Responses to RC on manuscript EGUSPHERE-2026-733

Authors :
Diego Monteiro,
Léo Viallon-Galinier,
Kévin Fourteau,
Oscar Dick,
Pascal Hagenmuller,

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.

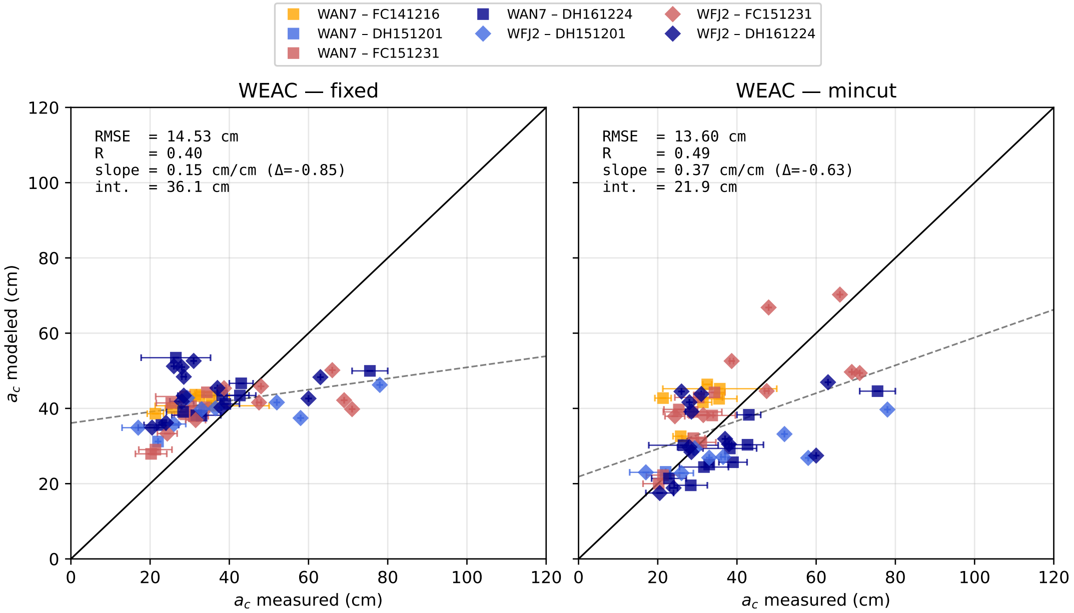
For clarity, please note that reviewers comments appear in dark blue, whilst authors responses are in black with added sentences in the revised manuscript in italic in the following document.

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)

