs in appendix. However, we do not agree that this particular point is a strong limitation within the scope of the paper, since the variations of fracture energy between different weak layers are huge compared to the mode related-variations on avalanche prone configurations.

In the abstract, the following sentence has been added : *"This yields a relationship between w_f and the min-cut, calibrated for the grain types most commonly associated with dry-snow slab avalanche release (faceted crystals and depth hoar) under predominantly Mode I loading conditions."*, and in the discussion section : *"Regarding the limitations of the w_f dataset itself, it is mainly restricted to Mode-I-dominant conditions, as illustrated in Figure 2b). We note, however, that following the work of Adam et al. (2024), who performed a large number of PSTs under various geometric configurations and loading conditions, that such conditions are representative of typical avalanche-triggering configurations (including "remotely") with slope angles of 0 to 40°"*.

4 The min-cut idea is physically attractive, but the paper does not yet establish a strong physical law linking min-cut to macroscopic fracture energy.

Here I see both a strength and a limitation. The strength is that the authors do not rely on density alone, and recent work by Schöttner and co-authors strongly supports the general idea that weak-layer mechanics are shaped by microstructure in addition to density. The limitation is that the actual evidence presented here for the specific min-cut wf link remains moderate : the reported correlations between min-cut and inverted wf are only R 0.31 .. 0.36 for observed profiles and R = 0.47 for Crocus profiles. Recent mechanical studies also suggest that the relevant microstructural controls are richer than sphericity and SSA alone. In Schöttner et al. (2026), stiffness is linked primarily to anisotropy and tortuosity, while strength is more sensitive to local interface geometry ; distinct grain-type regimes are also reported. That does not invalidate the present parameterization, but it does suggest that it should be presented as a pragmatic empirical proxy rather than a physically established constitutive law for fracture energy. Moreover, since CT scans are available, I encourage the

authors to consider the recent findings on links between microscructural and macroscopic properties and look further than mincut.

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We agree that the present work do not provide physically established constitutive law for fracture energy and we didn't intended to state that. The min-cut is described as a "quantitative proxy of fracture energy" (l.10), "a lower bound for the fracture energy and thus serves as a physical proxy for wf" (l.83). We clearly state our assumptions and the scope of our parameterization : "the second assumption is that the min-cut is linearly related to fracture energy. It appears physically consistent ; at the very least, we would expect it to be a lower bound since we neglect any additional energy required to continue fracturing related to the reorganization and stabilization of layers, as well as fractures that would be suboptimal." (l. 360-363).

Besides, the goal of the paper as stated, e.g., in the title is to provide a parameterization to "model the onset of crack propagation in snowpack models". These models do not provide advanced descriptors of the snow microstructure outside of solid fraction, SSA and sphericity. It is thus worthless to use other descriptors within the paper scope. In addition, (Schöttner et al., 2026) showed that the mincut bond area (related to the mincut defined here) "emerged as the single best predictor of mechanical properties.", so we appear closely in line with the cited recent findings.

Also, introducing min-cut as an intermediate variable substantially reduces the dimensionality of the problem. Directly fitting fracture energy (inverted from PSTs) to multiple interacting variables (density, grain type, SSA, sphericity) may be sensitive to over-fitting and restrain the scope of the parameterization to the considered data (FC and DH weak layers). The min-cut associated to a large set of tomographic images provides a single physically interpretable quantity that integrates both density and morphological effects. This enables calibration using a much larger and more diverse dataset than the PST dataset alone, thereby improving robustness across a broader range of snow types and microstructures.

We agree that the correlation between the parameterized min-cut from simple microstructure descriptors (measured or modeled by Crocus) is modest (R = 0.48 at most). The presented data and recent findings on links between microscructural and macroscopic properties, or the referee comment do not provide any argument that this modest correlation is mainly related to a too rough reduction of 3D data into one microstructural descriptor. We believe that the main limitations are : a) the limited resolution of density measurement (especially for mm-cm thick weak layers), b) limited capacity of snowpack models to capture the snowpack stratigraphy at the PST sites, c) limited description of snow microstructure in snowpack models, d) the LEFM assumptions which treat the energy release rate as an intrinsic property (e.g. collapse height may depend on the overburden) and assumes a small process zone compared to the elastic characteristic length (see Johan Gaume's comments). We already discuss potential reasons for that in Sect. 4.2.

The following paragraph have been added in the discussion section : *"The parameterization of w_f using the min-cut as an intermediate variable substantially reduces the dimensionality of the problem as it integrates both density and morphological effects into a single physically interpretable quantity. This allows calibration over a much larger and more diverse dataset than the PST dataset alone, improving robustness across snow types while limiting overfitting. The w_f min-cut relationship is nonetheless validated on a restricted set of FC and DH weak layers, though its application to full stratigraphic profiles (Figure 8) yields physically consistent a_c values across layer types, suggesting reasonable broader applicability."*.

