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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

H. Conway (Referee)

conway@ess.washington.edu

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The paper investigates possible links between meteorological variables and wet-snow avalanche days in the Davos region, Switzerland. The stated goal is to establish a predictive model of avalanche days based on meteorological observations and/or modeled surface energy balance alone. The leading hypothesis appears to be that met data alone are inadequate and a surface energy balance approach is necessary. 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

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Interactive Discussion

Discussion Paper

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).

That said, here the authors present observations and model results relating to wet snow avalanche activity over four consecutive avalanche seasons in the Davos region. The study leverages met and avalanche data from Switzerland. During that period observations indicate a total of 66 avalanche days and 663 non-avalanche days; apparently each avalanche season in the region is considered to be December-May (total of 729 days over the four year period).

Avalanche data:

1. The authors note that the Swiss recording system does not discriminate slope aspect when reporting avalanche activity, so instead they introduce an aspect index to discriminate between activity on southern and northern aspects. What is the validity of the assumptions about the aspect index?

2. In addition, how does the reporting system discriminate between wet/dry avalanches? Is the classification subjective? How accurate is the classification?

3. Does the system discriminate between wet-loose, and wet-slab avalanches? It would be important to distinguish between avalanches that initiate in the near surface snow and those that release as deep slabs.

4. I am confused by Table 1, which indicates just 23 wet-snow avalanche days over the four-year period (compared with 66 reported in the text). Further, Fig 7 shows avalanche days on March 8,14,18, 31, April 1-6, 11, 13 in 2009, and yet Table 1 indicates avalanche days on March 1-8, 18, and April 28 during that same year. What am I missing?

Met data:

1. A comprehensive suite of met data is available from three stations: two are just

above tree line (near 2150m) while the other (at 2540m) is above the start zones of most wet snow avalanches. These data are extrapolated to estimate variables and model surface energy balance at elevations of interest. This is a good approach although mention is made that the lapse rate used to extrapolate temperature may not be correct. This could be critical considering the non-linear responses expected when the snow temperature increases to 0 C. 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).

2. It is stated (p.2732 line 13) that rain-on-snow was a factor during just one of the events, but no further details are given. Given the differences in influxes of liquid water during melt and during rain-on-snow, it would be important to identify which event involved rain-on-snow.

Statistical analyses:

1. I understand that non-avalanche days were selected randomly from the data, but why do you need to have the same number of non-avalanche and avalanche days in your analysis?

2. 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?

3. 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 line 26) would be sufficient and would help focus the paper.

Discussion and conclusions

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1. The authors cite work by Ohmura (2001) on glaciers, questioning its relevance for snow, but in fact numerous authors have examined relationships between degree-day models and surface energy balance methods (e.g. Hock, 1999; 2003; Braithwaite, 1995; Braithwaite et al., 2008). Further, de Quervain (1979) published time variations in degree-day factor for snow in a 28-year series of snowmelt data from the Weissfluhjoch study site in Switzerland. 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.

2. The discussion needs considerable editing to sharpen its focus. For example there is discussion of flow fingers and infiltration, but no attempt to discuss how these might be parameterized. See for example Colbeck, 1979, for ideas on how this might be achieved.

3. Main results appear to be: (i) predictive results using specific met data (primarily air temperature in conjunction with surface temperature), and surface energy balance are similar. For practical purposes, temperature measurements are more readily available and do not require a model. (ii) results depend on elevation as well as slope angle and aspect; perhaps measurements at targeted locations would help resolve some uncertainty. (iii) Fig. 7 (showing evolution of liquid water content and cold content during March and April) is interesting. Results suggest that snow stratigraphy also plays a role in the timing of avalanche release.

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. The authors mention that using air temperature as a predictor has limited success, but results from their study indicate that it does very well, especially when used in conjunction with surface temperature. Degree-day factors to calculate melt can be tuned to slopes with different elevations and aspects (as well as albedo), similar to that used in surface energy balance methods.

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Interactive Comment

Additional specific questions/comments include:

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

2. P.2722 line 2722: 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?

3. P2712 line 10: 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?

4. P 2727 line 19: "In none of the multivariate approaches variables related to the total amount of liquid water were chosen." What does this mean?

5. P2729 line 23: "It seems, however, decisive how negative this sink was." What does this mean?

6. P2730 line 27: Why do you think your results are different that Peitzsch et al., 2012? Is it because outgoing long wave radiation is more important?

P2731 line 1: What do you mean by "less accidental"?

P2732 line 18: Note that Conway and Raymond recognized evolutionary states during rain-on-snow, depending on the state of the snowpack.

P2733 line 25ff: What do you mean by "state of energy of the snowpack. . ."

Table 3: I see that mean TSSmax at DFP for nAvD = 0. Is that correct?

Table 4: Shows 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?

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