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The Cryosphere Discussions

Interactive comment on "Validating modeled critical crack length for crack propagation in the snow cover model SNOWPACK" *by* Bettina Richter et al.

Anonymous Referee #1

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General comments:

Crack propagation in a weak layer is a key process of slab avalanche release. The crack propagation propensity can be estimated via the critical crack length, i.e. the length of the saw cut in a PST at which the created crack will start propagating. This length can be directly measured in the field using PSTs but also computed from detailed snowpack profiles either measured in the field or simulated by snow cover models such as SNOWPACK.

This paper comprises one of the first direct evaluations of the critical crack length simulated by SNOWPACK with field measurements. The authors conducted their evaluation

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on data spanning on two sites over two to three winter seasons in the region of Davos. On one hand, the authors use the parameterization proposed by Gaume et al. (2017) which relates the critical crack length to the weak layer shear elastic modulus, thickness and strength, and the slab weight, thickness and elastic modulus and applied it to SNOWPACK simulations forced by automatic weather stations. On the other hand, the authors used detailed snow stratigraphy and PST measurements performed on a weekly basis. The weak layer thickness in SNOWPACK is somehow related to numerical stability of the code (solver) and the history of the layer but has no direct relation to the weak layer collapse height, in contrast to the work of Gaume et al. (2017) used for the parameterization. The weak layer thickness measured in the field is related to the manual segmentation of the snowpack into layers and is generally larger than the "collapsible/active" part of the weak layer, due to the limited resolution of manual observations. Besides, the weak layer shear modulus has never been measured so far and must be assumed to be equal to 0.2 MPa. Therefore, the authors show that the direct application of the parameterization from Gaume et al. (2017) on the profiles simulated by SNOWPACK leads to poor agreement with the critical length measured in the field. As an alternative, the authors mainly propose to replace the "ambiguous" concept of weak layer thickness by a fitted function depending on weak layer grain size and density. With this new parameterization, the critical crack length is well reproduced by SNOWPACK with a normalized RMSE of 28%.

Overall, this study is very interesting as it tries to fill the gap between recent advances in snow mechanical modeling and numerical tools that can be used to forecast the avalanche danger. The results are new and exploit a huge, unique and valuable experimental dataset. Nevertheless, the fact that the initial parameterization of the critical crack length using the weak layer thickness as input leads to a poor agreement could have been presented before the final discussion. Indeed, for measured or simulated profiles, this thickness is more linked to the profile resolution rather than to the collapse height. In addition, adding a fitting factor would necessary improve the agreement between the simulations and the experiments. The methods and results are generally

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clearly described. However, the presentation is sometimes clumsy and may benefit from consistent English editing (logic link between sentences, syntax) and some re-organization to make the reading more fluent. I recommend this study for publication pending presentation and content revisions (listed below).

Recommendation : Publication with Major Revisions

General comments/questions :

1) The authors first tried to apply the parameterization of Gaume et al. (2017) using the weak layer thickness of the SNOWPACK simulations or the thickness measured in the field. In Gaume et al. (2017) , the collapse height is directly linked to the weak layer thickness (assembly of spheres in a triangular shape). There is no reason why the resolution of the measured or simulated profile is related to the collapse height. This is effectively discussed in the paper (page 15-16) but too late in my opinion, which might mislead the reader (like me). Please consider some rewriting to announce this idea much earlier in the paper.

2) This article is mainly about crack propagation and compares the measured critical crack length to the simulated one. The experimental data comprises also CT and ECT. In the paper, it is not very clear to me how this specific data is used. I understand from line 4 page 7, that it is only used to detect the weak layer of interest but I feel that the data (Figure 3) is somehow unexploited or too detailed. Moreover, in section 2.2, the stability tests CT, ECT and PST are described with the same level of details. I suggest to reduce the description of the CT and ECT (or exploit it more) and give more details (scheme or photo, for instance) about the PST.

3) The authors associated the observed weak layer to a simulated weak layer based on their respective "birth" date. According to line 11-12 page 4, the "birth" date of simulated layers corresponds to the deposition date and the "birth" date of measured layers to their burial date. I don't understand why this should be the same. For instance, depth hoar might originate from shallow precipitation of the beginning of the

winter (date of birth) which progressively transformed into depth hoar under clear sky conditions and which was buried only some weeks after (date of burial). You need to clarify the matching method between the modeled and measured weak layers.

4) The weak layer density appears as a very important parameter of the critical crack length evaluation. Measuring density of thin and very fragile layers is challenging. Could you please add details on the measurement procedure (e.g. size of the cutter, etc.) and discuss some discrepancies (or no) that may originate from the limited vertical resolution of the cutter (compared to the thickness of the "active" weak layer part).