5 The validation strategy needs to be strengthened, and stronger baselines are necessary.

As I understand the workflow, the Richter PST dataset is used to invert pseudo-observed wf, then to fit the min-cutwf relation, and then again to evaluate predicted ac. This is not an independent validation. A leave-one-weak-layer-out, leave-one-season-out, or leave-one-site-out cross-validation would materially strengthen the paper. This matters especially because the performance gains are uneven : for observed profiles the improvement over constant wf is substantial, but for the Crocus case that is closest to the intended operational application, the gain is modest (R = 0.76 versus 0.73 ; RMSE 10.1 versus 11.1 cm). In addition, given the recent literature showing that density is the dominant first-order predictor of weak-layer mechanical behavior, I think the key missing baseline is a density-only wf model. Without that comparison, it is difficult to quantify the actual value added by the morphology term.

A second useful baseline would be density plus discrete grain type. The present comparison against a constant wf is necessary, but not sufficient.

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We acknowledge that the same PST dataset is used both to establish the linear relationship between min-cut and inverted w_f and to evaluate the resulting predictions. While it is true that independent validation would always be preferable, in the present case, we do not believe that the lack of a leave-one-out validation fundamentally affects the conclusions of the study. Indeed, the proposed parameterization contains very limited flexibility. The relationship between min-cut and w_f is described by a simple linear fit with only one free slope parameter. Consequently, the risk of overfitting (which is the primary motivation for leave-one-out procedures) is inherently limited compared to more complex models with many degrees of freedom. We performed a leave-one-out analysis by removing iteratively one weak layer - site data from the fitting step and see the variation of the slope (the only degree of freedom) (Tab. 1).

Table 1 – Sensitivity of the fitted min-cut–w_f relationship to the leave-one-out (LOO) procedure. LOO was performed by recursively removing one weak layer at one site for a given season. Reported values correspond to the median fitted slope together with the 10th and 90th percentiles obtained across all LOO realizations.

Dataset	Median slope	q_{10}	q_{90}
Crocus	15.28	13.74	16.50
OBS	11.26	10.43	12.20

The resulting variations in the fitted slope are small and remain below the uncertainty associated with the SSA and sphericity assumptions used in the min-cut parameterization for observed profiles. We therefore consider that a leave-one-out validation provides limited additional information in the context of the present linear parameterization. Nevertheless, following the reviewer's suggestion, we have included the corresponding leave-one-out results in the Appendix to document the robustness of the fitted relationship and refer to it in the corresponding result section.

Regarding the baseline models, we consider that adding a density-only model provides limited added value compared to the constant w_f case. Such a model is unable to discriminate within a snow profile between layers of similar density but substantially different mechanical properties, for instance depth hoar and melted forms at $250\,kg\ m^{-3}$, which would be assigned similar fracture energy values. In addition, its calibration can only be performed using the available PST dataset, which is itself restricted to pre-identified weak layers, mainly depth hoar and faceted crystals, limiting its robustness and transferability to other grain types.

In the figure below (analogous to Fig. 9 in the original manuscript), we show results from the WEAC model forced with constant w_f, w_f parameterized using min-cut, and w_f parameterized directly from the weak layer density using a linear relationship. The results show that the density-only model performs very similarly to the constant w_f case. Neither approach is able to distinguish between weak layers that are identified as mechanically critical by CT analysis and where PST measurements were performed. These observations indicate that a density-only baseline does not provide additional information compared to the constant w_f model and for these reasons, we choose to not include the density-only model as an additional baseline in the main analysis.

