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Interactive Comment

Interactive comment on "Model simulations of the modulating effect of the snow cover in a rain on snow event" *by* N. Wever et al.

N. Wever et al.

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We would like to thank the reviewer for his constructive comments. Below, we would like to give our response to the most important issues raised by the reviewer. The many suggestions to improve readability and clarity, will be very helpful for revising the manuscript.

General points

General point (1): We are aware that it is important to clearly distinguish between results from measurements and from modelling. We will consider this point when revising the manuscript.

General point (2): There may be some confusion regarding the term runoff here. Throughout the manuscript, we use the term "snowpack runoff" to denote melt water leaving the snowpack. This term is commonly used in snowpack modelling literature. We can understand the confusion with the commonly used word "runoff" in the hydrological sciences. Although we consistently used the term snowpack runoff in the paper, we will explicitly clarify the possibly confusing definition when revising. Furthermore, the lysimeter is at ground level and is only measuring snowpack runoff. We will revise the manuscript to make this also clear by referring to the lysimeter as "snow lysimeter".

We realize that the information about soil conditions at the measurements sites is very limited. However, we decided to drive the SNOWPACK model with soil layers, in order to prevent that choices of lower boundary conditions for the snowpack would have a major impact on the results. The simulated soil is chosen as a shallow one of only 10cm depth, and is driven from below with measured soil temperatures from approximately this depth. This strongly reduces the influence of uncertainties in soil parameters. The spin-up of 10cm soil depth is expected to be marginal, in particular since the temperature at the lower boundary is prescribed. We started the simulations about 1 month before the event, something which was not described in the manuscript, allowing for significant spin-up time. The choice for a typical parameterization of coarse material is to prevent ponding inside the model domain. Due to the generally sloped terrain in the Swiss Alps, melt water that could not infiltrate the soil after leaving the snowpack is expected to leave the snowpack down slope, instead of ponding inside the snowpack. The paper will be made clearer on these points when revising.

General point (3): In our opinion, the analysis of the simulations for the 14 stations alone would have limited meaningfulness for general conclusions, as there are many factors (station location, meteorological conditions, etc), influencing the results for the particular measurement sites. To be able to distinguish between the various effects and in order to do a meaningful statistical analysis, we had a need to increase the data quantity. We performed the ensemble simulations to find out what meteorological and snowpack conditions are influencing snowpack runoff. In our opinion, replacing time series at a measurement site with a time series from another site is preferable to creating artificial time series, as the time series are self-consistent with real meteorological conditions that occurred during the event. Of course, the simulations cannot be considered valid for the elevation the station is at, or the topographical situation around the station, but that was not our goal of the ensemble simulation.

General point (4): We will discuss the verification station in more detail when revising the manuscript. The point made is strongly related to the reviewer comment about preferential flow paths. We indeed do not consider preferential flow paths in this study. It is clear that preferential flow paths have been observed in natural snowpacks, yet the importance of them in terms of contribution to runoff is not clear. For example, in Wever et al. (2014), we did not find a consistent time lag in sub-daily time scales between modelled and measured runoff, suggesting that preferential flow paths play a minor role, at least once the snowpack is isothermal. However, there are strong observations of preferential flow paths in snow covers that are below freezing away from the paths. Those flow paths can efficiently transport water downward. One of the problems is that firstly, snow lysimeter measurements have inherent inaccuracies that makes the measurements not necessarily representative for a larger area. Moreover, there is currently no model concept for preferential flow paths available. We will amend the manuscript at this point. We still think that the use of Richards equation for modelling liquid water flow is justified, because the bucket approach would give a larger discrepancy between the model and the snow lysimeter regarding the onset of runoff and the runoff rates.

We would like to argue that the time delay between measured runoff and modelled runoff in Figure 1 is about 2 hours rather than 4, as stated by the reviewer. The optimal model setup has a delay of 1.5 hours. We then consider only runoff amounts that are interesting from a hydrological point of view (>1 mm w.e). The tiny amounts of runoff recorded by the snow lysimeter in the hours before, that are either associated with basal melt, or arrival of preferential flow at the base of the snowpack are indeed not reproduced in the model, but they also play an insignificant role in the hydrological processes.