5) The model was evaluated in terms of probability of detection of the weak layer. The description of the methodology was not clear to me. First, I understood that the weak layer matching was already done with the "birth" date so you may only check whether the global minimum is located close to the associated simulated weak layer? Besides, as explained in the introduction, the stability of the weak layer-slab system is not only controlled by crack propagation propensity but also the sensitivity to trigger a crack (initiation). As the tracked weak layers were also identified by CTs, is it not hopeless to try to identify the observed weak layer only with the critical crack length? Last, it is not really clear to me how the probability of detection is computed. Does it mean that the weak layer is considered as detected when it is located in a band of 10 cm next to five "local" minima (i.e. an overall band of 50 to 30 cm)? Moreover, the term local minimum might be misleading as local minimum already refer to something well-defined (local minimum) and not the fact to "delete" a band of 10 cm in the search of iterative global minimum. According to Fig. 9d, you might consider to rank the real local minima by their prominence.

6) You use Neumann boundary conditions (heat flux imposed?). At WFJ, you also have the possibility to force the surface temperature, don't you? May this a way to get rid of possible errors in the surface energy budget that may cause discrepancies between the measured and simulated r_c, independently of the accuracy of the proposed parameterization? Indeed, you pointed some error (l. 4-8, p. 9) due to the presence/absence

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of melt crusts. Add some discussion on this point.

Technical comments :

1) The abstract needs significant rewriting. It is too approximate and does not give a precise idea of the results. I listed some problems hereafter. The term “data” is used in the text but it is not clear to what it refers (measurements?). I do not get the logic of the sentence “especially if they also provide information on snow instability”. The quantification of stability in terms of initiation, propagation, gliding is never presented. The reader may not understand that r_c is a measure propagation propensity. What was monitored in the experiments? What are the “two variables” (l. 6)? The word PST does not appear in the abstract, although it is the key measurement? The “NRMSE” (l. 8) of what ? What about the role of weak layer density? The algorithm of detection is not “simple”. One sentence on the implications of this study is missing.

2) “snow instability tests” (l.3, p.2 and elsewhere). Please use everywhere where possible “stability” instead of “instability”.

3) l.19, p. 1: Final gliding on the substrate may be added in the key processes of slab avalanche release.

4) l.20, p.1: “A third criterion”. The first and second criteria were not defined in the text here. Besides, the slab propagation support should be presented as a second complementary criterion (in addition to $r>r_c$) for crack propagation.

5) l.21, p.1: “type and location” are not “questions”.

6) l.3, p.2: “data” Do you mean measurements?

7) l.5, p.5: “can only be made”. Too definitive. You can also do more experiments. Reword. “Numerical snow cover model can help increasing spatial and temporal resolution of ...”

8) l.8, p.2: “SCM predicts indices describing the avalanche danger at regional scale”.

9) l.22, p.2: “good agreement”. Can you be more precise?

10) l.27, p.2: “one type of weak layer”. Which one?

11) l.31-33, p.2: the role of weak layer density is also re-inforced by the new paramaterization.

12) l.28, p.3: the mean r_c value of one to five PST tests is used. Why? It migth be worth to show the scatter (error bar?) on Figs. 6 and 10. Besides, individual r_c points are already shown on Figs. 5 and 11.

13) l.8, p.4: "was written for evey day". written -> stored. Can you give details on the exact time (eg. 6:00 UTC) of profile data? Can the comparison to measurements be affected by daily variation?

14) l.20-25, p.4: The shear strength of snow (except SH) is derived from power-law functions of density. Is it the standard of SNOWPACK, or is it a new parameterization? Give details/references.

15) Figure 2: Use international hand hardness code (Fierz et al., 2009; F, 1F, 4F, P, K, I) and explain the meaning in the legend. Is the shown total depth measured or simulated? I suggest to separate (a, b, c) from (d-i) into distinct figures and to SIGNIFICANTLY increase the vertical size of (d-i) and add the same graphs of the stratigraphy for WFJ. Moreover, could you add the density profile on the graphs.

16) Figure 4: I suggest to make a distinct large subplot for the graphs showing D_wl_measured = f(D_wl_simulated)

17) l.6, p.9: "observed weak layers [...] present in the simulated profiles". Currently it is not possible to see SH150124 in the measured profiles (no SH visible in Fig.2 e).

18) l.10, p.9 "Modeled slab". Could you detail somewhere how the slab is defined i.e. all layers above the weak layer (?).

19) l;6-8, p.9: "In the winter, ... degrees". I don’t see a crust in Fig.2f ??? You described

one specific difference between the measured and simulated profile. There are other differences, why did you point this specific one out?

20) Figure 6: Enlarge the figures end use smaller dots for the points.

21) Figure 8: May it possible to express the results in terms of True Skill Score (TSS)?

22) Section 3.4. As far as I understand, the fitting is conducted on the SNOWPACK output and then also applied on the measured profile. Is that correct? Could you please clarify in the text.

23) l.7 p.13 to l.3 p.14: The first paragraph of the discussion belong to the introduction as it is not based on any result presented in this paper.

24) l.4 p16 "while thus far it remains unclear whether the collapse height relates to r_c". Could you give some references on this point? And add some expected trend from the literature?

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