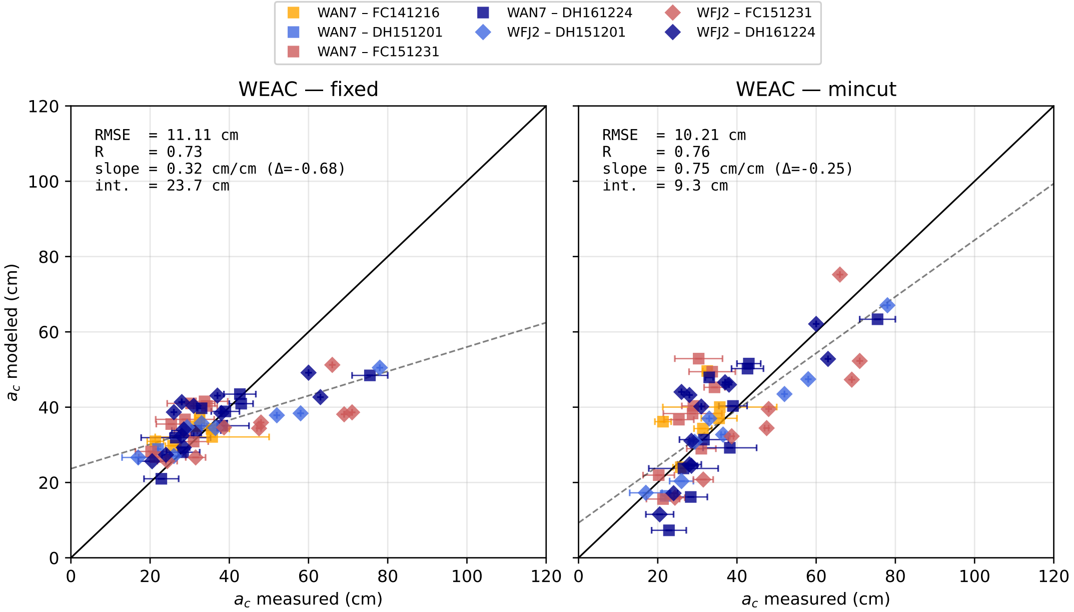


FIGURE 1 – Comparison of the measured and predicted critical cut length a_c for a weak layer fracture energy w_f either constant or parameterized using the min-cut derived parameterization Eq. XX. The comparison is performed using input profiles observed (a) or simulated with Crocus snow model (b). The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

Referee comments : Johan Gaume

I found this manuscript generally well written, timely, and of important potential interest for avalanche forecasting. The central objective is relevant : to relate weak-layer fracture energy in dry-snow slab avalanche models to snow properties that can be measured or simulated by detailed snowpack models, in particular Crocus to model the onset of crack propagation. The proposed workflow, linking microstructural information from 3D images to a min-cut proxy and then to PST-derived effective fracture energies, is interesting and useful. Although the underlying mechanical representation remains deliberately simplified, the model reproduces field critical crack lengths and their temporal evolution reasonably well. From that perspective, the present approach already appears to be a meaningful and promising step for operational avalanche forecasting : it does not solve the full avalanche-release forecasting problem, but it provides a practical and mechanically interpretable indicator for the onset of crack propagation in modeled snow profiles. More complex and complete models are certainly possible, and may be needed to address mode mixity, finite softening, crack arrest, slab fracture, or skier triggering more completely. However, for the scale and application considered here, the model appears to capture the dominant mechanics controlling the evolution of the critical length while remaining simple enough for operational use.

We would like to thank Johan Gaume for his extensive review and positive appreciation of the article. His comments have contributed to improving the quality and clarity of the paper. His perspective on strength/stress-based approaches (and promising cohesive interface approaches) also provides a valuable complement to the LEFM-based energy formulations considered in this study, although these frameworks are currently not combined in our work. Nevertheless, they open promising avenues for future research that we believe are worth exploring further.

1 General comments

1.1 First, I agree with previous comments that the fracture energy obtained here should be framed more explicitly as an effective PST-based failure/fracture parameter, rather than as a fully established intrinsic fracture law valid for all geometries and loading modes. This is already partly implicit in the manuscript, since wf is obtained by inversion of PST critical crack lengths using a slabweak-layer model, and the authors themselves report only moderate correlations between min-cut and inferred wf. I suggest making this point more explicit throughout the paper

We thank the reviewer for this comment, which aligns closely with concerns also raised by Ron Simenhois and reviewer P. Rosendahl (comment no. 7), to which we partly responded.

We agree that the inferred w_f should be framed more explicitly as an effective PST-based fracture/failure parameter, rather than as a fully general fracture law applicable across arbitrary loading conditions and geometries. With the PST dataset, we mainly explore the onset of crack propagation in mode I (now specified in the title). However, in the WEAC framework, the weak layer fracture energy is an intrinsic material property of the weak layer and could be used as an input of different configurations where the mode I dominates at the crack onset.

To clarify this point, we distinguish between two aspects of the proposed framework. The parameterization of min-cut itself is calibrated using tomographic images spanning a broad range of snow microstructures and may therefore be considered generalized. However, the relationship established between min-cut and fracture energy remains empirical. It is derived from a dataset of w_f values obtained through inversion of PST critical cut lengths using the WEAC model, under a relatively specific experimental and modelling setup. Indeed, the PSTs considered here were performed on flat terrain, under loading conditions strongly dominated by mode I (typically 85 to 100%), and on persistent weak layers (primarily FC/DH) from two nearby high elevation alpine sites within a similar snow climate.

