Answers to reviewer comments 1

We greatly appreciate the reviewer's constructive comments and address them one by one below. Changes in the manuscript are referenced.

In "Shear failure of weak snow layers in the first hours after burial" by Reuter et al., fracture mechanical properties of persistent and nonpersistent weak layers were measured in a cold room laboratory using shear frames, particle tracking, and a snow micropenetrometer. Note that nonpersistent can be hyphened or not, in my view. I enjoyed reading this manuscript and, as someone who is keenly aware of the bias towards the study of persistent weak layers in snow science, I appreciate the focus on nonpersistent weak layers. As the authors point out, failures on nonpersistent weak layers are less common in avalanche fatalities, although I think it's entirely possible that there are more avalanches on nonpersistent weak layers than on persistent weak
layers worldwide. Thus, I find the motivation of this study excellent. I found the manuscript well written and the experimental approaches and findings well supported. Most of my critiques are minor and are included as an annotated PDF.

In the following we refer to the reviewer's annotations in the submitted manuscript (by those line numbers).

Line 22: Changed as suggested.

Line 29: We changed one instance to be consistent with the term "non-persistent".

Line 31: We agree with the reviewer that non-persistent grain types refer to precipitation particles only. The ICSSG (Fierz, et al. 2009) contains two groups: precipitation particles (group PP) and decomposing fragmented precipitation particles (group DF). We now use the exact term "decomposing fragmented precipitation particles". Besides, we corrected a misleading statement on equi-temperature metamorphism by adding: "... once they are buried and temperature gradients are weak".

Line 38: We deleted a detail on strength differences between grain types. Decomposing fragmented precipitation particles (DF) and precipitation particles (PP) were slightly stronger compared to faceted (FC) and depth hoar crystals (DH) in Jamieson and Johnston (2001). However, we believe that field measurements may possibly not reveal the lowest possible values of strength.

Line 51: We inserted: "such as modulus or strength"

Line 80: We will be specific when required such as in line 51.

Line 96: The spray painted dots track the motion of several, but not individual grains. The error is dependent on the pattern size and shape, which determines how well the digital image correlation method can relate the patterns between subsequent images. Hence, a general statement on the uncertainty of the strain measurement is not possible. Nevertheless, Table 1 provides the standard deviation of our measurements for different types of weak layers. These values contain measurement and experimental errors plus temporal effects – hence, they provide a more conservative error estimate. We explain this in Section 3.4.

Line 119: Experiments were performed using snow that had been stored at a low temperature such that metamorphism was reduced and snow structure did not evolve significantly during storage. Snow was then disaggregated and sifted to form a snow slab for our experiments. We changed the wording to make clear how snow was stored and slabs were formed.

Line 131: Our setup had the advantage of having four samples with very similar weak layer properties in one box allowing in situ microstructural characterization with SMP and CT measurements which is crucial for time series. Moreover, the described loading apparatus controls force while our experiments were displacement controlled.

Line 145: Changed as suggested.

Line 157: Changed as suggested.

Line 173: The linear elastic limit is considered the region where the stress levels off and does not longer increase at the initial rate despite further displacement increase. Deformation beyond the linear elastic limit is not recoverable. See also comment on Line 231.

Line 202: The frame rate is stated in section 2.1.4 Data acquisition

Line 211: In fact, we have always varied the lower and the upper boundary to obtain an error estimate. We changed the description.

Figure 3: The black spaces are areas where the fins of the shear frame with the open side wall shines through which is particularly visible in the NIR pictures.

Line 231: The elastic limit was located where a smoothing local regression deviated from the initial slope in the stress - strain graph. We agree that the determination is somewhat subjective depending on the regression type. Given the scatter in both, the force and the strain measurements, a precise localization in different measurements seems difficult. Please see also the response to the first reviewer comment.

Table 2: We changed the wording in the table caption to be clearer. The average value and the standard deviation from the average value are shown for every weak layer property.

