Dear Dr. Rosendahl,

thank you very much for commenting and providing helpful suggestions on the manuscript. Below we have pasted your comments in blue, our point-by-point responses are given in black.

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 wBRf .
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:

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

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 wf when it should be N/m = J/m2 (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 wf , 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 wBR f , I specifically ask for clarification of the following:

1. Please explicitly explain (in the manuscript) how the fracture toughness w^{BR}_{f} is derived from SMP signals including corresponding equations and dimensions.

We regret that information was missing to understand the calculation of fracture energy derived from SMP measurements. We will include the necessary information in the Methods section of the revised manuscript:

"As a third approach, we used SMP measurements. Effective elastic modulus E_{sl}^{BR} was derived from SMP data as described by Reuter and Schweizer (2018), using the signal interpretation method suggested by Löwe and van Herwijnen (2012). Reuter et al. (2013) suggested a parametrization of the specific fracture energy based on the penetration resistance F(z). Using a moving window (size: w = 2.5 mm) to integrate F(z), they then defined the specific fracture energy as the minimum of the integral within the weak layer:

$$w_f^{\mathrm{SMP}} = A\ \min_{\mathrm{WL}} \int_{-w/2}^{w/2} F\,dz\ , \qquad (2)$$

where A is a fitting parameter. The integration has units energy (J) and relates to the work required to destroy the snow structure along the integration path. Specific fracture energy, however, has unit energy per area. Therefore, it is necessary to divide by an effective area, the fitting parameter A. While the effective area is unknown, it is likely larger than the cross section of the tip diameter (Johnson, 2003), and depends on snow structure(van Herwijnen, 2013) We therefore followed Reuter et al. (2019), and introduced a fitting parameter A to implicitly account for the unknown effective area. The fitting parameter was derived using a linear regression to PTV derived specific fracture energies (Figure 6 in Reuter et al. (2019)), resulting in $A = 2.95 \times 10^3 m^{-2}$). Which relates to a plausible effective cone area of 3.4 cm^2(radius ≈ 1 cm)."

2. Please comment on the units of w^{BR}_f and Eq. (1) above.

The unit discrepancy originated from the absence of the fitting factor *A* (see above, equation 2 in the reply to your comment 1). Its physical meaning and derivation will explicitly be stated (see reply above).

3. Please comment on the issue different probe diameters regarding Eq. (1) above.

With the fitting parameter A, accounting for the effective area, the equation to derive the fracture energy from SMP data now accounts for the probe size (see reply above).

4. Please comment on whether I correctly understood the validation of Eq. (1) in Reuter et al. (2013) and the consequential contradiction.

Given our reply above we hope that the issue is now clarified.
With the fitting parameter we introduced in Equation 2, it becomes clear now that the comparison to PTV-derived values in Reuter et al. (2013) and Reuter et al. (2019) served to parametrize the specific fracture energy on SMP signals. Reuter et al. (2013) and Reuter et al. (2019) showed a comparison of SMP- and PTV-derived values of the specific fracture energy. Rather than validating the accuracy of the SMP method, they discussed differences between different measurement methods. Currently lacking an alternative method for calibration, we used their PTV data to determine the factor *A* in equation 2.
Based on your comment 12 below, we applied corrections to the VH method (Fig 3a), which improved the fit of the mechanical energy. Nevertheless, the SMP-derived values are based on a parameterization derived from a linear regression with PTV-derived values. Once enough data are available, we will derive a parameterization using DIC data, or μCT data, or other future techniques, which may possibly provide more accurate w_f data. We deem it valuable to provide the BR estimates and to compare them to more elaborate methods. Only then we know how the method performs and we can possibly calibrate the SMP approach to the best method in the future. For lack of alternative, the SMP currently remains the only

efficient experimental method to determine snow mechanical properties, such as the specific fracture energy, at many locations in the field.
These points will be discussed explicitly in the in the revised version of the manuscript.

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.

We agree that the main message of the paper is the potential of high-resolution DIC measurements. As the SMP is a relatively widely used measurement method, we decided to keep the SMP results. Moreover, we deem comparisons with exiting methods good practice. However, we substantially reduced the importance of the SMP derived values by only mentioning those in the text, and not in the figures anymore, and we explained the derivation of the fracture energy in greater detail, as mentioned above.

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

5. Since you extract the external potential Vp directly from measured full-field data, Clapeyron's Theorem allows for direct identification of the total potential Vtot = Vm + Vp = Vp/2 and, hence, direct computation of the fracture energy wf = dVtot/dr = dVp/(2dr). 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.

Thank you for your suggestion. As Heierli's formulation of the mechanical energy did not represent the measured data very well, we followed your suggestion by fitting an arbitrary function to our data. We only had two constrains for the function: 1) it should have a value of 0 for $r = 0$, and 2) the function should be monotonically decreasing with r. We used a simple power law function of the form $f(r) = -ar^b$ (FU) and refined the fitting window to
15 cm < $r_{\max}$ < r_c.
In the end, we assessed the quality of the VH and FU fit with the root mean squared error and found that the simple power law function FU represented the measured data better ($\mathrm{RMSE}^{FU} = 0.007$, $\mathrm{RMSE}^{VH} = 0.013$).
We will hence introduce the power law fit into the revised manuscript.

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.

We agree that the historical perspective was rather prominent and will revise the Abstract.

