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.

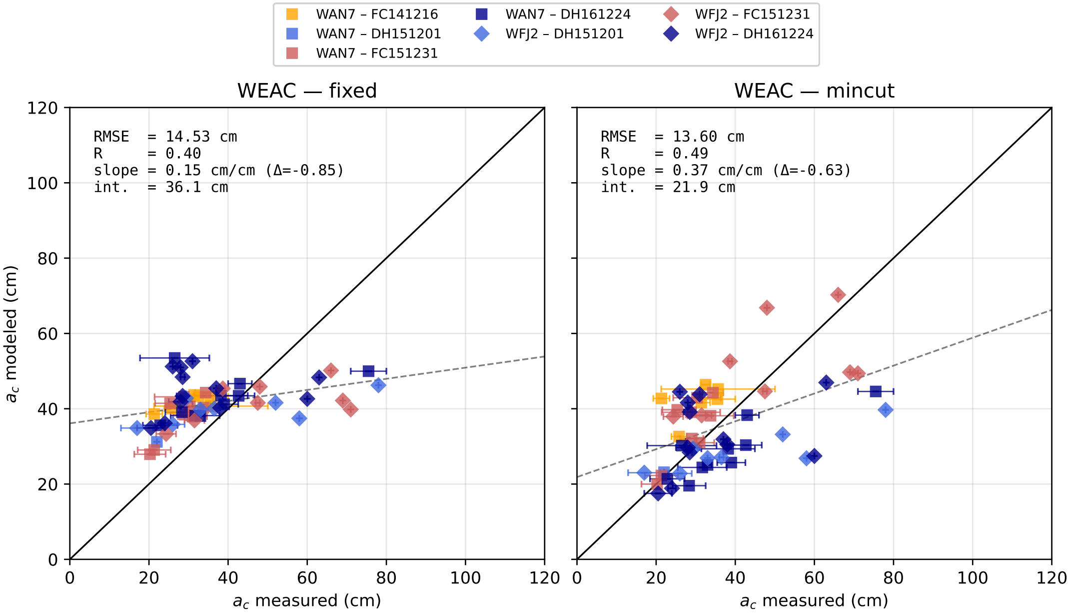
For clarity, please note that reviewers comments appear in dark blue, whilst authors responses are in black with added sentences in the revised manuscript in italic in the following document.

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)

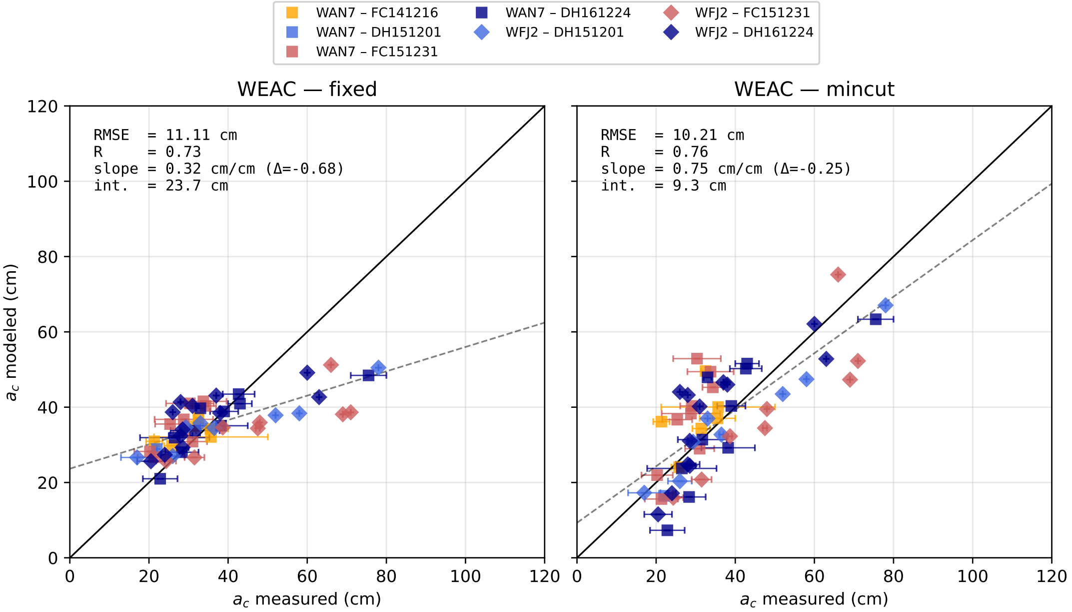


FIGURE 1 – Comparison of the measured and predicted critical cut length a_c for a weak layer fracture energy w_f either constant or parameterized using the min-cut derived parameterization Eq. XX. The comparison is performed using input profiles observed (a) or simulated with Crocus snow model (b). The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

The topic is highly relevant for avalanche forecasting and aims to bridge microstructural physics with operational snowpack models. In particular, the analysis of the CT dataset is valuable and extensive, contributing to an improved understanding of how microstructural properties relate to macroscopic properties. However, the manuscript currently suffers from conceptual inconsistencies, unclear physical interpretation, and methodological gaps that limit the strength of its conclusions.

