The Cryosphere Discuss., 6, C2083–C2089, 2012
www.the-cryosphere-discuss.net/6/C2083/2012/

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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Received and published: 2 November 2012

We are very grateful for the comments of Karl Birkeland. His valuable input helps to clarify a number of issues and deficiencies of the present manuscript. In the following we will reply first on the general comments and concerns and then address the more specific points. Karl Birkeland's comments are in italics, responses in normal font.

Questions and comments on the selected avalanche data

- *What type of avalanches (wet-loose / wet-slab avalanches) was included to determine avalanche days?*
- *How were avalanche days and non-avalanche days selected?*

We included both, wet-loose avalanches and wet-slab avalanches in our avalanche days. Whereas this choice is related to the less than perfect recording system, we think it is fully justified since only significant loose-snow avalanches, i.e. those including a mass comparable to slab avalanche are recorded at all; such large loose-snow avalanches are mainly observed under spring conditions. In addition we considered only days with a wet-snow avalanche activity (AAI) $>$ 2, i.e. that at least two size class 2 (Canadian size classes) or 20 size class 1 avalanches had to be recorded. Small slabs or wet-loose snow avalanches were filtered at this step. Based on the records, it is not always possible to clearly assign an avalanche type; it is though possible to record whether loose-snow avalanches and slab avalanches only, or both types were observed. The same is true for the position of the failure surface (interface snow-soil or within the snowpack). Therefore, we counted how often each avalanche type was mentioned. Considering all avalanche cycles, about as many times loose-snow avalanches as slab avalanches were reported (104 versus 77). For the location of the failure surface, 42% were within the snowpack and 58% of the avalanches released at the snow-soil interface. Also, if we pick particular days or prominent cycles the ratio hardly changes. Thus, both types frequently occur at the same time which is in line with our observations.

In addition, we used avalanches observed on a regional scale and not at the scale of a single avalanche path. So, in regard to triggering mechanism, we certainly agree that differences in snowpack structure and amount of water may exist and play a vital role in determining the type and fracture depth of the wet-snow avalanche. However, in any case, the avalanche occurrence is related to significant weakening of the snowpack related to meteorological conditions – which we try to address. Therefore, we think that differences in snowpack structure and amount of water will quickly even out

throughout the avalanche cycle and considering both avalanche types is justified.
As we are well aware of the complexity of the water-snowpack interaction, a problem intractable at present, we have deliberately chosen to ignore this aspect and focus on water production driven by meteorological conditions. Whereas this shortcut might be questionable we think it represents an approach that might help to advance our present understanding. At the same time we also work on a better representation of water flow in today's snow cover models such as SNOWPACK (see below) (e.g. Mitterer et al., 2011). In addition, we have certainly collected data on the snowpack conditions (see below, Fig. 1). These snow profiles, among other data, provided valuable insight but are not amenable to a statistical approach.
We will fully consider the concerns and include the above arguments partly in our Data and partly in the Discussion section.

We agree that Table 1 combined with Figure 1 and the number of avalanche days stated in the text is misleading. The main cause is an error in Figure 1b (thanks for pointing to that): the number should be 597 and represents all non-avalanche days for the four winters from December to May. Table 1 shows only the major wet-snow avalanche cycles. Single days with less prominent wet-snow avalanche activity are not included in this table. Single days in other months within the investigated periods represent mostly rain-on-snow events or single strong warming events. We will improve the wording of the caption of Table 1 in order to make this fact more clear.
It is of course true that Figure 1b shows a more distinct discrimination since a large set of non-avalanche days now is included. Nevertheless, it shows quite well that for predicting wet-snow avalanche days, we face the problem of having too many false-alarms when relying on the total amount of liquid water only. We prefer to keep Figure 1b, but will discuss the graph in more detail in the Discussion section by incorporating your suggestions on the state of the snow structure (almost dry versus ripe snowpack).

Questions and comments on the Discussion

- *Criticism on Discussion section*
- *Why snowpack structure (modelled or observed) was not considered for the analyses?*

We will re-write the Discussion section and in particular include the points mentioned above.

Concerning your question on why high incoming longwave radiation values are related to avalanche days. These conditions prevail of course during rain-on-snow events. The data set includes 9 days with rain-on-snow events. There are also other situations, which allow both an energy input due to shortwave radiation combined with longwave radiation. We sometimes observed large avalanche activity when cloudy nights followed warm and sunny days; also on days with a high thin layer of clouds both, longwave and shortwave radiation, will supply energy to the snowpack. In both cases cooling due to outgoing longwave radiation is hampered and values close to isothermal 0 C were observed at the snow surface.

