We thank Karl Birkeland and Scott Thumlert for their feedbacks and for their constructive comments that helped us to improve the quality of our paper.

We revised our paper in order to account for their remarks and we provide below detailed answers to the various issues raised by the reviewer.

Reply to Referee #1 (K. B. Birkeland)

Comment 1). *Page 4834, Line 15: Change "well produced" to "did well reproducing".*
Answer to comment 1). This will be done.

Comment 2). *Page 4836, Line 11: Change to "stress concentrators".*
Answer to comment 2). This will be done.

Comment 3). *Page 4838, in the discussion of the methods, I think the authors should mention here that this method will not discriminate between snowpacks with similar layers, but a different order of layers. the authors later discuss this in several points of the paper, but I first noticed this here and it would be good to have that point acknowledged in the methods.*
Answer to comment 3). We agree and thus a small new paragraph will be added at the end of paragraph 2.1 in order to mention this limitation already in the Methods section.

Comment 4). *Page 4843, Line 20, Isn't the slab almost always more rigid than the weak layer? In the field I cannot think of a case where the weak layer was more rigid than the slab. Perhaps re-word this sentence to indicate that?.*
Answer to comment 4). We agree that, in general, the layer just above the weak layer is harder than the weak layer. The notion of slab in this case is a bit misleading since it simplifies the layered system above the weak layer into one single layer, the equivalent slab. Yet, the hardness of the slab layers is not uniform. Hence, for instance, a combination between a hard slab and soft, new snow might lead in the end to a lower elastic modulus for the equivalent slab than for the weak layer according to Eq. 2. This is also due to the fact that the elastic modulus is parametrized as a function of density only (Eq. 11) without accounting for snow microstructure. Hence, new snow being less dense than weak layers of faceted crystals or depth hoar, the elastic modulus (according to Eq. 11) of a soft slab is generally lower than that of the weak layer. We also believe that this is probably due to the fact that the data we use were recorded at low elevation below the tree-line and on gentle slopes leading to rather soft slabs. A dataset recorded at higher elevations would very likely lead to significantly higher values of E_e. The same issue was reported by Habermann et al. (2008) who used a similar dataset from the same area.

In summary, we will rewrite this sentence and refer to "the equivalent slab (Eq. 2)" instead of the slab and provide a more detailed discussion in the Discussion section where this issue was already mentioned.

Comment 5). *Page 4843, Line 24, Delete the word "using".*
Answer to comment 5). This will be done.

Comment 6). *Page 4844, Line 9, I am hoping this point will be discussed further, but that can probably be saved for the discussion..*
Answer to comment 6). This is also a concern from reviewer # 2 and we will discuss the implications of this limitation further by adding a new paragraph at the end of the first paragraph of the Discussion section.

Comment 7). *Page 4845, Line 3-4, Replace "more than 2 times smaller" with "less than half".*
Answer to comment 7). This will be done.

Comment 8). *Page 4847, Line 6, Replace "deep" with "from the surface".*
Answer to comment 8). This will be done.

Comment 9). *Page 4848, Lines 22-25 and Page 4850, Liens 17-18, My main comment for this paper is that I would like to see this limitation of the study discussed in more details. What do the authors think about this limitation? And what are the implications of this limitation for their results. It would be interesting to address this both in the discussion and perhaps in the conclusions as well.*

Answer to comment 9). We will discuss the implications of this limitation at the end of the first paragraph of the Discussion. We will illustrate this point by using the results of Figure 5: This aspect is particularly limiting for profiles with a hard substratum and a slab for which the hardness decreases with increasing depth (e.g. Fig. 5b and 5g). In this case, which might occur with increasing wind speeds during a snowfall, our approach overestimates the stress due to a skier at the depth of the WL. Hence, our new stability index would be lower than the one predicted using the finite element method (FEM), for instance. These statements will be added in the Discussion and this point, which was already mentioned in the Conclusions, will be highlighted more in the conclusions by adding a new sentence to detail in which cases this limitation matters.

Reply to Referee #2: Scott Thumlert

Comment 1). *Page 4835, Line 17, The reference McCammon and Haegeli, 2007 refers to an analysis of "rule-based decision tools for travel in avalanche terrain". Suggest a reference more directly appropriate to the statement (e.g. Jamieson, B., Haegeli, P., Gauthier, D., 2010. Avalanche accidents in Canada or similar American reference such as Tremper, B., 2008. Staying alive in avalanche terrain).*

Answer to comment 1). Thanks for this suggestion. The reference to McCammon and Haegeli, 2007 will be replaced by Jamieson et al. (2010).