General point (5): We will revise the figures and text regarding the figures, following the reviewer comments.

Specific points

Introduction: Where does the present study go beyond the available research on rain on snow events? To our knowledge, most studies on rain on snow events focus on time scales from days to seasons. To understand what happened in the event we describe in the paper, it is essential to look at sub-daily time scales for which little is known.

But of course snow accumulation will also be determined by wind erosion or deposition and the snow height change might thus be difficult to directly compare to precipitation. Does this effect have an impact on the analysis? The difficulties in comparing measured precipitation with a rain gauge and snow height measurements are precisely the reason why we choose to use the snow height measurements to assess the state of the snowpack at the onset of rain. Snow deposition, not measured by the rain gauge, will be recorded this way. Erosion would lead to a lower snow height, and accordingly, less mass is added to the modelled snowpack. The fact that we may end up with a different snow cover depth at the onset of rain than when we would have used precipitation measurements is actually convenient, as our interest is primarily in the snowpack at the onset of rain, not in the solid precipitation amounts leading to the built-up of the snow cover.

Aren't there any direct observational data to back up this estimation of cloud coverage? We will contact the Swiss Federal Office of Meteorology and Climatology for further information about cloud coverage.

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Does the unventilated sensor always measure temperatures that are 1.2 deg too low (at least this is what I understand)? It is not expected to be a persistent offset as the sensors are regularly inspected. The problem is especially that unventilated sensors may get covered with snow, and also condensation at the sensor may occur in moist conditions. However, this particular event was accompanied with such a strong increase in temperature, that it is only an important threshold for the very beginning of the event. We therefore do not expect this to be an important influence on the results.

- Why is Q_{sum} "prescribed"? I would rather say this flux is modelled.
- But in that case R_{net} (Eq. 1) should contain an additional term reflecting that not all shortwave radiation is actually part of the boundary condition. The term "prescribed" for Q_{sum} was used from a modelling perspective, as the prescribed flux in a Neumann boundary condition. We will change wording to prevent confusion. Also, we will reformulate this equation to make explicitly clear that the shortwave radiation is not part of the Neumann boundary condition, but rather it is used as a source term in the snowpack. R_{net} in the SNOWPACK model then only refers to the net longwave radiation. We originally formulated the energy balance in this way as to stay consistent with commonly used energy balance equations.

Why aren't there any preferential flow paths for the sites studied here? Please see our response at major point (4).

How is basal melt calculated? Basal melt is predicted by the model. The heat advection equation is solved for the combined snow and soil model domain, applying boundary conditions only at the top of the snowpack and the bottom of the soil. Basal melt is than just the result of heat advection from the soil or from snow layers above. Again, we argue that the shallow soil of only 10cm, combined with a prescribed soil temperature at the lower boundary, minimizes the effect of uncertainties in soil properties on the final results.

Is the extra runoff due to the destruction of the snow matrix? And not rather due to

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snow melting? We argue that the fact that the regression coefficient of runoff with snow melt is above 1.0 is due to the fact that snow melt not only generates extra liquid water available for runoff, but also destroys the matrix where suction forces keep the liquid water inside, generating extra runoff.

Why "accidental"? We used the term accidental, because we did not found a reason why deeper snow covers should experience stronger melt rates than shallow snow covers. We suspect that it is accidental that the deeper snow covers experienced more melt. We will reformulate to make this point clear.

Reference

Wever, N., Fierz, C., Mitterer, C., Hirashima, H., and Lehning, M.: Solving Richards Equation for snow improves snowpack meltwater runoff estimations in detailed multi-layer snowpack model, The Cryosphere, 8, 257–274, doi:10.5194/tc-8-257-2014, 2014.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 4971, 2014.