We agree that the snow structure plays a vital role in determining infiltration patterns and velocity, and ultimately wet snow stability. For a proper local forecast, i.e. for a specific avalanche path, this kind of information is paramount and will enhance the quality of the forecast. The main problem is the highly non-linear and very heterogeneous character of water flow in snow. It is difficult to decide whether e.g. a snow profile is representative or not. Fig.
1 shows two snow profiles which were recorded recorded on slopes close to the weather station Dorfberg (DFB). The profiles were recorded at the same day with an hour difference. Profile (a) is located on a 35 south-facing slope, (b) on a 31

southeast-facing slope. The profile locations are 10 meters apart. Slope and elevation are almost identical, but snow depth varies quite a lot. The difference in snow depth and aspect causes two completely different profiles. Along with expected heterogeneity of flow paths this observation exemplifies that it is very difficult to obtain profiles on representative slopes. In addition, timing is crucial and data collection is most wanted when it is not safe to do so.

The challenges to be tackled on the numerical side are quite complex. In order to solve water flow within a stable wetting regime, i.e. uniform advance of the wetting front, we must solve Richards' equation which is a highly non-linear partial differential equation. The solution is mostly based on parameterised values for the saturated hydraulic conductivity and refers to laboratory measurements of so-called water retention curves. These curves describe the relation of capillary forces to the amount of water in snow and are dependent on grain size, grain size distribution and density. The values of the water retention curves are mostly obtained in laboratory tests and often only valid for very dense snow ($>$ 400 kg/m3) since the experiments are very difficult to conduct with low density snow (200-300 kg/m3). In addition, we have to solve correctly the thermal equations, the phase change (melting or refreezing), wet-snow metamorphism and densification processes. All these changes will again affect the solution of the water flow equation and the structure of the snowpack. Until now no preferential flow channels and no horizontally diverted water, which might be quite decisive on slopes, are included into these considerations. All these facts led us to the conclusion that at present it makes no sense to include snowpack structure in our statistical analysis.
However, we agree that we have to discuss the role of snow structure in a better way. We will be more concise on this topic and will include it in the revised Discussion section.

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Answers to specific points:

Page 2716, line 2: We will mention that Trautman et al. (2006) used that method to measure the loss in shear strength to predict shallow wet-loose snow avalanches.

Page 2718, line 22: Basically, the aspect index describes how often the aspects SE-S-SW were recorded versus NE-N-NW for a single day. We will improve the description.

Page 2719, line 16: Wind only has a minor direct effect on the energy balance since energy input due to pumping effects are low compared to the other sources we are dealing with. It does, however, play an important role in determining how much energy is supplied to the snowpack by the turbulent heat. This is included within our analysis by modelling the sensible and latent heat flux. What practitioners observe is probably always connected to high values of air temperature. Therefore, wind as single variable was not considered.

Page 2719, line 23: We did not do that so far, but will include this point in the revised manuscript. We compared the air temperature for both sites to test whether our lapse-rate is realistic. The air temperature lapse rate was 0.7-0.8 C/100 m; this is slightly higher than the value we assumed. As mentioned in the manuscript (Page 2730, Lines 16-21) this will lead to a slight underestimation of the sensible heat flux.

Page 2720, lines 1-12: We will shorten the section and try to be more concise.

Page 2728, line 2: The RF models have mostly lower scores as the method is more conservative and more robust for the low numbers of events. Classification trees tend to be over-fitted. However, by cross-validating and introducing cost-to-complexity limits for the tree, we try to stay as conservative as possible. The differences in the

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trees obtained from the stations were mainly due to the different variables that showed statistically different distributions for avalanche and non-avalanche days. Only if a variable was statistically significant it was passed to the multivariate approaches. As these were not the same for both stations, the tree could not pick these variables and therefore had to grow with other variables. The tree for the station itself was build 10 times and in 2/3 of all cases the same tree emerged, thus we believe that the trees are not unstable. The large changes discussed in section 4.3 were synthetically forced. Fewer variables were allowed to be used, in order to see, how a less detailed data set would change the predictive skills. By reducing the information to commonly available sensors we obtain results which we think are fairly good. Having information on air temperature and snow surface temperature allows hitting 90% of all events, and 60% of the non-events (Fig. 2). We will replace Table 5 with the figure shown below (Fig. 2) to improve the description on the predictive performance.

Page 2716, line 2: should be "non-existent" rather than "inexistent". Page 2721, line 16: should be "are" rather than "is". We will change as suggested.