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We would like to thank Valentin Adam for his detailed evaluation of the article. In the following section, we have chosen to break down his comments into different parts, which we address point by point in this document.

1 Critical energy release rate vs. critical crack length

From a fracture mechanics perspective, it remains unclear what is meant by the term "critical crack length. One could interpret it as the finite crack size required for the onset of crack propagation within a coupled criterion framework (Leguillon, https ://doi.org/10.1016/S0997-7538(01)01184-6), but this is not the case here. Renaming this quantity to "critical cut length would help avoid this ambiguity. Furthermore, the manuscript neglects the second condition required for fracture initiation, namely that the stress must exceed the strength of the weak layer (Leguillon). The context of coupled criteria is therefore missing. "The stability of a snowpack and the critical length above which a crack spontaneously propagates depend on both the fracture energy of the weak layer w_f and on the mechanical behavior of the overlying slab". What critical length is ment here, the finite length of a coupled criterion which needs to be exceeded (like everyone with a mechanical background reads) or the "critical crack length of a PST (like snow practitioners reads it) ?

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We thank the reviewer for raising this point regarding the interpretation of the term critical crack length. In this study, we consider a pre-existing crack created by the saw cut in a PST configuration. To avoid any confusion, we have replaced the term *critical crack length* with *critical cut length* throughout the manuscript when referring to PST measurements.

The use of a coupled stress energy criterion, such as that proposed by Leguillon, is indeed very relevant in situations where crack initiation from an initially intact material is considered. However, this is not the case here, as fracture is assumed to originate from a pre-existing substantial cut. Here, the stress criterion is thus expected to be less restrictive than the energy criterion, which therefore controls the onset of propagation.

"The fracture energy itself is known to be a function of the snow layer density and microstructure. This has never been stated in the mentioned reference Adam et al., https ://doi.org/10.1038/s414 024-51491-7

We thank the reviewer for pointing out this inconsistency. We based our citation on the following sentence from this paper "Common predictors for fracture toughness across highly porous materials include density and microstructure". This is not strictly the same and we reword accordingly : *"Recent literature mentioned that common predictors for fracture toughness, related to fracture energy through Irwin's relation, in highly porous materials include density and microstructure (Adam et al., 2024)."*

The magnitude of the "critical crack length derived from a PST does not provide direct information about crack propagation propensity. It is a geometric measure rather than a material property. For the same weak layer, different critical cut lengths can be obtained depending on slope inclination, slab stratification, and cutting direction (see Adam et al., https ://doi.org/10.1038/s41467-024-51491-7).
"A small a_c means that the weak layer below the slab is prone to crack propagation." This statement is incorrect. A small "critical crack length" is not a direct indicator that the weak layer is prone to crack propagation, but a configuration-dependent system response of the slabweak layergeometry setup as you partly mention : "The critical crack length ac

thus depends on both how the slab deforms when it loses support from the weak layer, and the specific fracture energy of the weak layer ". Same applies for "Finally, we examine the potential of predicted critical crack lengths to discriminate between weak layers and more stable layers within a profile."
The "critical crack length" has so many geometric and material dependencies which scale differently that its nearly impossible to interpret it. E.g. a longer "crack length can be related to a stiffer slab due to a crust and even though the weak layer might be potentially dangerous. Vice versa how should be the small "crack length at lower slab height be interpreted in Figure 9 without knowledge ? Those interactions makes the interpretation highly complex, particularly for operational applications or practitioners.
The profiles in Figure 9 mainly reflect the effect of slab stiffness, which causes the "critical crack length to increase more or less linearly with depth. The observed discontinuities are introduced by the min-cut parameterization associated with different grain types. If the min-cut parameterization were plotted with the corresponding calculated critical energy release rates, this would contain essentially the same information but would be easier to interpret. In this context, the level of "critical cut length is unnecessary. These values are absolute and configuration-dependent, and therefore not intrinsic material properties. As such, they are not suitable quantities to describe or predict fracture initiation or propagation.