Comment 2). *Methods 2.1 and 2.2: The methodology described on page 4838 lines 17 24 and page 4839 lines 1 15, details how a multilayered slab and weak layer system are generalized into a single layer. The substratum (below weak layer) does not appear to be included in the generalization. Yet, the results shown in Figure 5 include two different substratum types (hard and soft) with differing stress results. Page 4839 lines 11 15 describe how the influence of the substratum is accounted for, but this it is not entirely clear. Could the authors please provide more detail on exactly how xzml was calculated for the substratum depths in Figure 5?.*

Answer to comment 2). We agree that this should appear already in paragraph 2.1. We will add how the additional stress is computed in the middle of the weak layer according to:

$$\Delta\tau_{xz}^{ML} = \frac{155}{2}\left(\frac{1}{h_{e,n}} + \frac{1}{h_{e,n+1}}\right) \tag{1}$$

where $h_{e,n}$ is the equivalent depth of the n slab layers above the weak layer and $h_{e,n+1}$ the equivalent depth of the $n+1$ slab and weak layers above the substratum.

Comment 3). *Page 4844 lines 9 - 11 state "However, our approach can obviously not discriminate between the profiles with upper layers having the same equivalent elastic modulus (Eq. 2) but a different order of the layering (Fig. 3b, c, g, and h)." The results for xzml presented in Figure 5 profiles b and c / g and h appear to show differing stress levels as depth is decreasing. However, the profiles should have equivalent elastic modulus, but differing orders. Thus, Figure 5 shows that your approach is indeed taking into account the order of layering in the slab! It appears as though a calculation of xzml for a single depth in the snow cover would not discriminate between the layer ordering, but performing xzml for every depth in a snow cover would indeed account for the layering. Perhaps more detail on the exact calculations for Figure 5 would help clarify? This is also stated in the discussion on page 4848 lines 22-25.*

Answer to comment 3). If we look at the value of the additional stress $\Delta\tau_{xz}^{ML}$ for a layer $n+1$, it will be exactly the same if the upper n layers have the same equivalent modulus E_e but a different order of layering. This is why the value of k is exactly the same **at the depth of the weak layer** for profiles b and c ($k = 0.48$) and for profiles h and g ($k = 0.99$). However, the overall stress profile is indeed different (except at the depth of the weak layer) because the slab layers are different. For example, if we take profiles b and c and we compute the additional stress at the interface between the two first layers, the equivalent elastic modulus is the elastic modulus of the first layer, which is different for profiles b and c and thus leading to different values. We will clarify in the text that the additional stress is the same for profiles with the same equivalent modulus but different orders of layering **only** at the depth of the weak layer.

Comment 4). *Page 4840 lines 15 22. Shear strength (I,II from equation 6) has been shown to increase with increasing load above the layer (e.g. Zeidler and Jamieson, 2006a and b). i.e. the weak layer shear strength increases as the layer is buried deeper with pressure sintering and time for metamorphism. This generally leads to larger values of I,II with increasing depth. Also, density typically increases with depth into the snow cover. Thus, the xz = ghsincos in most cases will increase with increasing depth. Calculating skier stability indices that account for layering, should include the density of the layers when calculating stress from the slab above the weak layer (xz) and include estimates of I,II based on the layering above the weak layer.*
Answer to comment 4). We certainly agree. In fact, these aspects were accounted for in our approach:

- the average density of the slab is computed from the slab layer densities according to: $\rho = 1/h_{tot} \sum_{i=1}^{n} h_i \rho_i$.
- the weak layer shear strength also depends on the weak layer density (which generally increases with overburden) according to the power-laws proposed by Jamieson and Johnston (2001). This was accounted for in the comparison to simulated snow profiles (Figures 7, 8, 9 and 10) but a constant shear strength was assumed for the parametric analysis (Figure 6) and for the simplified profiles (Figure 5).

These two aspects will be clarified in the revised manuscript.

Comment 5). *Suggest the above be left out of the calculations for Figure 6 for reasons of clarity and simplicity, but be discussed similar to what was done on page 4845 lines 6-9. But, I do suggest these be included in the comparisons to the 160 manually observed profiles. The manual profile data should provide enough information to include good estimates of both xz and I,II.*
Answer to comment 5). The change in shear strength with increasing slab depth was indeed not accounted for in Fig. 6 for reasons of clarity. In general the WL shear strength is parametrized with the weak layer density (Jamieson and Johnston, 2001). As suggested, we will discuss this aspect as we do for slab Young's modulus and slab density. However, note that for the comparison with manual profiles, the shear strength of the weak layer has been directly measured using a shear frame as stated (page 4841 lines 20-21).

Comment 6). *Further, I am not sure if the SNOWPACK model uses estimations of weak layer shear strength in the stability index calculations, but surely this would be a valuable improvement if not. After reading page 4847 lines 27 28 and page 4848 lines 1-3, it appears as though SNOWPACK does calculate slab induced stress from the layering above the weak layer.*
Answer to comment 6). The shear strength parametrizations proposed by Jamieson and Johnston (2001) are implemented in SNOWPACK (Lehning et al., 2004). One parametrization concerns persistent weak layers while the other one concerns non-persistent weak layers.

Comment 7). *Page 4846 lines 13 19: I think there is a problem here or some clarification is needed. As observed in the field, the weak layer is almost always softer than the slab. How can more than 50% of the data show lower values of the equivalent slab modulus Ee compared to the weak layer modulus?.*
Answer to comment 7). Please refer to the answer to comment 4 raised by referee # 1.