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

Coalescence of subcritical failures is part of our conceptual understanding of natural avalanche release (Schweizer et al., 2016). The formation of subcritical failures occurs at the microscale (scale of snow crystals and bonds, <<1 mm). At this scale two competing processes occur simultaneously (Capelli et al., 2018a): 1. Weak layer damage (meaning the breaking of bonds), and 2. Weak layer strengthening/sintering (meaning creation and strengthening of bonds). When the damage process dominates, more bonds break and a localized failure may develop, i.e. subcritical failures coalesce.

This damage process, aka failure events (bond breaking), manifests itself by acoustic emissions (Capelli et al., 2018b)

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

We will motivate the importance of crack speed by pointing out that crack speed is an indicator of the crack propagation mode and may provide insight into an ongoing discussion about crack propagation in snow.

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.

Indeed, the touchdown length is not a material property. We will provide the range of slab densities and slab thickness reported in Bair et al. (2014).

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 wf , and the model requires E as and input, the back calculation will change if E changes?

We agree with the reviewer that the statement was unclear. We will therefore reword the sentence to: *"This emphasizes a weakness of the method, the back-calculated specific fracture energy relies on the input of the elastic modulus. A parameter that contains large uncertainties, especially if it cannot be determined in-situ."*

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

In all tests we acquired an image with a 2 m reference length fixed on the PST side wall. We will explicitly mention this.

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

We thank the reviewer for pointing out this mistake. Indeed, we did not define V_p per unit width, resulting in wrong estimates of the elastic modulus and specific fracture energy of the VH method. We will correct this error and make the necessary changes throughout the revised manuscript.

13. (line 175) Equation number missing.

We will add the equation number.

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

In Heierli (2005), speed is proportional to the 4th root of the bending stiffness of the slab, which itself is directly proportional to the elastic modulus. Computing the speed with the VH and RW elastic modulus gives values of c^{VH} = 25 ms^{-1} and c^{RW} = 35 ms^{-1}.
The model of Heierli assumes free fall motion of the slab during weak layer collapse, and therefore does not consider weak layer properties. Accounting for weak layer rigidity would therefore likely reduce the speed estimates.

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.

We will not mention Clapeyron´s Theorem anymore by changing the statement to:
*"Their (elastic modulus of RW method) estimation was stable with increasing cut length (**Fehler! Verweisquelle konnte nicht gefunden werden.**a) and the visual similarity between the experimentally determined data and the applied model seems to be good (**Fehler! Verweisquelle konnte nicht gefunden werden.**)."*

Figures and Tables:

16. All images seem to have a low resolution and show compression artifacts. Is this a draft issue?

We will improve resolution.

17. (Figure 1) Images are very small.

We will enlarge the three images with the limitation to fit everything in a single line. Since the intention of the figures is to present a schematic workflow of the processing, it was more important for us to keep a single line instead of showing the steps in detail.

18. (Figure 2) Red text on gray picture is hard to read.

We agree and will improve the figure.

19. (Figure 10b) Why does w^{RW}_f decrease with rsaw? I would expect the contrary. Is a constant Young's modulus chosen for each data point or does is change alongside rsaw? 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.

We do not see why the contrary should be expected. With a "perfect measurement" and a "perfect model" we would not expect any trend. Since this is, however, never the case, we investigated how strong the elastic modulus (moduli) varies when changing the fit interval ($r < r_{saw}$, VH method) or when taking another measured displacement field ($r = r_{saw}$, RW method). In a further step the weak layer fracture energies w_f are derived from the elastic properties as:

$$w_f^{\mathrm{VH}} = -\frac{d}{dr} V_{\mathrm{m}}\Big|_{r=r_c} ; \; w_f^{\mathrm{RW}} = G_{\mathrm{I}} + G_{\mathrm{II}} = \frac{E_{\mathrm{wl}}^{\mathrm{RW}}}{2t} w_{\mathrm{RW}}(r = r_c)^2 + \frac{\mathrm{E}_{\mathrm{wl}}^{\mathrm{RW}}}{2t(\nu_{\mathrm{wl}} - 1)} u_{\mathrm{RW}}(r = r_c)^2$$

Therefore, the variation of elastic properties propagates into w_f^{VH} and w_f^{RW}. To illustrate that, Figure 10b shows how the variation of the elastic moduli affects the derived values of w_f.

Using a "converged modulus" would result in one specific fracture energy for each method. These energies are basically already given as the data points with largest r_{saw} in Figure 10b.

To avoid misunderstandings, we will mention this explicitly:
"*Of course, to derive w_f both models are evalutated at the critical cut length r_{saw} = r_c, but the computation of w_f is based on E_{sl}(and E_{wl} for the RW method), and E_{sl} is sensitive to changes of the fit interval (r < r_{saw}, VH method) or when taking another displacement field (r = r_{saw}, RW method).*"

20. (Table 2) Again, please check the units of wBR f .

This should now be clarified given our reply to your comment 2 above.

21. (Table 3) 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?

We are unsure whether you suggest to introduce a fourth column because you may oversee the last line in Table 3, in which the mean crack speed away from beam edges (1 m < x < 2 m) is shown.
Or, do you question if the last line in Table 3 is meaningful at all?
In that case we think it is useful to separate two regions and state mean crack speeds within. Our discussion also considers drivers of crack speed changes within these two regions and the regions are:
1. Close to beam ends, where strong edge effects are to be expected.
2. The middle part of the beam, where edge effects are less pronounced but still present as long as the crack is not in a steady state propagation.

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