Following the reviewers suggestion, a sentence has been added in the abstract : *"This yields a relationship between w_f and the min-cut, calibrated for grain types typical of the weak layers associated with the triggering of dry-snow slab avalanches (faceted crystals and depth hoar), in a PST configuration that* *is primarily representative of mode I (i.e., compression)"*. We also revised the discussion by adding a whole new section on limitations and perspectives of the study, to state this limitation more explicitly. We also highlight as a perspective the need to evaluate its transferability using PST datasets covering a broader diversity of snow climates, slope inclinations, loading configurations, and weak-layer types. Such an extension would require substantial additional datasets, including co-located snow profiles and meteorological forcing suitable for reproducing snowpack evolution in snowpack models.

The following paragraph is part of the new discussion section : *"However, the linear relationship between the min-cut and w_f, as well as the validation presented in Sections XX, are established and evaluated on a w_f dataset derived from the inversion of critical cut lengths measured in PSTs using the WEAC slab model. As a result, the proposed parameterization is strongly related to this specific dataset, which partly motivated the choice of a first-order linear fit in order to avoid overfitting a relationship whose "training" set remains limited."*

and the paragraph : *"Regarding the limitations of the w_f dataset itself, it is mainly restricted to Mode-I-dominant conditions, as illustrated in Figure B1 b). However, following the work of Adam et al. (2024), who performed a large number of PSTs under various geometric configurations and loading conditions, demonstrate that such mode mixity conditions (i.e. crack propagation dominated by mode I, approx. > 75%) are representative of typical avalanche-triggering configurations (including "remotely") with slope angles of 0 to 40°. Nonetheless, the snowpack in a 30 to 40°actual avalanche start zone may differ substantially from a research site in terms of snow stratigraphy (i.e. due to wind redistribution, settlement, radiation, and spatial variability). This represents a significant limitation, although it is common in most research on snowpack stability, due to the difficult access and monitoring conditions in these areas."*

1.2 Second, a critical crack length model should, in principle, involve the weak-layer strength, its pre-peak compliance, and, if the weak-layer behavior is not perfectly brittle, a softening distance or fracture energy. In the present framework, the role of strength is somewhat hidden inside the effective energy-based formulation. It would therefore be interesting to compare the present approach not only with the Heierli model, but also with the stress-strength model previously implemented in SNOWPACK by Richter et al. Although that model is also simplified, notably because it assumes perfectly brittle behavior and does not explicitly include a softening length or fracture energy, it provides a useful reference point, and I suspect that very similar results may be obtained. Such a comparison could be placed in an appendix, or at least discussed qualitatively if the authors prefer not to expand the main analysis. In fact, the Richter model somewhat hides fracture-energy or softening information, which may partly explain the empirical correction factor proposed by Richter et al., whereas the Heierli-type formulation hides strength and weak-layer elasticity information inside an effective wf. More generally, explicitly including strength would help avoid potentially inconsistent interpretations, for example when a stress-based stability indicator such as MEPRA predicts natural release while the present propagation model still returns a finite positive critical crack length.

We thank the reviewer for this comment. We agree that the energy release rate is the signature of a hidden weak-layer constitutive law. In the case of a small process zone compared to the elastic stress redistribution length, the linear elastic fracture mechanics (LEFM) framework does not build on the detailed interface law but only requires the integrated energy release rate. In principle, for any material, strength and toughness can be de-correlated. Moreover, local stress approaches might be limited by the singularity at the crack tip and should be replaced by "integrated or non-local" stress approaches (CZM or finite increment of crack size). Therefore, there is no theoretical reason to have a match between energy and stress approaches. However, in practice, for a narrow range of snow microstructures (e.g. weak layers), we might expect a correlation between toughness and strength, which may yield similar predictive performance of different approaches.