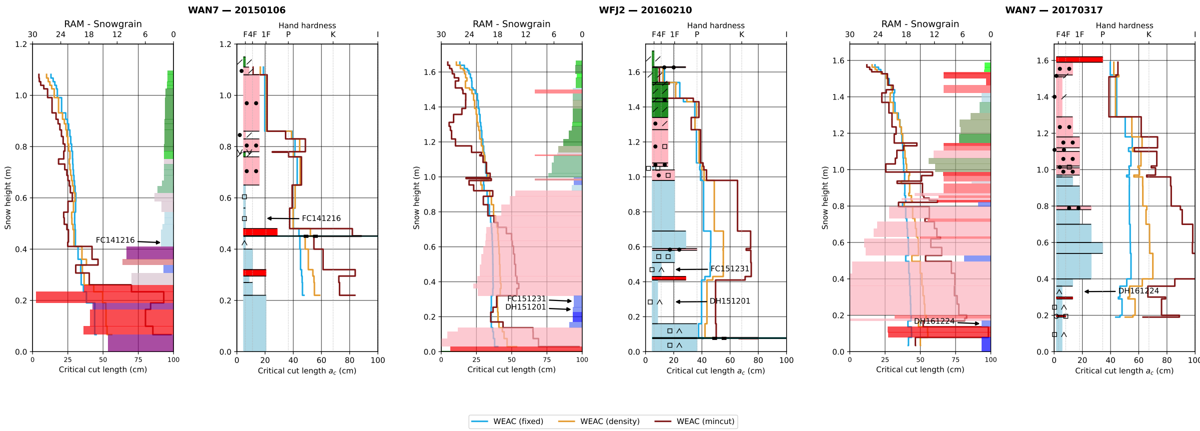

FIGURE 3 – Modeled (left) and observed (right) snow profiles for three dates at different sites : January 6, 2015, and March 17, 2017, at the Wannengrat (WAN7) site, and February 10, 2016, at the Weissflujoch (WFJ2) site. The colors indicate the grain types, and rectangle dimension, the thickness of each layer, their resistance to penetration for the modeled snow profiles, and their hand hardness for the measured snow profiles. For each layer and each profile, the critical cut length using the WEAC model, either feed with a fixed w_f (blue), density-only w_f (orange) or w_f estimated using the linear relationship with the parameterized min-cut (darkred). For each profile, the identified position of the weak layers is displayed either as measured or estimated using the deposition date in the case of modeled snow profiles as described in section **??**.

In contrast, the density-plus-grain-type baseline could be of interest. However, constructing such a model in a physically defensible manner is not straightforward within the present framework. As stated above, the PST dataset used for the w_f inversion is largely restricted to faceted crystals and depth hoar, providing insufficient information to establish grain-type-dependent w_f relationships across a broader range of snow types. Such parameterization would therefore require introducing additional assumptions or relying on external formulations. Additionnaly, this would imply the reintroduction of a discrete categorization of snow types, whereas one of the motivations for using min-cut and microstructural descriptors as intermediate variable is precisely to obtain a continuous parameterization based on measurable microstructural characteristics rather than categorical grain-type classes.

Finally, we believe that standard metrics such as RMSE and correlation only partially capture the added value of the proposed parameterization, particularly for the Crocus simulations where performance gains appear modest. To better quantify the benefits of the min-cut approach, we introduced an additional evaluation metric based on the slope of the relationship between predicted and observed a_c. This metric highlights the ability of the parameterization to reproduce the observed dynamic range of critical cut lengths, whereas both the constant w_f and density-only models tend to produce an overly restricted range of values. In addition, we have extended the comparison of the constant w_f and min-cut models in the time-series and snow-profile analyses (Figures 8 and 9 in the initial manuscript). These comparisons further illustrate that the primary added value of the min-cut parameterization lies not only in improving aggregate performance scores, but also in its ability to reproduce temporal variability and local minima in critical cut length within snow profiles.

6 The inversion is likely sensitive to elastic and geometric assumptions, and this deserves a more systematic uncertainty analysis.

The manuscript fixes several quantities that recent work suggests are influential : weak-layer height is set to 3 cm, weak-layer modulus is taken from a density-only Gerling relation, the WEAC setup assumes upslope cuts, and the Heierli implementation uses an equivalent modulus derived from the WEAC mode-I formulation. Bergfeld et al. (2023) showed that slab layering materially affects inferred stiffness, weak-layer modulus, and fracture-energy partitioning ; Adam et al. (2024) and Walet et al. (2024) likewise emphasize the importance of retrieving elastic properties more directly from experiments. I therefore encourage the authors to add a sensitivity analysis for weak-layer thickness, modulus parameterization, and touchdown assumptions, or at least to propagate those uncertainties into the inferred wf. At present, the uncertainty bands for observed profiles mainly reflect the assigned Table A1 ranges of sphericity and SSA, which are themselves taken from 5th95th percentile ranges in long-term Crocus reanalysis rather than from co-located microstructural measurements.

6 The inversion is likely sensitive to elastic and geometric assumptions, and this deserves a more systematic uncertainty analysis.

We agree that several modelling assumptions may influence the inverted w_f, including weak-layer thickness, elastic modulus parameterization, and touchdown conditions. We included a sensitivity analysis in the Appendix section to quantify the influence of these assumptions on the inferred w_f (Fig. 4). However, for clarity and readability of the main results, we chose to retain a fixed set of working assumptions throughout the manuscript, while explicitly mentioning the choices in method section and referring readers to the sensitivity analysis for details, that are also discussed below.

