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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

**C. Mitterer and J. Schweizer**

mitterer@slf.ch

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We want to thank Howard Conway for his valuable comments that definitely help to improve the understanding and quality of the presented work. In the following Howard Conway's comments are in italics, responses in normal font. We will first focus on the general comments and questions and then address the more specific ones.

**Questions and comments on using only meteorological and surface energy balance data**



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- *The idea of using either met data or surface energy balance alone as a predictor is odd since it is well known that snow stratigraphy exerts strong control on water infiltration and avalanche activity (e.g. Gerdel, 1954; Colbeck, 1979; Wankiewicz, 1979; Marsh and Woo, 1984, 1985; Marsh, 1988; Conway and Raymond, 1993; Conway and Benedict, 1994; Techel and Pielmeier, 2011)*

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 in the Discussion section.

**Questions and comments on the selected avalanche data**

- *What is the validity of the assumptions about the aspect index?*
- *In addition, how does the reporting system discriminate between wet/dry avalanches?*
- *Is the classification subjective? How accurate is the classification?*
- *Does the reporting system discriminate between wet-loose, and wet-slab avalanches?*



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The Swiss recording system for avalanche activity was designed mainly for the purpose of avalanche forecasting with the aim of obtaining as many observations as possible in a short span of time without imposing too much effort (i.e. time) on the observers. Accordingly, it is possible to report, for example, several avalanches at once. The disadvantage in that case is that only the number, the size class of the avalanches and whether the avalanches have been classified wet, moist or dry can be analysed. When an observer reports several avalanches at once, it is not possible to clearly assign aspect, slope, elevation, avalanche type, and fracture depth to each avalanche. Therefore we were forced to introduce the aspect index as the ratio of how often the aspects SE-S-SW versus NW-N-NE were reported. In all, but one of the 66 days the ratio was significant, i.e. , it was quite obvious whether avalanches on southern or northern slopes dominated. Nevertheless, this index should rather give a hint, which aspects prevailed during the specific avalanche events and we will mention this in the revised manuscript.
As mentioned, it is not always possible to clearly assign an avalanche type to a recorded avalanche size class; 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 location 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 loose-snow avalanches as slab avalanches were reported (104 vs. 77). For the location of the failure surface, 42% were within the snowpack and 58% of the avalanches released at the snow-soil interface. Thus, both types frequently occur at the same time. 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. In any case, the avalanche occurrence is related to significant weakening of the snowpack related to meteorological conditions – which we try to address. In summary, we conclude that it is not so important to discriminate between the avalanche types and that considering number and size is

sufficient – in other words that the avalanche database at hand is not perfect but absolutely suited for the purpose of our study.
The classification into wet/dry is based on observation, mainly on visual clues. No objective measurements on the amount of liquid water within the starting zones are available but most observers are well trained to discriminate between wet and dry snow avalanches. Nevertheless, there is some subjectivity.
Regarding your concerns on the actual number of avalanche days, we understand that Table 1 combined with Figure 7 and the number of avalanche days stated in the text is misleading. Table 1 represents only the major wet-snow avalanche cycles, i.e. the days with the highest activity in the season. Single days with less prominent avalanche occurrence are not presented in Table 1. We will make the caption of Table 1 more concise and revise Figure 7.

**Questions and comments on the meteorological data**

- *It would be good to investigate uncertainties related to lapse rate. I would have thought you could calculate it using data from the weather stations at different elevations.*
- *Concerning changes in transmission with elevation (p. 2720 line 1), it would be good to have more details of the method used, perhaps in an appendix. The citation (Marty, 2001) is not easily accessible.*

We compared our lapse-rate with the air temperature measured at the AMS on Dorfberg and Weissfluhjoch. The median lapse rate between the two stations was in average 0.7-0.8 C/100 m for the entire season; this is slightly higher than the value we assumed. We will add a graph and run the modelled energy balance for the station Dorfberg in order to better account for the uncertainties within our data set. The transmission with elevation is again a very simple lapse rate which is based on measurements made at different elevations. We can easily provide more information on that in the revised manuscript.

**Questions and comments on the statistical analyses**

- *Why do you need to have the same number of non-avalanche and avalanche days in your analysis?*
- *Which data were used for selection? At first I thought the selection might have come from the 663 nAvD, but I see that Fig. 1b compares AvD (N=66) and nAvD (N=1394). Does this imply that nAvD during summer months as well as during December-May were used? What am I missing?*
- *Since the "RandomForest" analysis does not add to your discussion, why is it included? I think that dismissing the method in a sentence (such as that on p.2726 line26) would be sufficient and would help focus the paper.*

By selecting the same number of non-avalanche days as avalanche days we obtained a balanced data set which is commonly considered as a prerequisite when dealing with discriminating problems. Figure 1b in the manuscript shows that one main problem in predicting wet-snow avalanche days is related to many false-alarms created by days late in the season. So we think that the criteria of the randomly selected same number helps to objectively cut off the probability of a false-alarm. Figure 1b contains an error (thanks for pointing that out): the number should be 597 and represents all non-avalanche days for the four winters from December to May. The RF method was included as it represents a more robust method for our limited data set. We will shorten the description of the method in section 3.2.

