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Hydrology and Earth System Sciences
Discussions

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

Anonymous Referee #1

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This paper describes the investigation of a rain on snow event in October 2011 in two regions of the Swiss Alps. Using a complete physically-based model that resolves all relevant processes in the snow cover, the temporal evolution of snow melt, water storage and runoff are analyzed. The model allows recognizing the driving processes for runoff generation and water retention by the snow pack during rain on snow events and thus understanding the complex nature of such hydro-meteorological conditions. Although I found the paper interesting to read and the findings important for a better assessment of rain on snow events I have a number of substantive comments that should be addressed by the authors:

(1) Model treated regarded as "measurement": This is pure modelling study (including one single validation site from which the results are only marginally discussed).

Throughout the paper, however, the model results are often presented as if they were measured variables in the field. The authors should be more careful with their wording and make it clear that time series of runoff, melt and storage are modelled and not actually backed up with direct measurements. This can be achieved be simply reformulating some parts of the results.

(2) What about the conditions of the soil below the snowpack? The model set up for the snow cover is described in detail and all results are presented relative to the snow pack itself. However, for runoff generation and water retention, the substrate below (!) the snow cover and its physical state is highly important. How is the soil parameterized for the different sites? Surely not all sites have exactly the same soil characteristics (assumed to be "coarse material", see page 4979, line 20). How is the initial soil saturation prescribed, i.e. is a spin-up of the model performed to determine the correct state / saturation of the soil? In my opinion, this will have a major impact on the overall hydrological response of the investigated site.

(3) Ensemble simulation: A significant part of the paper is the ensemble simulation where the climatic forcing is interchanged between the different sites to obtain an ensemble runoff response on the rain on snow event. Until the end of the paper, it did not become entirely clear to me what the benefit of applying a measured meteorological time series to a different site is. Also, I am not sure if it makes sense – from a meteorological point of view – to "replace" time series, i.e. attribute a measured series to a different elevation and a different topographical situation. In my opinion, this would then result in an "unphysical/unreal" boundary condition from which drawing conclusions is difficult.

(4) Verification: I would like to see a more in-depth discussion of the disagreements of the model results with the measurements at Weissfluhjoch, and – more importantly – a discussion of the possible impacts of these on the final conclusions. In fact, Figure 1 indicates that the time lag from the onset of the precipitation event to the first snowpack runoff is overestimated significantly by the model (e.g. about 4 hours, or 70% by

the energy balance model with the Richards equations). As later the (model-based!) retention is interpreted and discussed this point deserves more attention. One or two paragraphs on this topic in the discussion section should be added.

(5) Figures and Writing: The figures need to be improved. When printed on A4, I was unable to read most (!) of the axis labels, and even on screen the figures need to be strongly enlarged to become clear. Please enlarge the axis labels and the legends. Furthermore, the writing could be more concise at many places. At present the manuscript contains many unnecessary sentences, such as descriptions of what can be "seen" in the Figures. There are also several repetitions, e.g. in the Abstract.

Detailed comments:

Abstract: should be significantly shortened. It is partly repetitive and does not concisely focus on the major points of the paper. Also reformat the abstract into one paragraph.

Introduction: I am missing a short review of recent studies that have addressed rain on snow events. The references provided in the introduction are rather general and might imply that this study is the first one to actually investigate a rain on snow event with a snow pack model. Where does the present study go beyond the available research on rain on snow events?

Goal of research: The introduction is also missing a paragraph that clearly outlines what is done in the present study. The last paragraph of the introduction is now mainly a description of the event. But what exactly will be investigated here? And how does the paper go beyond the collection of articles compiled in Badoux et al. (2013)? This would be a very important addition to make the paper acceptable.

Page 4975, line 22: The idea to use snow height changes as a proxy for snowfall intensity rather than data from an unheated precipitation gauge is a good one. 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?

Page 4976, line 16: Aren't there any direct observational data to back up this estimation of cloud coverage? I am sure the automatic climate stations provide some quantitative evidence on this.

Page 4977, line 19: This puzzles me. Does the unventilated sensor always measure temperatures that are 1.2 deg too low (at least this is what I understand)? Wouldn't this have quite an important effect on the energy balance calculations? Or is this bias just relevant around the melting point? If there was a bias in fact, wouldn't it be more sensible to correct it before feeding the time series into the model?

Page 4978, line 4: Why is Q_sum "prescribed"? I would rather say this flux is modelled.

Page 4978, line 11: I understand that the penetration of shortwave radiation into the snowpack is accounted for. 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. Or am I missing something here?

Page 4981, line 6: please reword "it can be seen" (here and at other instants)

Page 4984, line 5: Example for my major point (1) and the comment above: As it is formulated here, the time lag seems to be a fact / an observation. However, it is modelled, and according to Fig 1. the model does not match the data very well.

Page 4986, line 1: Why aren't there any preferential flow paths for the sites studied here? This (strong) assumption requires some argumentation. At present the reader rather understands that there are no preferential flow paths as the SNOWPACK model is not able to simulate them.

Page 4986, line 9: This entire section discusses "regression coefficients". Although the method section provides some hints on how to interpret them, the reader desperately needs some guiding here. What do these coefficients tell us? How can they be interpreted? I suggest that the section is rewritten to more clearly explain the outcomes of

this experiment.

Page 4986, line 13: This is the first time that “basal melt” is mentioned. How is basal melt calculated? Also here, I assume that a model spin-up for the time before the event is absolutely necessary to determine the heat content of the ground which might be released into basal melting of the snow.

Page 4986, line 19: Is the extra runoff due to the destruction of the snow matrix? And not rather due to snow melting?

Page 4987, line 13: What does “applied” mean in this context. Please clarify.

Page 4988, line 23: Why “accidental”?

Discussion: A subsection or a paragraph discussing the limitations of the model and the dependence of the main conclusions on these limitations is definitely needed.

Page 4990, line 9-16: This paragraph is weak and could almost be completely removed.

Conclusions: In my view, this section is lacking a condensed and easily understandable description of the general effect of the snow cover on runoff found in this study: (1) runoff is delayed by a few hours thus strongly reducing the rain-induced runoff peak (by how much?), and (2) total runoff over a longer time span is larger by 10-20% (?) than the rain-event due to snow melting.

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