

Interactive comment on “Retrospective analysis of a non-forecasted rain-on-snow flood in the Alps – a matter of model-limitations or unpredictable nature?” by O. Rössler et al.

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The authors present a very interesting retroactive look at a major rain on snow event in the Swiss Alps. They first reconstruct the synoptic climatic conditions leading to the event and also analyze the local climatic particularities in detail. Subsequently the model used for the flood forecasting predictions is used to try and model the observed runoff curves. The authors conclude that major modifications of the model are needed to successfully model the runoff. A further modification of the climate input variables, taking into account the local topography and near surface air flow conditions further improved the simulations. Finally the predictability of the event was investigated using

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the optimized model and a climate forecast data set.

The study presents some very valuable insights into a central European rain on snow event. While many of the results are somewhat confined to the very specific local conditions in the study area, the authors also arrive at several conclusions about processes and model set ups that are transferable to other areas. It probably would be beneficial to discuss these “universally” attributable and transferable aspects in a little more detail, maybe even devote a separate section in the discussion to these conclusions. This would further enhance the value of the study for researchers working in the same research topic area but different geographical regions.

This review will focus on the snowmelt/runoff aspects of the study and will not address the discussion of the atmospheric conditions outlined by reviewer 1. The observations, simulations, and conclusions concerning the snow melt and rain on snow flood simulations seem very reasonable and well supported by the observations and simulations.

In the following there are some comments about specific parts of the paper.

p. 12862 Line 14 replace the word “drastic”. As mentioned by reviewer 1, at several points in the paper there are descriptive words such as “drastic” or “enormous”, that should probably not be used in scientific papers.

p. 12862 Line 15: where was the temperature increase of 8K observed? Was this an average of observed temperature increase at all stations or is this just an approximate value? Also how could a temperature increase of 8° shift the zero degree line by 1700m of elevation? Was there a special atmospheric layering or was the temperature increase higher at higher elevations? Please expand on this point.

p. 12862 line 16 here you describe the conditions on the “northern and southern flank of the valley” while on the following pages you always refer to conditions on “north or south facing slopes of the valley”. While obvious that south facing slopes are on the northern flank of the valley I still find this a little confusing and would suggest to stick

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with the terminology of south and north facing slopes throughout the paper.

p. 12864 line 10 "... on 9 October (hereafter 9 October), ... on 10 October (hereafter 10 October)..." I'm not sure what you mean by this or why it is necessary to make this point.

p. 12870 Model parameters adjustments: The authors mention three adjusted terms. To better understand why these three factors and only these three were adjusted it might be helpful to expand the description of the WaSIM Model in the previous section. While the snowmelt routines are explained adequately, the model simulation techniques for the transfer of the resulting melt water through the snow cover and into the stream are not described. Since two out of the three adjusted factors are apparently dealing with this part of the model such a description would be helpful.

The authors remark that the melt factors were adjusted with respect to both discharge and snow depth. Could you expand on how this was done? And could you provide the actual values of the melt factors before and after adjustment so that the reader has an idea of how big the adjustment was?

The same suggestion about providing original and adjusted values is valid for the adjustment of the term "response time of direct flow and interflow".

The third adjusted term is referred to differently first as "fraction of direct flow from snowmelt" then as "fraction of snowmelt that is direct runoff". While this is obviously very close please be consistent. The term was increased from 10% to 90%. While I understand that this was necessary to make the model results fit the observations, I would like to see a short comment of whether the 90% surface runoff is a reasonable value. Even with saturated soils I find this number surprisingly high.

Finally the authors mention that "The modeled snow depth is calculated from the SWE amount, assuming a snow density of 0.1 g cm⁻³. I assume that the model puts out SWE amounts on the ground, that were then converted by the authors to snow depths

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to compare them to observations. Is this correct? Or is this density only used for quantifying the amount of snow precipitation from the measurements? (And could you provide a sentence explaining this?) If snow on the ground is meant, it is reasonable to assume a density of 0.1 for the fresh snow at the beginning of the event. However, this is certainly not the case for the fully saturated wet snow that remains in parts of the basin at the end of the event. The authors should repeat these calculations using a variable (increasing) snow density or explain why they think this is not necessary or possible.

p.12871 line 6 replace “center of action”

p.12871 line 9 the sentence “. . .Because of the snow fall measurements . . .” sounds very awkward and should be reworded

p. 12872 line 12 “After the frontal passage a breezy northwesterly of polar air brought prolonged snowfall on Saturday 08 October.” This sentence seems to be missing a word.

p. 12872 line 23 “rover” should be “river”

p. 12872 line 26 the reference Ralph et al. 2011 is not listed in the List of References

p. 12873 line 16 the site of the air sondes launch is written as Payerne in Figure 2. Which is correct?

p. 12874 line 13 the Figure discussed here is missing completely if I’m not mistaken.

p. 12875 line 11 the figure discussed here is Figure 4

p. 12876 line 7 Figure 4d includes data for snow depth at the Ried station which seems very unreasonable. Even though this is the lowest station, it seems to accumulate snow on 09 October and keeps this snow cover until the end of the period. This is probably a measurement error, or not? Also as a suggestion: If snow depth data from the model is available for these locations, it could be compared directly to these observations

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(as was done for the SWE in the Milibach basin in Figure 9) to show that the model captures the elevation dependent snow melt rates adequately.

p. 12876 line 12 and following: I was very confused by Figure 5 and its discussion. The discussed and graphed wind directions did not seem to match at all.

p. 12878 line 3 The snowpack acts as runoff enhancer not only by trapping and releasing rainwater but by contributing melt water to the liquid water available for runoff.

p. 12882 line 6 The authors mention that an analysis showed that the model reproduced the flood peak approximately with the standard meteorology but that this was for the wrong reasons. I would list these reasons here again shortly because I'm not sure I know what those reasons are. The point mentioned immediately prior to this statement (that the standard meteorology would result in a snowmelt contribution to flood water of 62%) can, in my opinion, not be used as a reason for this conclusion. While 62% snowmelt contribution is on the high side we in our research project and others have observed rain on snow floods that had snowmelt contributions of up to 65%.

p. 12882 line 16/17 The color of the COSMO and refined met temperature curve mentioned in the text do not seem to match those in the Figure 10 legend.

Figure 4 I would suggest using the term "snow depth" instead of "snow height" it is much more recognized internationally. Also the Figure legend "Snow height" seems to be upside down compared to the "relative humidity" legend above it.

Figure 6 Why is there a question mark in the Figure? If this indicates an uncertainty is this discussed in the paper anywhere?

Figure 7 the observed runoff curve of the Lonza at Ferden seems to suddenly stop, probably close to the peak. I assume the gauge data after this point is missing but this should be discussed/mentioned in the text. Also the end point of the curve is mentioned in the text as flood peak runoff. Are you sure that this is in fact the case and that runoff did not further increase after the gauge stopped working? I would

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recommend discussing this in the text.

Figure 10 Figure legend upper right hand corner should read “...modelled rainfall” (lightblue)... There is an “a” missing in “rainfall”

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