The first part of the comment concerning the misleading interpretation of the values of the critical crack length in the original manuscript is already addressed in the reply to remark 1 of reviewer P. Rosendahl, please refer to it for more details.

As stated by the referee Johan Gaume "it is precisely because a_c depends on geometry, slope angle, elastic properties, slab thickness, weak-layer strength and fracture energy that it is useful as a system metric". Therefore, we choose to keep a_c as it provides a system-level metric characterizing the onset of crack propagation of the given system. In addition, a_c allows a more direct connection with field measurements such as propagation saw tests. The objective of this study is not to define an intrinsic fracture criterion, nor to represent both fracture initiation and propagation onset criteria. Instead, we evaluate the ability of a_c derived from an anticrack framework (e.g. Heierli et al. (2008) ; WeiSSgraeber and Rosendahl (2023)), to identify layers that are close to the onset of crack propagation within a snow profile and track it over the course of a season. Therefore, a_c is not proposed as a standalone stability criterion, but rather as one component that should be combined with other indicators (e.g. failure initiation criteria) for a comprehensive stability assessment.

The terminology "crack propagation propensity is vague. The critical energy release rate describes fracture initiation (or "onset, as stated in the title). If crack propagation itself is to be addressed, a steady-state framework (e.g., Rosendahl et al., https ://doi.org/10.5194/nhess-25-1975-2025) or dynamic fracture processes (e.g., Bergfeld et al., https ://doi.org/10.5194/nhess-23-293-2023) must be considered. From a fracture mechanics standpoint, the PST-derived "critical crack length is not a valid indicator of crack propagation and has historically been misinterpreted in the snow science community.

A detailed response to this comment has already been provided in the reply to comments on the same subject by Referee P. Rosendahl (comment no. 2. The manuscript should more clearly separate onset of propagation from sustained propagation, crack arrest, and avalanche release").

Since PST measurements are partly conducted on inclined slopes, the relative contributions of mode I and mode II should also be clarified. A brief statement quantifying that the loading is predominantly mode I (e.g., 95%) and that the analysis is therefore restricted to mode I-driven initiation would improve clarity.

A detailed response to this comment has already been provided in the reply to comments on the same subject by Referee P. Rosendahl (comment no. 3. "The recent mixed-mode literature raises an important limitation of the scalar wf formulation used here.").

2 Missing physical justification of the min-cut fracture energy link

No direct experimental validation is provided to support a correlation between min-cut and critical energy release rate. The argumentation of the parameterization follows an

indirect route via the "critical crack length. The comparison with a constant energy release rate primarily shows that, within the PST dataset, the inferred energy release rate is not constant but varies, likely with density. Since density is also a primary input to the min-cut formulation, the resulting improvement in modelled "critical crack length does not constitute a robust justification for a physically meaningful parameterization between min-cut and fracture energy. Rather, this highlights the need for a controlled experimental campaign combining fracture mechanical field tests with direct CT measurements across different grain types.

We agree that the proposed parameterization of the minimum cut (min-cut), and its relationship to fracture energy, is not directly validated by independent experimental measurements of the critical energy release rate. Instead, our evaluation relies on an indirect approach based on the critical crack length a_c derived from propagation saw test (PST) data. This choice is partly dictated by data availability. To our knowledge, PST datasets are currently the only field data that provide both a large number of observations related to crack propagation onset and detailed snowpack profiles. While they do not allow direct measurement of fracture energy, they provide a basis for evaluating model behavior in an operational perspective.

We also agree that the comparison with a constant fracture energy does not, by itself, demonstrate a physical relationship between min-cut and fracture energy. This relationship is an assumption, which we consider physically plausible and which is supported by previous work (e.g. LeBaron and Miller, 2014). The comparison presented in Figure 7 is therefore not intended as a direct physical validation, but rather as a practical evaluation showing that the proposed parameterization leads to more consistent modeled a_c values than assuming a constant fracture energy. The main contribution of this study is thus to propose a practical and evaluated parameterization of the weak layer fracture energy w_f, a poorly constrained parameter in energy-based slab models, from macroscopic variables available in snowpack models, with the aim of applying it to snow model outputs.

Finally, we acknowledge that a series of controlled experiments combining fracture mechanical test along with CT measurements on several grain types would be very useful, but this goes obviously beyond the scope of the present study.