The present study is deliberately based on LEFM and energy-based anticrack formulations. Our objective was therefore not to directly compare LEFM-based and stress/strength-based approaches, but rather to investigate whether fracture energy could be parameterized from microstructural information available in snowpack models.

We agree that comparing the present energy-based approach with the stress/strength framework developed by Gaume et al. (2017) ; Richter et al. (2019) would be scientifically valuable, particularly since it represents one of the latest operational implementations of crack-propagation onset modelling in snowpack models. In fact, the present study builds extensively on the same PST measurement campaigns originally performed for Richter et al. (2019) study. However, implementing a direct quantitative comparison with the Richter model would require substantial additional developments. In practice, this would involve adapting and recalibrating the stress/strength formulation to the prognostic variables available in Crocus (e.g., density and grain-size distributions), and reperforming the corresponding PST-based fitting. We believe such an extension would constitute a substantial study in itself and would fall beyond the scope of the present manuscript.

Following the reviewers suggestion, we have expanded the Discussion to more clearly position the present work relative to strength-based approaches, and qualitatively compare our results to the one of Richter et al. (2019) : *"A qualitative comparison with Richter et al. (2019) reveals consistent results across several aspects. The stratigraphic profiles of* a_c *(their Figure 9) exhibit a pattern similar to those obtained here, with a progressive increase of* a_c *with depth and local minima clearly associated with weak layers. The temporal evolution (their Figure 11) is likewise comparable : critical cut lengths remain relatively stable over most of the season before increasing substantially towards its end, consistent with the progressive stabilization of weak layers. Similarly, Richter et al. (2019) report degraded performance when using manually observed profiles instead of simulated ones, in line with the score differences observed here between observed and Crocus-simulated profiles. This overall consistency across two independent modelling frameworks is encouraging and further supports the physical coherence of the present results. Beyond this agreement, the approach proposed here builds on the state-of-the-art WEAC slab model and a min-cut parameterisation derived from a large and diverse set of snow microstructure samples, suggesting more robust transferability to a broader range of snowpack conditions. The inherent limitations of the linear relationship between min-cut and* w_f *are, however, discussed in Section 4.5."*

1.3 Going one step further, one could introduce both pre-peak compliance and finite softening in a simplified manner (see preprint link at the end of the review). This may help constrain either fracture energy or softening length more robustly, and may also improve the agreement with measured critical crack lengths. Alternatively, the full coupled criterion of WeiSSgraeber and co-workers could be considered, although constraining both mode-I and mode-II fracture properties from the present dataset may be difficult without dedicated fracture experiments or simulations. The model proposed in the preprint linked at the end of this review could also provide a simplified route, with the merit of clearly distinguishing the brittle contribution associated with the elastic mismatch between weak layer and slab from the finite-softening contribution within a compact mechanical formulation. Distinguishing strength and fracture energy may also help improve the link with the min-cut density. For instance, the data compiled by Jamieson and Johnston show that snow strength is strongly related to density, but also to grain type. Their 2001 work (https ://doi.org/10.3189/172756401781819472) proposed separate density-based parameterizations for persistent and non-persistent grain types, which is broadly consistent with the trends observed here for the min-cut. This raises the question of whether the min-cut may, at least partly, correlate more directly with weak-layer strength than with fracture energy alone. If this dominant strength contribution could be separated, the remaining contribution associated with softening distance or fracture energy might be easier to evaluate. One possible first test would be to use a model combining a brittle contribution ac0 and a softening (or fracture energy contribution), as suggested in the preprint linked below, with a brittle contribution similar in spirit to the Richter/Gaume formulation, possibly using the Jamieson and Johnston (2001) shear-strength parameterization already used in SNOWPACK. A correction term would then account for finite softening or fracture-energy effects. Such a formulation would provide a simple way to include both pre-peak weak-layer compliance and finite softening, while keeping the model compatible with operational snowpack-model inputs. It could also help determine whether the min-cut primarily reflects strength, fracture energy, or a combination of both. I am not requesting to try this in this paper but I think it could be interesting in the future. As a first and simpler step, I suggest testing the Richter/Gaume model with a grain-type-dependent strength parameterization.