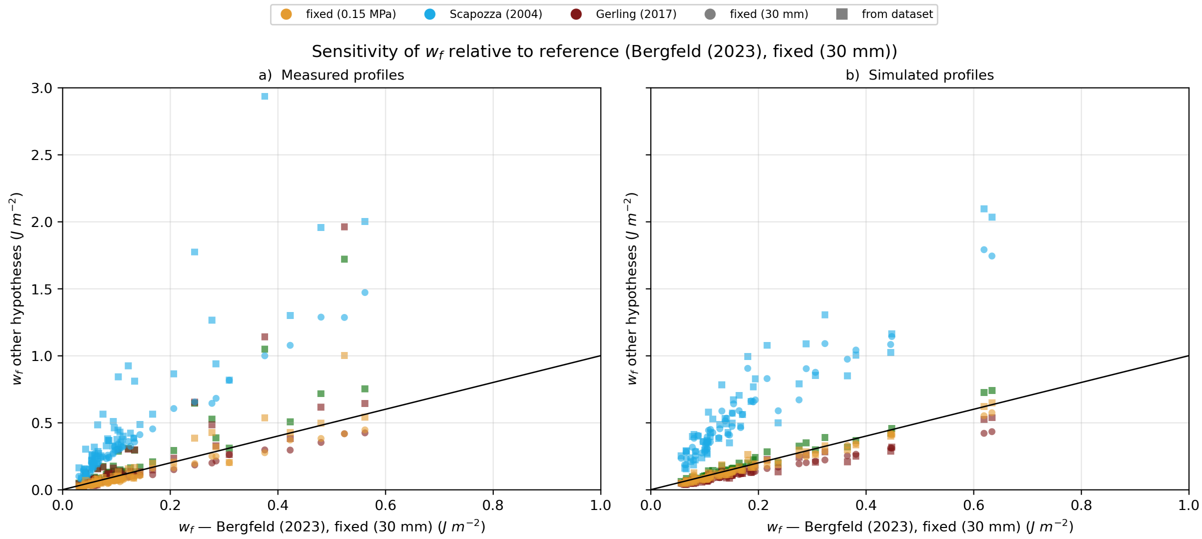

FIGURE 4 – Sensitivity analyses of w_f for measured (a) and simulated profiles (b), showing the w_f values for all layers and sites of the study under different elastic modulus parameterizations : fixed (0.15 MPa) in orange, Scapozza (2004) in blue, Gerling (2017) brown and Bergfeld (2023) in green. Weak layer thickness h_{wl} assumptions are also displayed : fixed at 30 mm (circle markers) or derived from dataset profiles (square markers). These values are plotted against the w_f values obtained from the hypotheses retained for the study, namely Bergfeld (2023) and fixed h_{wl} at 30 mm. Note that the fixed E at 0.15 MPa only applied to weak layer elastic modulus, while using this assumption slab elastic modulus is determined using Bergfeld et al. (2023) formulations.

The different assumptions were selected based on recent literature and methodological consistency considerations. Elastic modulus formulation (for slab and weak layer -except for the 0.15 MPa assumptions applying only to weak layer-) : we used the density-based parameterization proposed by Bergfeld et al. (2023), which slightly revises the formulation of Gerling et al. (2017) and is derived from acoustic-wave measurements. To the best of our knowledge, this currently represents one of the most experimentally supported density-based relationships available and the one used in study using the WEAC model (WeiSSgraeber and Rosendahl, 2023). We note that the original manuscript contained a typo error in the written equation, where the Scapozza formulation (exponent 5.13) was mistakenly reported, although the calculations were performed using the Bergfeld et al. (2023) formulation. With regard to the choice of the Mode I equivalent elastic modulus in the Heierli formulation, we believe it to be the most appropriate given that all PSTs involve predominantly Mode I loads, and that this formulation is derived from the most advanced anti-crack model (WEAC).

Weak-layer thickness : we fixed the weak-layer thickness to 3 cm to ensure methodological consistency between Crocus simulations and observed profiles. As discussed by Richter et al. (2019), layer boundaries identified in manual snow profiles are strongly observer dependent and often reflect apparent stratigraphic homogeneity rather than mechanically relevant layer thickness. As a result, manually identified weak layer thicknesses may span 5 to 10 cm (up to 30 cm), while in Crocus, layer aggregation and model resolution can similarly produce weak layers ranging from a few centimeters up to 15 cm. In both cases, it appears physically inconsistent to assume that the entire layer acts as a mechanically homogeneous weak layer. Note that this choice do not affect touchdown potential as collapse height is parameterized as $4.70 * (1 - \exp(-h_{wl}/7.78))$ mm and effectively converges toward 4.7 mm over the relevant range of weak-layer thicknesses. Moreover, as shown in the newly added sensitivity analysis Figure 4, using measured or modeled weak-layer thicknesses can leads to substantially larger inverted w_f values, often above the range commonly reported in the literature (from 0.01 to 0.7 $J\ m^{-2}$, see discussion section of the manuscript for detailed references).