**Questions and comments on the Discussion and Conclusion**

- *The discussion needs considerable editing to sharpen its focus.*

- *It would be useful to have a more in-depth discussion of the degree-day approach, especially in context of the results of the analyses.*

We will rewrite the Discussion section and sharpen its focus along the points you proposed on page 1752, lines 11-18.
In regard to degree-day models, we believe that one of the main differences to glaciers or especially large ice capes is the different slope angle. Since we are interested in data of steep terrain, shortwave radiation plays a more important role than for flat terrain in high latitudes. The physical explanation given by Ohmura (2001) for the representativeness of air temperature for shortwave radiation is questionable for our purposes. In his work, he relates high temperature to solar radiation by enhanced warming around meteorological stations situated on bare rock outside the glaciated area. Absorbing solar radiation due to the low albedo of the surrounding causes the warming. This is definitely not the case on our slopes and since incoming shortwave radiation is the largest energy input it is under-represented by air temperature. In addition, degree-day models tend to be tuned. We will discuss this topic in the Discussion section.

**Questions and comments on the Abstract**

- *It is stated that formation mechanism for wet snow avalanches is poorly understood, but really it is the evolution of thermal and mechanical conditions in the snowpack that is difficult to measure.*

This is certainly true and we will change as suggested.

**Answers to specific questions/comments**

*In some places (especially maritime climates) rain-on-snow is a major source of heat and liquid water.*

We stated that our results are biased towards situations when the melting is driven by high irradiation and air temperature values (P. 2732, 13-18). Typically, these conditions prevail in transitional to continental snow climates. We will be more specific in this context and use your suggested comment.

*Mention is made of work by Hirashima et al., (2010) and Mitterer et al., (2011). It sounds as if you have no confidence in the infiltration model? Do you think the issues will be resolved in the near future?*
At the moment both implemented infiltration models are more or less complex tipping bucket approaches. At best, they represent the advance and behaviour of a uniform (stable) wetting. These approaches and similar ones proposed by e.g. Colbeck (1972) or Jordan (1993) do perform well as soon as the snowpack is ripe. At the moment Wever et al. (in prep.) are on the way to fully implement Richards' equation into SNOWPACK. However, still a large shortcoming has to be solved in the future: preferential flow. We think that this unstable flow regime may be dominant during periods with high wet-snow avalanche activity early in spring. In order to tackle this complex problem in the near future, we will have to combine concepts from statistical models, e.g. percolation theory or fibre-bundle models with the classical Richards' formulation. Thresholds supplied by the so-called capillary bond number could help to switch between the statistical approaches and the numerical solution of Richards' equation. So we are quite confident that we can take a step further to solve the problem in the not too far future.

*Can you comment on differences in snow stratigraphy on north vs. south facing slopes? Is the spring stratigraphy more complex on one aspect compared with the other?*
Based on our limited data set, we cannot directly compare snow stratigraphy from north-facing to south-facing slopes. However, we think that complexity in spring will depend on how often water from short melt processes will infiltrate to still cold and

dry parts of the snowpack. In addition, small scale differences in terrain will cause large differences in snow stratigraphy. Fig. 1 shows two snow profiles which were recorded only a few meters apart. The location of the first profile was on a slope of southern aspect, while the other one was on an adjacent slope facing a bit more to the south-east. 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. With ongoing melt, complexity will decrease and snow stratigraphy will become more uniform.

*"In none of the multivariate approaches variables related to the total amount of liquid water were chosen." What does this mean?*
Neither, the maximum, the minimum, the mean or any sum calculated with the variable total amount of liquid water within the snowpack was chosen by the multivariate approach. Though significantly different for event and non-event days this variable was not important for the RF models and the classification trees.

*Why do you think your results are different that Peitzsch et al., 2012? Is it because outgoing long wave radiation is more important?*
No, mainly because in Peitzsch et al. (2012) relative humidity was low on avalanche days; that is opposite to our observations.

*What do you mean by "less accidental"?*
That with a more homogeneous water flow, the effect of water on snowpack stability is more predictable. Since we do not know exactly where water penetrates or ponds during the unstable flow regime a prediction of the mechanical stability is very difficult.

*Note that Conway and Raymond recognized evolutionary states during rain-on-*

*snow, depending on the state of the snowpack.*
Thanks for pointing this out; we will consider this in the revised manuscript.

*What do you mean by "state of energy of the snowpack..."*
Basically, how "cold" the snowpack is. With other words, how much buffer is left until the entire snowpack is at 0 C and may thus start to melt.

*I see that mean TSSmax at DFB for nAvD = 0. Is that correct?*
Yes, this is correct. Many non-avalanche days were picked late in the melt season and therefore snow surface temperature was often close or at 0 C

*Table 4Shows measured and modeled data at 2500m; from Table 1 it looks like median elevation of South slopes was 2400 and 2275m for north slopes. I am guessing that non-linear relationships between meltwater production and energy input will change results. How do threshold values change with elevation?*
The threshold value for sensible heat will likely change with elevation. However, the most dominant variable for the energy input is incoming shortwave radiation which is much less effected by elevation, but has major effect on the thresholds for different aspects.



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