Following this comment and other comments from referees and community highlighting the limitations of the present study, we added a paragraph in a new discussion section that partly address this part of the comment : *"It would therefore be valuable to extend the validation of the proposed parameterization by applying the method to other PST datasets, in order to test the transferability and robustness of the relationship across different snowpack types (oceanic, continental, and Arctic snowpacks), a larger range of grain types including surface hoar SH, non-persistent weak layers such as PP and DF, and ideally PSTs performed on non-weak layers as a reference, as well as on geometric configurations favoring greater mode mixity. The proposed w_f parameterization should thus be regarded as an effective, PST-based formulation, subject to the limitations of the dataset on which it was built, rather than as an intrinsic mechanical law, and its operational applicability to any avalanche release zone remains to be demonstrated."*

It is unclear whether the PST dataset alone is suitable to validate such a relationship across different grain types. It should first be demonstrated that grain types can be statistically correlated with fracture energy derived from the PST dataset itself. If this is not possible, the dataset may not be appropriate for validating the proposed parameterization. In addition, it is unclear whether the PST dataset contains a sufficient diversity of grain types to support such validation.

As mentioned in the response to last comment, the evaluation of the relation between the predicted mincut and the pseudo-measured weak layer fracture energy is restricted to weak layers, which are the only layers for which both onset of propagation measurements and detailed on the snowpack (and meteorological forcings) are available. Indeed, propagation saw tests are performed almost exclusively on pre-identified weak layers (typically selected using compression tests). As a result, the dataset is inherently biased toward specific types of weak layers and does not provide a representative sampling of all grain types. Within this limited scope, we assess whether the proposed parameterization leads to consistent and realistic estimates of critical cut length (as a proxy of crack propagation onset).

However, we agree that this relationship is not directly validated across all grain types in the present study. The use of min-cut is intended as a way to introduce a microstructure-based descriptor that is, a priori, applicable across different snow types. Based on this assumption and given the availability of tomographic data covering a wide range of grain types, we propose a generalized formulation. However, we agree that its validity beyond weak layers remains to be clearly validated in future work.

This comment is also in line with those made by P. Rosendahl and Ron Simenhois regarding the limitation of the validation to certain types of snow and snowpack ; we have addressed this in the manuscript by adding a paragraph to the "Discussion" section and rephrasing certain sentences to indicate more clearly the limited validity of our parameterization.

The following paragraph have 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."*

The assumption of a fixed weak-layer thickness in the PST analysis is not explained, as this information is available in the original dataset. Similarly, the weak-layer elasticity is parameterized via density relationships derived for general snow types rather than specifically for weak layers. It is not stated which parametrisation is chosen (AC, CT, SMP) from Gerling 2017. This would improve clarity. Furthermore, density-based parameterizations reflect an apparent stiffness rather than the theoretical Youngs modulus. Consequently, the experimental method used to derive such parameterizations should be consistent with the actual loading conditions, particularly with respect to strain rate and time-dependent behavior. For example, formulations such as Gerling (2017) may yield Youngs modulus values several times higher than the effective modulus relevant for PST-type configurations (see Schöttner et al., 2025, Fig. 13a). In addition, more appropriate parameterizations exist for specific weak layer grain types (e.g., Schöttner et al., 2025). A clear separation between the derivation of weak layer and slab elastic moduli is necessary.

In the initial version of the manuscript, the reasoning behind the various assumptions was not clearly explained. Following this comment, as well as other comments highlighting the need for a sensitivity analysis of the various assumptions regarding the modelled values of w_f, as mentioned in comment no. 6 by P. Rosendahl, the revised version of the manuscript contains more explicit explanations of these choices and their implications. Please refer to the reply to P. Rosendahls comment for further details.

Since the weak-layer Youngs modulus strongly influences the calculated energy release rate (see sensitivity study Adam et al., https ://doi.org/10.1038/s41467-024-51491-7), it is important to assess whether the observed correlation between min-cut and fracture energy isnt in fact primarily a density correlation. Given the multiple sources of uncertainty in deriving fracture energy from PSTs, this should be explicitly tested. For example, a direct comparison between weak-layer density and min-cut may already yield similar or even stronger correlations.

This point is partly addressed in the reply to the comment above, and partly in line with comment no. 5 by P. Rosendahl ; please refer to the reply for further details.

The manuscript itself acknowledges that density is the main variable controlling min-cut and that measured densities exhibit strong scatter : Since the density of the weak layer is the main variable used to estimate the min-cut in the parameterization Eq. 5, and "we assume that the min-cut can be related to a macroscopic quantity, in particular the snow density. A sensitivity analysis quantifying this effect would therefore be highly valuable.

