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The Cryosphere Discussions

Interactive comment on "Analysis of the snow-atmosphere energy balance during wet-snow instabilities and implications for avalanche prediction" *by* C. Mitterer and J. Schweizer

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This is an interesting paper that utilizes measured and modeled energy balance components to better explain and predict wet snow avalanche activity. I especially like the idea of looking at the cold content of the snow since an isothermal or near isothermal snowpack is often a prerequisite for wet snow avalanche activity. Though this particular approach of using energy balance components is new for this problem, I was disappointed that the authors did not attempt to somehow integrate data on the snowpack structure (either measured or modeled with SNOWPACK) to better explain the wet snow avalanche activity since the snow structure is critically important.

My biggest question with this paper is whether the authors were looking at wet slab or wet loose avalanches? Or were they looking at both? I think that this needs to be clearly defined in the paper because wet slab and wet loose avalanches are so different. Though both of them require free water from either rain for snow melt, the snow structures required for each differs, as does the amount of water needed to initiate avalanche activity. I am assuming that the authors are combining the two types of avalanches in their analyses. If so, they should justify this and discuss how this might affect their results and conclusions.

My second main point concerns the selection of the non-avalanche days. The paper says that there were 663 non-avalanche days and 66 avalanche days. It sounds like the non-avalanche days were selected so that they represented the same months proportionally as the avalanche days (p. 2723, line 8). However, the data show that the avalanche days only occurred in March and April (Table 1). So, are all the non-avalanche days from March and April? If that is the case, then there should be far fewer than 663 non-avalanche days from which to choose.

Along those lines, I don't understand Figure 1b. The N-size for this figure is 1394. Is this how many total non-avalanche days there are? Why does this vary from the 663 figure? Also, I don't think it's appropriate to show this figure. Of course there will be a strong discrimination between the two datasets when you use all the non-avalanche days, but that is because you are now comparing your relatively wet spring snowpack to the drier mid-winter snowcover that exists on many of the non-avalanche days. So, I would suggest removing that figure.

My third point concerns the Discussion. In my opinion, the discussion is too long and detailed, and it does not focus on the main points of this paper. I would suggest rewriting the discussion to make it shorter and more to the point. It should also focus on this work, its significance and its place in the literature. One point in the discussion that I find a bit puzzling is that the "incoming longwave radiation tended to be higher on event days than non-event days" (p. 2730, line 26). This suggests that the wet snow avalanche activity was higher on days with clouds than days that were clear. This seems strange given that incoming shortwave is often a driver for snowmelt and subsequent wet snow avalanche activity where there is not a rain-on-snow event. Do the authors have an explanation for this? Are a large number of the avalanches in their dataset due to rain-on-snow during cloudy conditions?

One point that is not emphasized in the discussion is the importance of snow structure in wet snow avalanches. I think this point needs to be made and it can help to explain some of the discussion. For example, at one point the comment is made that "too many non-avalanche days snow the same behavior and have a high production of water" (p. 2731, line 11). Of course, this is likely due to changes in snow structure. Some snow structures would be quite responsive to the addition of water, while more "mature" spring or summer snowpacks that have undergone some melt would not be likely to avalanche with the addition of more water.

In the conclusions I think it would again be useful to discuss the importance of snow structure for the wet snow avalanche problem.

Some other points:

Page 2716, line 2: Again this comes back to whether or not we are looking at wet slab or wet loose avalanches. For deep wet slabs, we probably do not have in-situ tests. However, for wet loose Trautman et al. (2006 ISSW proceedings) proposed using shear frames to look at the cohesiveness of the near-surface layers.

Page 2718, line 22: I don't understand the Swiss system of recording avalanches, but is there no location identified for each avalanche? From the paper it appears that you have the elevation for the starting zone for each avalanche, but no aspect. Is this the case? If you had the location, I would think you could easily figure out the aspect for each slide.

Page 2718, line 23: It is not clear from this description exactly what the "aspect index" is. Can you please explain more clearly?

Page 2719, line 16: Is there a reason why wind was not considered? Field workers recognize that wind can have a large effect on wet snow avalanche activity.

Page 2719, line 23: You extrapolated from one weather station to get the energy balance components for different areas. Did you test how well this worked by comparing your data from this station to extrapolations to your other weather stations?

Page 2720, lines 1-12: This paragraph feels like it is getting long and I find that it is not very clear. It might be useful to try to re-write this section and try to improve its clarity for the reader.

Page 2728, line 2: The authors state that "the predictive skills...were rather good". I guess it depends on your point of view, but I think that this is overstating the performance. What we see is that the classification tree for WFJ does fairly well, but the RF model does not do nearly as well. This may be because the RF technique is more robust, while the amount of data used in this work (N=132) is relatively small for classification trees. This latter point may also be the reason why the tree created using the data from DFB was different than the WFJ tree and did not perform as well. Or, perhaps the authors have a better explanation for this? I am guessing the classification trees may be fairly unstable. This point is made more clearly throughout section 4.3 where changing inputs into the trees caused large changes in the accuracy of the trees as well as the variables used to classify the events. The authors should better acknowledge the instability of their results and that a relatively small dataset was used for a classification tree analysis.

Minor points/typographical errors:

Page 2716, line 2: should be "non-existent" rather than "inexistent".

Page 2721, line 16: should be "are" rather than "is".

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Interactive comment on The Cryosphere Discuss., 6, 2715, 2012.