Touchdown assumptions : Touchdown was enabled in the computation performed for the revised version of the manuscript, but it was never activated under the considered configurations. This result appears physically consistent with the experimental setup : the analysed PSTs are performed over 2 m columns length with cut lengths systematically shorter than 1 m, generally below 60-80 cm. This seems to be confirmed by the work done on slab touchdown effects using WEAC (Rosendahl et al., 2025) that indicates that the geometries commonly encountered in our PST experiments are insufficiently long to induce touchdown (i.e. requires at least 1 m of unsupported length to get the initial contact, see Figure 8 of the article).

For these reasons, we chose to retain the selected assumptions throughout the main manuscript while making them more explicit in the Methods section and complementing them with the newly added sensitivity analysis in the Appendix.

7 The applicability across weak-layer types should be stated more cautiously.

The validation data are dominated by faceted crystals and depth hoar, and the single surface-hoar layer in the Richter dataset was not reproduced by Crocus. At the same time, Adam et al. (2024) derived their mixed-mode interaction law on surface-hoar weak layers, while Schöttner et al. (2026) report distinct scaling behavior across FC/DH, DF/RG, and SH categories. I therefore do not think the present manuscript yet supports a broad statement about snow fracture energy in general. What it supports best, in my reading, is an effective parameterization for PST-like onset calculations in persistent FC/DH type weak layers under the specific modeling assumptions used here. The conclusions should be narrowed accordingly.

We agree with the reviewers assessment, and some formulations in the original manuscript may have suggested a broader level of generality than is currently supported by the available validation data. We agree that the present study does not establish a universal law for snow fracture energy across all snow types and loading configurations.

More precisely, the min-cut parameterization is calibrated on a large tomographic dataset gathering a wide range of snow microstructures and densities, giving us confidence that this relation is quite robust and transferable. However, the establishment and validation of the resulting linear min-cut w_f relationship through PST-derived inversions is effectively restricted to persistent weak layers, primarily faceted crystals (FC) and depth hoar (DH), under alpine conditions and within the modelling assumptions adopted in this study. We therefore agree that the proposed parameterization should be view as an effective parameterization for PST-based crack-propagation onset calculations in persistent weak layers, rather than as a fully general fracture-energy law applicable to all snow types.

Following the reviewers suggestion, we have revised the discussion and some sentence in the conclusion to narrow the scope accordingly. We now explicitly state that additional validation on independent PST datasets spanning a broader diversity of snow climates, weak-layer types, and mechanical regimes is required to assess the full transferability of the parameterization. In particular, extending the analysis to non-persistent weak layer grain type (PP, DF) and even to mechanically stable layers would provide valuable insight in future work.

The following sentences in the new discussion section have been added : *"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."*.

8 Specific remarks

8.1 When referring to the PST measurement, I suggest to consistently use critical cut length rather than critical crack length, except where the latter is explicitly defined as a modeled fracture-mechanics quantity.

The manuscript has been modified accordingly.

8.2 Please report the mode-I / mode-II share of the energy release rate for the inverted WEAC cases, not only the final scalar wf. This would greatly help readers position the results relative to Adam et al. (2024), even if dominated by mode I.

This comment is directly related to general comment no. 3 and has already been addressed above ; please refer to that section for a detailed response.

8.3 Please add density-only and density-plus-grain-type baselines, and perform held-out validation by weak layer or season. That comparison is central to the papers main claim that morphology materially improves prediction.

This comment is directly related to general comment no. 5 and has already been addressed above ; please refer to that section for a detailed response.

8.4 Title : The title currently promises a general parameterization of snow fracture energy (fracture toughness), which suggests applicability across mixed-mode fracture, compression, tension, shear, and related cases. The manuscript does not support such a broad scope. As presented, the study is limited to compression fracture toughness of weak layers. I therefore recommend a shorter title that reflects this narrower scope more accurately (see comment 2 above).

To avoid any confusion, we propose changing the title to : *"Parameterization of the snow fracture energy to model the onset of mode-I crack propagation in snowpack models"* and adding a clarification in the abstract indicating that the established w_f parameterization is established on a set of PST mainly representative of mode I.