We agree that a sensitivity analysis of density uncertainty would be valuable, particularly given the central role of density in the proposed min-cut parameterization. However, we distinguish between two sources of density information in the present study. First, snow density derived from X-ray microtomography is generally considered highly reliable and robust. Previous studies have shown strong agreement between muCT-derived densities and independent measurements, typically within a few percent, supporting the use of muCT as a reference for snow microstructural characterization (Proksch et al., 2016). We are therefore confident that the min-cut parameterization itself, which is calibrated using tomographic measurements, is robust with respect to density estimation.

The main source of uncertainty lies in the weak-layer densities measured in snow profiles, which strongly influence the relationship between min-cut and fracture energy shown in Figures 6a and 6b. As already discussed in Section 4.2, these uncertainties likely arise not only from the sampling method itself (e.g., density cutter limitations), but also from the pronounced small-scale spatial variability of the snowpack. Due to logistical constraints, the different field measurements conducted over consecutive weeks were performed at nearby, but distinct, locations, introducing additional variability. Furthermore, model degradation when using measured rather than modelled densities appears to be linked not only to measurement uncertainty itself, but also to inconsistencies in the temporal evolution of measured weak-layer densities relative to the evolution of other snowpack variables and propagation saw test measurements.

While we acknowledge this as a limitation of the present study, a robust quantification of density uncertainty and its propagation into the model remains a complex problem that would require substantial additional work beyond the scope of this study.

While the min-cut may indeed be a promising proxy for fracture energy, the current approach does not convincingly demonstrate this. It would be more straightforward to directly test correlations between fracture energy and other available microstructural descriptors (e.g., from CROCUS or grain-type parameterizations) rather than introducing an additional layer of assumptions via min-cut.

We agree that directly testing correlations between fracture energy and microstructural descriptors (e.g. density or grain-type parameterizations) would be a simpler approach. However, the motivation for introducing the min-cut is precisely to provide a descriptor of the microstructure that goes beyond such bulk or categorical variables. The min-cut has a direct mechanical interpretation : it represents the minimum number of bonds that need to be broken to separate a snow sample into two parts. As such, it provides a physically meaningful proxy for fracture processes and, in particular, for fracture energy. Importantly, min-cut implicitly integrates both density and microstructure (e.g. connectivity and geometry of the ice matrix), which are known to be factors that influence fracture toughness in porous materials, and help reduce the dimensionality of the problem (refer to the reply to comment from P. Rosendahl for further details). In contrast, descriptors such as density or grain type alone do not explicitly capture this structural connectivity. In addition, min-cut benefits from the availability of a large tomographic database across a wide range of snow types, which enables the development of a more robust parameterization. This comment overlap with comment "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." from P. Rosendahl. We have provided a detailed response to this ; please refer to it for further clarification.

3 Further comments

The inclusion of the Heierli model does not add clear value to the manuscript. The WEAC model is state-of-the-art and computationally efficient, and the comparison does not contribute significantly to the overall argument.

We agree that the WEAC model represents a state-of-the-art, and that it is well suited to the PST configuration. However, the inclusion of the Heierli model serves a complementary purpose in this study. The Heierli model provides a much simpler analytical framework. Its comparison allows us to evaluate whether the additional complexity of WEAC significantly affects the results. Our findings suggest that, for the questions addressed here, the simpler model captures the same key results, which is an important result in itself. Furthermore, the Heierli model is computationally more efficient and easier to implement in snowpack model for application involving many layers over large spatial and temporal domains. Demonstrating that similar conclusions can be obtained with this simpler framework supports its potential use in operational or large-scale modeling contexts as mentioned in section 2.1 of the article.

Figure 1 is misleading, as the slab is not represented as a true Timoshenko beam ; the boundary conditions do not allow for rotational freedom at the slab end.

We are not entirely sure how to understand this comment and thus how to address it. However, we do agree that Figure 1 could indeed be misleading, particularly regarding the distinction between deforming and non-deforming parts, which is why we have added a clarification to the caption of Figure 1. We also added a slight rotation of the unsupported part of the slab.

The nomenclature should be used consistently (e.g., a_c vs. AC in figures). Indexing of Units

should also be checked carefully m^2 squared instead of just m2.

The manuscript and typo has been corrected accordingly.

Introduced variables should appear consistently in figure labels (e.g., min-cut with ζ).

