Interactive comment on "Dynamic crack propagation in weak snowpack layers: Insights from high-resolution, high-speed photographys" *by* Bastian Bergfeld et al.

Philipp L. Rosendahl (Referee)

rosendahl@ismd.tu-darmstadt.de, mail@2phi.de

The manuscript presents a methodology for full-field measurements of snowpack displacements using digital image correlation. The work opens numerous possibilities for the extraction of snowpack properties. Among these, the authors discuss ways to obtain an effective homogenized elastic modulus of the slab, the weak layer fracture toughness and the speed of cracks running in the weak layer. The study focuses on the comparison of different methodologies using three representative examples.

The paper makes a significant contribution towards the understanding and characterization of fracture mechanical processes that lead to slab avalanche release. However, I have a serious concern regarding the derivation of the weak-layer fracture energy from SMP signals, denoted w_f^{BR}.

The manuscript cites Reuter and Schweizer (2018) [doi: 10.1029/2018GL078069] for a description of the approach. In this work, however, I find no explanation of the methodology. Instead, I am referred to Reuter et al. (2018) [doi: 10.16904/envidat.40], which, again, does not clarify the procedure. In the accompanying README file I am referred to publication: Reuter et al. (2015) [doi: 10.5194/tc-9-837-2015], Eq. (4). Here, the fracture energy is obtained from the integration of the SMP force signal over certain windows and subsequent selection of the minimum value within the weak layer:

$$w_f = \min_{\mathrm{WL}} \int_{-\frac{w}{2}}^{+\frac{w}{2}} F \mathrm{d}z, \tag{1}$$

where w is the windows size and F the penetration resistance. From the publication I understand that w is of dimension length and F of dimension force (e.g., Reuter et al. (2015) [doi: 10.5194/tc-9-837-2015], Figure 3). This yields units of Nm (energy) for w_f when it should be N/m = J/m^2 (energy per unit area).

The following thought experiment raises another concern about the above equation (1). Imagine we probe the same weak layer (with the same fracture energy) with an SMP of twice or half the original diameter. The former should yield a larger resistance F, the latter a smaller one. Evaluating all three signals (original, double, and half diameter) with the same window size will yield three different fracture toughnesses of the same weak layer. Which one is correct?

Moreover, Reuter et al. (2015) [doi: 10.5194/tc-9-837-2015] refer to Reuter et al. (2013) [url: http://arc.lib.montana.edu/snow-science/objects/ISSW13_paper_O2-02.pdf] regarding the validation of the methodology surrounding the above equation (1). Here, the accuracy of the method is checked using an approach similar to the VH method used in the present work. However, in the present manuscript, the VH method is deemed unfit for the derivation of w_f, for instance because of its inability to model the measured strain energy (Figure 3a) or its inability to correctly account for the slab's Young modulus (Figure 10a). I encourage the authors to comment on this contradiction because I cannot understand the details of the procedure used by Reuter et al. (2013)

since no equations are given.

Concluding my concerns surrounding w_f^{BR}, I specifically ask for clarification of the following:

1. Please explicitly explain (in the manuscript) how the fracture toughness w_f^{BR} is derived from SMP signals including corresponding equations and dimensions.
2. Please comment on the units of w_f^{BR} and Eq. (1) above.
3. Please comment on the issue different probe diameters regarding Eq. (1) above.
4. Please comment on whether I correctly understood the validation of Eq. (1) in Reuter et al. (2013) and the consequential contradiction.

These points should be clarified beyond doubt. If a comprehensive discussion of the methodology goes beyond the scope of the present work, I suggest omitting the SMP methodology for now. After all, its connection to high-resolution and high-speed photography is weak.

Aside from the above crucial points, I only have one other major remark:

5. Since you extract the external potential V_p directly from measured full-field data, Clapeyron's Theorem allows for direct identification of the total potential $V_\mathrm{tot} = V_\mathrm{m} + V_\mathrm{p} = V_\mathrm{p}/2$ and, hence, direct computation of the fracture energy $w_f = -\mathrm{d}V_\mathrm{tot}/\mathrm{d}r = -\mathrm{d}V_\mathrm{p}/(2\mathrm{d}r)$. No fitting to an analytical expression, only some form of signal processing of the experimental data shown in Figure 3a is required to compute the derivative.

Finally, I ask the authors to consider the following minor remarks:

6. The abstract devotes considerable attention to historical developments (lines 10–15) but does not include key findings of the manuscript. I suggest moving the historical perspective to the introduction and add key results such as determined crack speeds and fracture toughnesses – including the respective most suitable techniques for their identification.

7. (line 31) How does process of coalescence of subcritical failures work?

8. (line 47) Please motivate and discuss why and how crack speed is important.

9. (line 60) The touchdown length is not a material constant but depends, for instance, on the slab's bending stiffness and its density ρ. In order to give context to the listed absolute values, I suggest adding additional information.

10. (line 68) The statement is a bit misleading. The fracture energy itself is an independent fundamental material property and independent of other fundamental properties such as the elastic modulus. I assume what is meant is the following: because the method employs a certain model to compute w_f, and the model requires E as and input, the back calculation will change if E changes?

11. (line 130) Can you provide examples of used reference lengths?

12. (lines 157–162) The equation in line 162 only holds if V_m and, hence, also V_p in line 157 are defined per unit width. Please explicitly state (in an equation) how V_p is determined. Is layering considered?

13. (line 175) Equation number missing.

14. (lines 413–414) Can you discuss possible reasons for this discrepancy? How does weak-layer rigidity or compliance affect crack speed?

15. (line 424) Clapeyron's Theorem is a fundamental law of mechanics and should not be brought in context with the limitations of certain models. Instead, I suggest

to explicitly repeat arguments for weaknesses of the VH method that were given around line 301.

Figures and Tables:

17. All images seem to have a low resolution and show compression artifacts. Is this a draft issue?
18. (Figure 1) Images are very small.
19. (Figure 2) Red text on gray picture is hard to read.
20. (Figure 10b) Why does w_f^{RW} decrease with r_{saw}? I would expect the contrary. Is a constant Young's modulus chosen for each data point or does is change alongside r_{saw}? I would suggest to use the "converged" effective modulus from Figure 10a (for both the VH and the RW methods) to calculate the fracture energies in 10b.
21. (Table 2) Again, please check the units of w_f^{BR}.
22. (Table 4) The mean of c^{corr} suffers from (potential) inaccuracies towards the boundaries. Does it make sense to introduce a fourth column where the mean is evaluated on a more reasonable x-domain?

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-360, 2021.