We added a sentence in the abstract : *"This yields a relationship between w_f and the min-cut, calibrated for the grain types most commonly associated with dry-snow slab avalanche release (faceted crystals and depth hoar) under predominantly Mode I loading conditions."*.

8.5 l5 : The critical crack length measured in PSTs is specific to the PST configuration, i.e. the cut length beyond which a crack self-propagates in that particular setup. It should not be interpreted as directly transferable to skier-triggered anticracks in an effectively infinite domain. The energy release rates in these configurations differ substantially ; this can be demonstrated, for example, with the Heierli model or WEAC. Because the free boundaries in PSTs increase the energy release rate, enclosed anticracks in the same weak layer would generally require much longer critical lengths for propagation. This is a central methodological issue in the manuscript. Critical lengths are configuration- and geometry-dependent and therefore have limited predictive value on their own. For modeling and forecasting, they are better used to infer material properties such as fracture toughness, not treated as transferable physical properties themselves. By contrast, fracture toughness is a material property and is, in principle, transferable across geometries, slope inclinations, and slab configurations. This distinction should be stated clearly and introduced much more carefully.

This comment is directly related to general comment no. 1 and has already been addressed above ; please refer to that section for a detailed answer. We focused w_f (see title) which is supposed to be an intrinsic property of the weak layer in the LEFM framework. Here it mainly characterizes the mode I fracture. We may transfer w_f into an infinite domain where the mode I fracture dominates (and assuming that initial assumptions hold such as no large process zone, etc.).

8.6 l13 : The meaning of "inverting" becomes clearer later in the manuscript, but in the abstract the term is too vague. I suggest stating more explicitly what is being inverted and for what purpose.

The manuscript has been modified accordingly to improve clarity in the abstract : *"We retrieved w_f by inverting the WEAC slab model (WeiSSgraeber and Rosendahl, 2023) : we supply the model with slab characteristics that are either obtained from field measurements or from Crocus snowpack simulations and solve w_f for the given PSTderived cut lengths."*.

8.7 l46 : This statement is not correct, and the clarification given in l50 points in the opposite direction. Critical crack length depends on geometric, elastic, and bulk properties of the system and is not, by itself, a reliable predictor of crack propagation. For example, a flat slope and a 60 degree slope with the same weak layer and slab can yield very different cut lengths because the slab deformation is fundamentally different. A short cut length measured at 60 degrees does not imply easy propagation on flat terrain. I recommend revising this passage carefully and making these limitations explicit. This comment is closely related to my remark on l5.

The sentence has been removed and a paragraph added to clarify the system-dependence of the critical crack length : *"In practice, the critical crack length is system-specific and vary significantly depending on a wide range of factors, including the characteristics of the slab and the weak layer, as well as the geometric configuration of the snowpack (unbound at the sides or embedded, slope angle)."*.

8.8 l52 : The energy release rate is a system property, reflecting the change in total energy, and should not be attributed to the slab alone. I agree that beam theory can provide a useful modeling framework here, but the wording should be more precise.

We thank the reviewer for this remark and agree that the wording in the original manuscript was not sufficiently precise. In this section, our intention was to describe separately the different contributions of the total energy balance of slab models as their complexity increases. We first introduced the slab deformation and fracture-energy terms corresponding to simplified beam-theory approaches (e.g., Heierli-type models), before discussing the additional contributions accounted for in more advanced formulations, such as slab stratification, weak-layer deformation, and substratum effects.

To clarify, we revised the wording in the manuscript and added sentences at the end of the previous paragraph (related to last comment) and at the beginning of the section describing critical crack length modelling : *"Modeling of the critical crack length typically relies on linear fracture mechanics and the Griffith criterion (Griffith, 1920 ; Margolin, 1984), by performing an energy balance for the entire system (including the slab and its deformation, and potentially including that of the weak layer or even the substratum, depending on the complexity of the model used)"*.

8.9 l55 : I find this statement too strong in its current form. Whether slab compliance or weak-layer compliance dominates depends strongly on their respective elastic properties. Please qualify the statement more carefully.

This sentence does not speak about the partition between slab, weak layer and substratum energies (which has been clarified, see previous comment). Rather, we aimed to describe the main factors controlling the slab contribution to the overall system response, namely slab weight and elastic properties.

8.10 l63-l65 : This section requires much more careful framing. In its current form, the method appears insufficiently justified. For instance, how sensitive are the results to changes in SMP diameter, layer thickness, or tip shape ? Would a doubled diameter, doubled layer thickness, or a much sharper tip yield the same inferred fracture toughness ? I am not sure. These concerns should be addressed.