The manuscript has been corrected accordingly.

Figure 5 contains redundant labeling (headers and x-axis labels).

The manuscript has been corrected accordingly.

Figures 3 and 4 appear redundant ; one should be removed. Additionally, labeling inconsistencies (min min-cut vs. min-cut) should be corrected.

Following this comment, the figure has been removed.

Avoid using red and green with identical marker shapes due to accessibility concerns.

We believe this comment refers to Figures 3 and 4, and that it echoes the point made by P. Rosendahl. For further details, please refer to the reply addressed to P. Rosendahl.

Use consistent terminology throughout (e.g., min-cut vs. mincut).

The manuscript has been corrected to use consistently the terminology "min-cut".

A schematic overview of the full methodology (similar to Mayer et al., 2023, Fig. 1 https ://doi.org/10.5194/ 23-3445-2023) would greatly improve readability.

We have decided to modify the section describing the methodological approach of the article at the beginning of the "Results" section in order to clarify the overall structure of the article.

The following section has been modified in the revised version of the manuscript : *"In this section, we first established an empirical fit (i.e. a power law) describing the relationship between the min-cut ζ and density, both derived from 3D images of snow samples (see section XX). This relationship is refined to account for differences between grain types using morphological descriptors specific to the Crocus snow model, namely the sphericity and SSA. Following the establishment of the relationship between the min-cut and density, the second part of the study focuses on the relationship between the min-cut ζ and the fracture energy w_f using a firstorder linear relationship. To do so, we estimate the weak layer fracture energy using the PST dataset : the WEAC model is provided with the critical cut length measured with PSTs, for both observed and modelled profiles. Knowing the critical cut length and the entire profile, the model allows us to trace back to the corresponding fracture energy of the weak layer. With these pseudo-observations, we can relate w_f to the min-cut and ultimately to the density and morphology descriptors. Eventually, we evaluate the parameterization of w_f and its relevance to model the critical crack length in advanced (WEAC) or simple (Heierli) slab models."*.

The treatment of sphericity and SSA is difficult to follow. It is only briefly mentioned in a figure caption (Fig. 6) and appendix. A1 clearer methodological description of how different grain types are handled is needed.

We agree that this part is only briefly described, a clearer and more explicit methodology has been added to the appendix in the revised manuscript : "Table A1 associates grain types with sphericity and SSA ranges. These ranges are obtained from the 5th and 95th percentiles of the sphericity and SSA distribution for each grain type, using 60 years of the S2M reanalysis (Vernay et al., 2022) in the French Alps massifs using version 9.0 of Crocus snow model (Lafaysse et al., 2025). They correspond to the distribution of SSA and sphericity as modelled by Crocus and are therefore specific to it. These ranges are used throughout the article, where fracture energies w_f are calculated using field measured profiles, for which we do not have access to the sphericity or SSA of the weak layers (see sections XX and XX)."

Key contextual explanations currently presented in the first section of the discussion should be moved to the methods, this would help to better follow the conceptual idea.

We have decided to retain the explanations and contextual information from the first part of the discussion, although these have been partially revised following consideration of comments from other reviewers. The paragraph at the beginning of the results section, added in response to a comment mentioned earlier, also serves to provide some additional contextual information.

Figure 2 would benefit from additional labelling of the weak layer of interest

We have added a labeling of the weak layers of interest in Figure 2 as suggested in this comment.

3.2.2 (Fig. 6Y) ?

This was a typo and it has been corrected accordingly in the revised manuscript.

Figure 7 : Indicate clearer within the plots that a) b) are observed and c) d) are simulated. This can only be know with reading the caption.

The manuscript has been modified accordingly in order to improved clarity.

Figure 6 c) to f) x axis label is missing or can not be required to be transferred by the reader from g) because its another scale and plot.

The manuscript and the labels have been amended accordingly.

Figure 6 : what are the units for time ?

In this figure, the units do not correspond directly to regular time intervals, as they represent sampling points, usually on a weekly basis. We acknowledge that this may not be obvious to readers, thus we have modified it to be in days.

The inconsistency in error use and their origin is confusing think about adding errors in the whole model chain as well.

All inconsistencies have been clarified and revised throughout most of the manuscript based on comments from reviewers and public discussion.

Figure 7 a) where does the different errors measured for a_c come from ? They are rather relative nor absolute.

The errors on measured a_c correspond to the various PSTs conducted on the same date and on similar profiles, and represent the standard deviation and the mean of the measurements. They are therefore absolute values.

Références

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