Comment 8). *Figure 7: Some of the data classified as good observed stability show predicted stability from both skier stability indices near 0! Could the authors provide some explanation of these specific discrepancies? Is there a common trend with these discrepancies that could useful to understand? .*
Answer to comment 8). We chose to attribute cases with RB$\geq$5 to the stability class "good". For RB=5 and RB=6, three cases with very low stability indices (< 0.3) were recorded. This is indeed surprising and seems to be due to very low values of the shear strength (< 300 Pa) or very thin slabs ($D < 0.3$ m). However, we decided not to remove these outliers from the data for reasons of transparency.

Comment 9). *Figure 7: It is difficult to observe the difference between SKML38 and SK38 for the predicted stability. Perhaps the figure fonts can be improved to highlight the difference more effectively? .*

Answer to comment 9). Figure 7 was modified accordingly and is provided below:

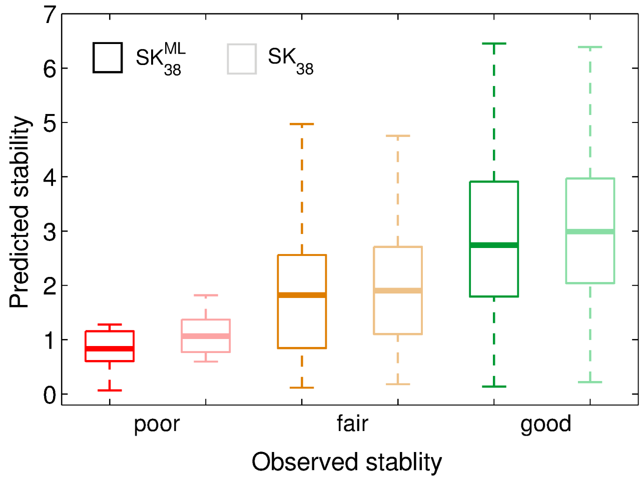

Figure 1: SK_{38}^{ML} (left) vs. SK_{38} (right) predicted stability distributions per observed stability class (poor: manual profiles with RB scores 1 and 2, $N_{poor} = 10$; fair: manual profiles with RB scores 3 and 4, $N_{fair} = 53$; good: manual profiles with RB scores 5, 6 and 7, $N_{good} = 97$). Boxes span the interquartile range from 1st to 3rd quartile with a horizontal line showing the median. Whiskers show the range of observed values that fall within 1.5 times the interquartile range above and below the interquartile range.

Comment 10). *Figure 8: It would be appropriate to show the whisker ranges for the boxplots (i.e extend the graph y-axis lower than 0.6) .*

Answer to comment 10). Figure 8 was modified accordingly and is provided below:

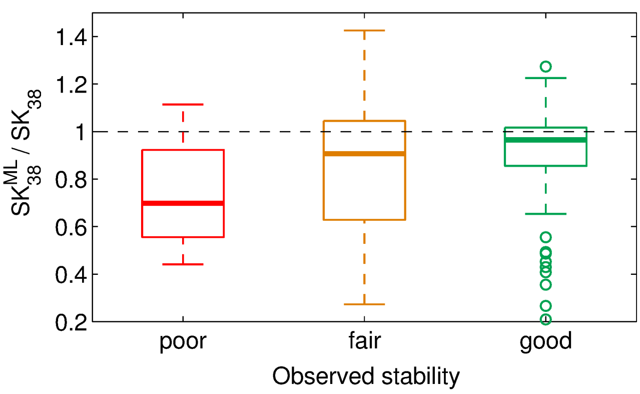

Figure 2: Distributions of the ratio between SK_{38}^{ML} and SK_{38} per observed stability class (poor: manual profiles with RB scores 1 and 2, fair: manual profiles with RB scores 3 and 4, good: manual profiles with RB scores 5, 6 and 7). Four outliers (>1.5) not shown.

Comment 11). *Figure 6a: I believe the caption should show SK38ml to match the y-axis in the Figure. .*

Answer to comment 11). Thanks, this will be modified.

Comment 12). *Page 4849 lines 6 7. Minor grammatical change. The sentence should read: The dataset we used, collected in the Columbia Mountains of western Canada, was not the most appropriate for our purpose. OR The dataset we used was collected in the Columbia Mountains of western Canada and was not the most appropriate for our purpose. .*

Answer to comment 12). Will be changed as suggested.

References

Habermann, M., J. Schweizer, and B. Jamieson, 2008: Influence of snowpack layering on human-triggered snow slab avalanche release. *Cold Reg. Sci. Technol.*, **54**(3), 176–182.

Jamieson, J. and C. Johnston, 2001: Evaluation of the shear frame test for weak snowpack layers. *Ann. Glaciol.*, **32**, 59–69.

Lehning, M., C. Fierz, B. Brown, and B. Jamieson, 2004: Modeling snow instability with the snow-cover model SNOWPACK. *Ann. Glaciol.*, **38**(1), 331–338.