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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Snow instability evaluation: calculating the skier-induced stress in a multi-layered snowpack" *by* F. Monti et al.

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General comments: This paper was a pleasure to read. The organization of the methods, results and discussion made the paper very easy to read and presented the study very well. The inclusion of layered snow cover in the skier stability index will likely improve the index and when implemented in the SNOWPACK model should provide more accurate snow cover stability modeling. There are a few minor changes and some important clarifications that I suggest being done prior to publication.

Specific comments: Page 4835, Line 17: The reference McCammom 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.,

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Haegeli, P., Gauthier, D., 2010. Avalanche accidents in Canada or similar American reference such as Tremper, B., 2008. Staying alive in avalanche terrain).

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 $\Delta\tau$xzml was calculated for the substratum depths in Figure 5?

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 $\Delta\tau$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 $\Delta\tau$xzml for a single depth in the snow cover would not discriminate between the layer ordering, but performing $\Delta\tau$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.

Page 4840 lines 15 – 22. Shear strength (τI,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 τI,II with increasing depth. Also, density typically increases with depth into the snow cover. Thus, the τxz = ghsinΨcosΨ in most cases will increase with increasing depth.

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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 (τxz) and include estimates of τI,II based on the "layering" above the weak layer.

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 τxz and τI,II.

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.

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?

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 lto understand?

Technical corrections:

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?

Figure 8: It would be appropriate to show the whisker ranges for the boxplots (i.e.

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extend the graph y-axis lower than 0.6)

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

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.”

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 3, 4833, 2015.