We fully agree with this interpretation. In mode I, the fracture energy is expected to reflect both the brittle failure of the weak-layer bonds and the subsequent compaction and possible crushing of the weak layer. The former is related to the amount of load-bearing ice that must be disconnected to create a macroscopic fracture and is therefore expected to be linked with strength (and min-cut), whereas the latter is likely controlled by weak-layer density and microstructure.

From a fracture-mechanics perspective, these different dissipation mechanisms are not treated separately. Instead, they are incorporated into a single effective quantity, the mode-I fracture energy. The LEFM framework does not require a detailed description of the underlying processes, but only that their combined effect can be represented by w_f and that the small-scale yielding assumption is satisfied.

In PST experiments, the cut zone is completely destroyed and the residual stress associated with weak-layer compaction is assumed to vanish. Under these conditions, post-peak compaction processes contribute to fracture propagation only through their contribution to the effective fracture energy. They do not generate long-range slab / weak-layer interactions extending beyond the fracture process zone, which would fall outside the scope of LEFM.

This is truly a very interesting discussion. However, from a practical perspective, it is less clear how these ideas could be incorporated into the present manuscript. We deliberately chose to work within the LEFM framework and to use the associated numerical tool WEAC, which provides a well-established basis for the interpretation of PST experiments.

Cohesive-zone models certainly open promising avenues for future work, particularly for investigating the respective roles of bond failure and weak-layer compaction. However, such developments are beyond the scope of the present study.

In addition, as discussed in Comment 9.2, it is not straightforward to apply the approach proposed by Richter, which was developed using SNOWPACK simulations, to Crocus simulations because several diagnostic variables required by the method are not available in both models.

Finally, it should be noted that the cohesive-zone model presented by Johan Gaume in the ArXiv manuscript is not an extension of Richter's work. Rather, it is based on a cohesive-zone formulation coupled with a simplified slab representation and therefore follows a different modeling approach.

1.4 The low predicted critical crack lengths close to the surface deserve additional discussion. From the point of view of the slabweak-layer system and the equations used, such low values are understandable. However, near-surface snow is often poorly bonded and may not form a sufficiently cohesive slab. In practice, crack propagation may then be prevented by slab fracture or crack arrest, even if the computed weak-layer critical crack length is small. The manuscript already mentions this point, but I think it should be emphasized more in the context of future work as a simple tensile-slab criterion could be a useful addition. For example, one could compute a tensile failure length (including bending and pure tension e.g. as in Gaume et al. (2015, TC)) and compare it with the system critical crack length. If the tensile fracture length is smaller than the critical length, sustained propagation in the weak layer would not be mechanically possible because the slab would fracture first. While the critical crack length is a meaningful metric for the onset of crack propagation in an idealized slabweak-layer system, it is not sufficient by itself for skier-triggering assessment. A useful next step would be to develop an index in the spirit of Gaume and Reuter, and more recently Méloche et al., comparing the critical crack length to a skier-induced crack or damage length. This would naturally connect the present work to operational triggering likelihood rather than only to propagation onset. This aspect could be discussed as an outlook.

We fully agree that the critical crack length estimated in this article is not a sufficient criterion to assess a risk of crack propagation in a snowpack layer. As noted by the reviewer, fresh snow near the surface may not be sufficiently bonded to constitute a cohesive slab and therefore does not necessarily present a real risk of propagation onset.

We agree that the critical crack length indicators estimated here are insufficient on their own to assess hazard, and must be combined with additional criteria : strength-to-stress ratios for failure initiation (or coupled criterion like Leguillon), tensile strength for crack arrest, dynamic crack propagation and slab detachment criteria, as suggested in this comment. In response to this, we propose to add a perspective in the discussion section (specifically discussing the results presented in Fig. 9) stating explicitly that the critical cut length alone is insufficient to assess avalanche release hazard, that it must be coupled with other mechanical criteria, and that future work in this direction would be particularly valuable to combined indicators.