Line 255: Here we comment on shear fracture toughness. Jamieson and Johnston (1990; 2001) and Roch (1966) did not test surface hoar, nor did they measure toughness. The values we obtained were lower than previously reported values and for the first time a comparison with surface hoar was possible. Moreover, strength and toughness are not necessarily related (e.g. Richtie, 2011, doi: www.nature.com/doifinder/10.1038/nmat3115).

Line 274: Changed.

Figure 6 (now 7): We are not sure about the reason, but it is possible that sintering also increased the strain concentration in the weak layer due to an increasing elastic mismatch between the slab and the weak layer in the very early stages of our time series. However, we did not observe a significant change in strain concentration with time (line 296, section 3.4).

Line 316: Changed as suggested.

Line 326: We updated the citation.

Line 368: We actually cite two different references here.

Line 370: We observed the average of the maximum extension.

Line 403: Wording improved.

Line 414: We thought about doing so, but it was not feasible with our setup. Even if the failure strain would be estimated a priori, we did not control strain in the experiments, but retrieve it from the images in a later DIC analysis. Unloading also requires a load actuator that can reverse faster than it loads. We ran the measurements, however, already at the highest possible displacement rate to achieve strain rates preferably above the ductile to brittle transition.

Line 423: We agree with the reviewer's comment.

Line 441: This statement refers to the average increase observed in all seven measurements.

Line 478: Please see reviewer comment 3.

Line 496: Please see reviewer comment 4.

In the following we respond to the reviewer's comments.

1) It's not clear how the linear elastic limit was determined. From what I can tell, it

was visually estimated from the stress/strain curves. Given the scatter in these points, there is substantial uncertainty in this estimate. That uncertainty is not reflected in the results, i.e. Table 2.

The elastic limit was located where a smoothing local regression deviated from the initial slope in the stress - strain graph. Both, the scatter of the force and the strain measurements add up to an error when determining the linear elastic limit. Aware that the determination of the elastic limit has limited precision, we provide in Table 2 the standard deviation of the average from up to eight measurements for each weak layer type. This error estimate includes, measurement errors, experimental errors possibly due to frame placement etc, and effects of temporal evolution due to sintering. Hence, the error estimate provided in Table 2 is rather conservative and does include the measurement error.

2) Although I appreciate the focus on nonpersistent weak layers, it is not a new finding that these weak layers are as weak or often weaker in shear than all the other types. Roch (1966) and Jamieson and Johnston (2001) have both previously showed this.

We agree with the reviewer that previous research has shown that the shear strength of precipitation particles is low compared to other grain types, and we refer to that in the paper. However, the mentioned references have not reported measurements of surface hoar, neither did they investigate shear modulus or fracture toughness, as was done in this study. Thus, it is reasonable to highlight that according to our measurements of shear modulus and fracture toughness decomposing fragmented precipitation particles and surface hoar particles were similarly fragile (see Abstract and Conclusions).

3) Likewise, the authors suggest that "Conditions for sufficient strengthening, when decomposing fragmented particles are no longer prone to fail, could be studied in the future." Although this hasn't been done in a laboratory, there is lots of evidence about waiting for a day or two for nonpersistent weak layers to strengthen. For instance, in Bair (2013) I found that waiting 24 hr after new snowfall dropped the number of avalanches to a median value of zero. The result is significant at $p < 0.01$ for over 1000 days of avalanche control work with explosives at a ski area where 87% of avalanches fail on nonpersistent weak layers.

The suggested reference provides field evidence on new snow instabilities and was included. Conditions for weak layer strengthening will depend not only on time, but probably also on initial microstructure, temperature conditions or overburden. Given the different drivers and conditions it seems worthwhile to investigate the efficiency of sintering to increase snow strength and eventually mitigate the different avalanche problems.

4) Making the data available on request does not satisfy The Cryosphere's data policy (https://www.the-cryosphere.net/about/data_policy.html#data_availability). I suggest adding them as a supplement or putting them in a publicly accessible repository.

Data will be publicly available at a DOI, once the review process is finished.

Overall, these criticisms are minor and I would recommend publication after addressing them.