Our intention in this section was to provide a brief overview of the different approaches available in the literature for estimating or measuring fracture energy, rather than to present or evaluate this method in detail. We factually describe the methodology proposed by Reuter et al. (2015) and described by Eq.4

therein. We do not use the results of this study for the present work. The questions raised by the referee may be interesting but thus appear out of the scope of the paper and should be directly addressed to the authors of the paper.

8.11 l68 : The method of Richter et al. (2019) relies on highly simplified weak-layer stress representations and a long chain of assumptions that now appear dated. I suggest acknowledging these limitations more explicitly when citing this work.

The paragraph in introduction has been modified to more explicitly state the hypothesis of the model and its implementation : *"To our knowledge, only Gaume et al. (2017) and Richter et al. (2019) have attempted to parameterize a_c as a function of snowpack model variables. Their approach relies on an empirical stress-strength formulation derived from DEM simulations of a PST configuration, in which the slab is discretized as a sphere assembly and the weak layer as a collapsible triangular arrangement of bonded spheres (representative of depth/surface hoar) distinct from classical fracture-mechanics-based slab models. Richter et al. (2019) improved the parameterization by addressing the mismatch between the collapse height of the initial parameterization and the SNOWPACK stratigraphy resolution via an empirical fit accounting for grain size and density. While lacking a fully established physical basis, this approach provides a practical model implementation, though it currently requires human expertise to pre-identify weak layers."*

8.12 l77 : I see a conceptual tension between "physics-based" and "parameterization" in the way these terms are used here. A physics-based model aims to represent causal physical mechanisms, even if approximately. A parameterization, by contrast, is generally phenomenological and correlation-based. I recommend clarifying this distinction.

This comment is directly related to General Comment 4 and has already been addressed above ; please refer to that section for a detailed response.

We also respectfully disagree with the proposed distinction between "physics-based" and "parameterized" approaches. First, such a distinction appears overly categorical, as parameterizations can range from purely empirical relationships to strongly physics-informed formulations based on physically meaningful predictors. Second, the minimum cut is not introduced as a purely statistical predictor. Rather, it can be interpreted as a geometrical surrogate for the amount of load-bearing ice that must be severed to create a macroscopic fracture. As such, it reflects a physically meaningful aspect of the fracture process and captures part of the energy required to disconnect the ice network during weak-layer collapse. We therefore consider our approach to be a physics-based parameterization rather than an empirical correlation.

8.13 l89-l90 : This statement is an oversimplification. Crack propagation also depends on weak-layer elastic properties, geometry, slope angle, and related system characteristics. Please revise accordingly.

The manuscript has been revised accordingly : *"The stability of a snowpack and the critical length above which a crack spontaneously propagates depend on the mechanical behavior of the weak layer and the overlying slab and on the geometric configuration of the snowpack considered."*.

8.14 l110 : The equation appears to lack physical dimensions or units. In particular, considering E but not weak-layer thickness t directly affects the modeled wf, since $wf = sig^2/k$ where sig is stress and $k = E/t$ weak-layer stiffness. This should be discussed explicitly.

The manuscript has been amended accordingly, and more detailed justifications for the assumptions made have been added, along with a sensitivity analysis of the estimations of w_f with respect to these assumptions, in the appendix. For further details, see the response to general comment no. 6.

8.15 l169 : Does the Richter dataset include weak-layer thickness ? If so, I strongly recommend using it. If not, please provide an error-propagation analysis to quantify the effect of assumed versus measured weak-layer thickness.

This comment is directly related to general comment no. 6 and has already been addressed above ; please refer to that section for a detailed response.

8.16 l193 : The opening sentence of the Results section is unclear. Please rephrase for clarity.

This sentence has been reworded for clarity : *"In this section, we first established a fit (i.e. a power law) describing the relationship between the min-cut and density, both derived from 3D images of snow samples. This relationship is refined to account for differences between grain types using morphological descriptors."*.

8.17 Figure 3 : This figure appears redundant. In my view, Figure 4 already conveys the necessary information, so Figure 3 could be omitted.

We agree with reviewer suggestion, consequently Figure 3 has been removed in the revised version of the manuscript.

8.18 Figure 4 : Please consider using a more accessible color palette.

The color palette we use corresponds to that defined in the International Snow Classification (Fierz et al., 2009), which is widely used in the snow science community ; we have therefore chosen to retain it as it stands.

8.19 Figure 6 : Does this figure not suggest that a single parameter is insufficient to model wf ? Have you considered additional parameters ? Since CT data are available, I encourage you to examine recent work by Schöttner et al. on informative microstructural descriptors and to test parameterizations based on those variables. I understand the motivation to develop a practically usable model with easily measurable inputs. However, even if the eventual goal is a reduced operational model, it is still important first to identify the best-performing physically meaningful model rather than starting from assumptions that may already be too restrictive.