The following paragraph is part of the new discussion/perspective section : *"Ultimately, the critical cut length modeling proposed in this study remains insufficient to assess fracture propagation on its own, as it addresses only one specific stage : the onset of crack propagation, assuming that failure initiation as already occurred. As illustrated in Figure XX, which shows the evolution of* a_c *along a snow profile, particularly low critical cut lengths are found near the surface. However, near-surface fresh snow may lack sufficient bonding to constitute a cohesive slab (Schweizer et al., 2003), and crack propagation may be prevented by slab fracture or crack arrest even when the computed critical cut length is small (Gaume et al., 2015). This highlights the need to combine the present indicator with additional mechanical criteria in future work : a strength-to-stress ratio for failure initiation, thereby satisfying the coupled criterion of Leguillon, 2002 ; a tensile strength criterion for crack arrest and slab detachment (e.g. Gaume et al., 2015) ; or an index comparing the critical cut length to a skier-induced crack or damage length. Such a combined analysis would connect the present work to operational triggering likelihood, and would contribute to providing more effective tools for operational risk management."*.

Overall, I find the manuscript promising and worth publication after revisions. The main improvements I would recommend are to clearly frame wf as an effective PST-based failure/fracture parameter, clarify the role of strength and softening (or fracture energy), possibly compare the present results with previously published stress-based approaches, and extend the discussion on how this approach could be developed in the future to address current limitations. With these revisions, the study would make a valuable contribution by bridging microstructural snow physics, simplified but mechanically interpretable release-process indicators, and operational snowpack modeling. Finally, I am grateful for the opportunity to review this paper and to take part in this open discussion. The manuscript and associated comments, together with the recent renewed interest in shear-based theories in the context of supershear crack propagation, motivated me to revisit some old notes and complete a derivation that I had left aside when the field was shifting strongly toward anticrack mechanics. This resulted in the preprint linked below, which focuses primarily on shear failure while also exploring anticrack propagation, and which I hope may provide a useful complementary perspective on the role of weak-layer strength, elasticity, and finite softening in critical crack length models.

We sincerely thank Johan Gaume for his thorough and constructive comments, which have helped clarify the scope of what is addressed and modelled in this article, and shed light on complementary approaches that would be worth developing in future work.

Références

Adam V, Bergfeld B, Weißgraeber P, van Herwijnen A, Rosendahl PL (2024) Fracture toughness of mixed-mode anticracks in highly porous materials. Nature Communications 15(1) :7379, DOI 10.1038/s41467-024-51491-7

Gaume J, van Herwijnen A, Chambon G, Birkeland KW, Schweizer J (2015) Modeling of crack propagation in weak snowpack layers using the discrete element method. The Cryosphere 9(5) :1915–1932, DOI 10.5194/tc-9-1915-2015, URL `https://tc.copernicus.org/articles/9/1915/2015/`

Gaume J, van Herwijnen A, Chambon G, Wever N, Schweizer J (2017) Snow fracture in relation to slab avalanche release : critical state for the onset of crack propagation. The Cryosphere 11(1) :217–228, DOI 10.5194/tc-11-217-2017, URL `https://tc.copernicus.org/articles/11/217/2017/`

Richter B, Schweizer J, Rotach MW, van Herwijnen A (2019) Validating modeled critical crack length for crack propagation in the snow cover model SNOWPACK. The Cryosphere 13(12) :3353–3366, DOI 10.5194/tc-13-3353-2019, URL `https://tc.copernicus.org/articles/13/3353/2019/`

Schweizer J, Bruce Jamieson J, Schneebeli M (2003) Snow avalanche formation. Reviews of Geophysics 41(4), DOI https://doi.org/10.1029/2002RG000123, URL `https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2002RG000123`, `https://agupubs.onlinelibrary.wiley.com/doi/pdf/10.1029/2002RG000123`