This comment is directly related to general comment no. 4 and has already been addressed above ; please refer to that section for a detailed response.

8.20 l274 : I cannot follow the equation in its current form. Please define the variables explicitly.

The equation is introduced earlier in the manuscript in the same form (l. 211), and we already refer the reader to this section for details regarding the parameter values used. We believe that the current referencing and level of documentation are sufficient, while reiterating the complete set of parameters here would introduce unnecessary redundancy.

8.21 Figure 7 : Adding row labels such as "observed" and "simulated" would improve readability.

The manuscript has been revised accordingly.

8.22 l301 : With constant t, the Heierli and WEAC models will naturally give very similar results. Variability in layer thickness would likely provide a stronger basis for discriminating between the models.

This comment is directly related to general comment no. 6 and has already been addressed above ; please refer to that section for a detailed response.

8.23 Figure 8 : I would appreciate uncertainty estimates for both the data points and the model curves.

In the original version of the manuscript, Figure 8 already displayed all available PST measurements for a given date whenever multiple observations were available. However, we acknowledge that the original formatting may not have been sufficiently clear, and we have therefore revised the figure to display these observations using error bars for improved readability.

Regarding uncertainties associated with the model curves, as discussed above in relation to the sensitivity of w_f to modelling assumptions, we chose to retain only a subset of assumptions in most figures throughout the manuscript. The rationale behind this choice is detailed in General Comment No. 6, to which we refer the reviewer for further explanation. Adding uncertainty bars or shaded areas to these figures would substantially reduce their readability and make visual interpretation harder.

8.24 l348l349 : This is presented as an advantage, whereas I would view it more as a limitation in terms of physical representation. I suggest rephrasing accordingly.

We maintain our view that deriving a continuous parameterization across the spectrum of snow types, rather than relying on a discrete classification, constitutes a meaningful advantage. In our view, this framework provides additional physical interpretability by helping identify the mechanisms underlying differences in min-cut between snow types, namely variations in SSA and sphericity, instead of directly fitting empirical relationships to grain types alone.

Since grain type classifications primarily represent a nomenclature of snow categories with underlying properties that vary continuously, we consider that a continuous parameterization offers a more physical representation of the system. For this reason, we have chosen to retain the original wording in the revised manuscript.

8.25 l360 : This assumption appears insufficiently supported by fracture-mechanics reasoning, especially for highly porous media, and your own results seem to contradict it. I therefore recommend rewording this point more cautiously.

As described just after (l. 360-363), the minimum cut only accounts for the amount of connected load-bearing ice that must be severed to disconnect the sample. It does not explicitly represent additional dissipation mechanisms that may occur during fracture propagation, such as local rearrangements, stabilization processes, or deviations from the minimum-energy fracture path. Consequently, it may be interpreted as capturing only part of the total fracture energy. However, we acknowledge that establishing a rigorous lower-bound relationship would require a dedicated fracture-mechanics analysis and is beyond the scope of the present study. We cautiously removed the wordings "It appears physically consistent". The contradiction raised by the reviewer is not specified precisely and we cannot build on for a more precise answer.

8.26 l366 : Given that ţCT data are available, I encourage the authors to go beyond noting the "lack of complexity in the proposed parameterization" and to provide a more advanced analysis.

This comment is directly related to general comment no. 4 and has already been addressed above ; please refer to that section for a detailed response.

8.27 l427 : The Heierli model does not focus on physical processes at the crack tip. Rather, it only describes slab deformation in regions not supported by the weak layer. This should be stated more accurately.

The sentence has been reworded accordingly : "In contrast, the Heierli model is based on an idealized geometry in which a homogeneous slab is supported on one side, and describes the deformation of slab sections that are no longer supported by the weak layer."

8.28 Overall, I view the manuscript as promising and potentially useful, particularly for operational snowpack modeling. However, I think substantial revision is needed before publication. The key changes are conceptual rather than cosmetic : the paper should more carefully distinguish system-level PST observables from material properties, onset from sustained propagation, and empirical operational parameterization from physically established fracture law. With those revisions, plus stronger baselines and more defensible validation, the study could become a valuable contribution.

We thank the reviewer for this overall constructive assessment of our work. We carefully considered the reviewers comments and incorporated most of them, with the aim of improving both the clarity and robustness of the manuscript. In particular, we revised several sections to better clarify the conceptual distinctions highlighted by the reviewer and strengthened parts of the analysis where possible. We believe these modifications have substantially improved the quality of the revised manuscript.

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