# Responses to Referee 2:

### Please note, each comment of the referees is answered using italic letter and indented paragraphs.

### General comment:

*The authors like to thank the reviewer for his very helpful and constructive comments on the manuscript! We found all remarks very reasonable and helpful to improve the manuscript. We corrected the critics point by point, and above all worked on the model parameter description and how they were changes (new Table 2), added a comparison with further snow stations (new Figure 10), and extended the discussion by a paragraph about the transferability of the results. The remark referring to changing snow density during the event was most difficult to deal with. We agree with the reviewer that snow density is likely to change, but we found no indication of how exact. Therefore, we added an uncertainty band of +-25 %.*

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.

*The reviewer is correct that the transferability of the case study and transferable results could have been discussed in more detail. We added a paragraph in the discussion section (last paragraph) to comment on the transferability and what can be learn in more general view from this event.*

### Specific comments:

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.

*As suggested by the reviewer, we rephrased the mentioned words and went through the manuscript to correct for similar wordings.*

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.

*The sentence in the Abstract was misleading. We rewrote the sentence to clarify the temperature increase: Now the sentence is as follows: "It [The atmospheric river] was accompanied by a temperature increase that shifted the zero degree line from 1500 m a.s.l. to 3200 m a.s.l. during 24 h with a maximum increase of 9 K in 9 hours.". This seems reasonable and was measured at several meteorological stations in the valley (Figure 4). We furthermore added a sentence in the results section to describe the shift of the zero-degree-line.*

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

*We agree with the reviewer and changed the sentence according to the reviewers suggestion.*

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.

*As in the original Word-document it is written: "on 9 October (hereafter "9/10")", we think that this is a formatting issue. To avoid any further problems we changed the format to "9. Oct" throughout the text for every date.*

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 reviewer is correct. We partly rewrote the subsection "Hydrological Modelling" to describe the relevant processes in more detail. Most important changes:*
> *A)" In both model versions the water from snowmelt and rainfall percolates without delay through the snow cover and infiltrates into the soil. To account for lateral processes in the snow cover, a fraction of this water is directly attributed to surface runoff (parameter SF). This fraction needs to be calibrated"*
>
> *b) "Surface runoff, interflow, and base flow are superposed for runoff generation, and runoff concentration is described by conceptual recession parameters that refer to the response time of a catchment after rainfall. These recession constants are used for direct runoff (kd) and interflow (ki) and need to be derived from the hydrograph or need to be calibrated (Hölzel et al. 2011).. "*

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".

> *We extended the section on model parameter adjustment to clarify our adjustment. In addition we added a table (new Table 2) that summarizes the parameter values before and after the adjustment.*

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.

> *Thank you for this. We changed the description to be consistent throughout the manuscript into "fraction of direct flow from snowmelt"*

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.

> *We added: "While this adjustment was necessary to fit the model to the observed runoff, this value is quite high and therefore unlikely, but still possible."*

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 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.

> *In the original manuscript the description was misleading, thank you for bringing this up. We rewrote the section to make clear that we compared modeled snow water equivalent (and only the solid part of the snow cover) with observed snow water equivalent that was derived from snow depth. As correctly mentioned by the reviewer this is sensitive to our validation of the snow modelling. Therefore, we explained the choice of the snow density in much more detail and tried to incorporate uncertainties of snow density.*
>
> *Although the reviewer made a good point, we refused to assume changing snow densities during this short event, as no data are available how snow density might change. The suggestion to calculate several SWE curves using a variable (increasing) snow density is interesting, but this would make the calibration/validation of the model output a bit arbitrary. Hence, we just included uncertainty bands of +/- 25 % (0.075 – 0.125 g/ $cm^3$).*

**Language:**

p.12871 line 6 replace “center of action”

*Replaced by: “within the most affected catchment Milibach.*

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

*The sentence was reworded and split into two sentences, and the entire paragraph was partly rephrased to account for the inclusion of new Figure 10*
*“Snow fall measurements from the SLF station Gandegg was found to be more accurate compared to snow measurements from private stations. Hence, we fitted the precipitation against snow depths (assuming a density of 0.1 g/cm$^3$) measured at the SLF IMIS station Gandegg.”*

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.

*We added a comma after passage to clarify the structure of the sentence, and added a “flow” after “northwesterly”.*

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

*Thanks. We fixed that typo!*

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

*We added the reference.*

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

*Payerne is the French name, Payern the German name. We changed the name in the entire manuscript into Payerne.*

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

*The information discussed here refers to the PV levels of the atmosphere that is expressed in the 2 PV unit line (Figure 2). We agree that this is a quite compressed but anyway informative way of showing the data. We considered also adding another three panels illustrating PV maps, but refrained from doing so, as the readability of the other panels would decrease.*

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

*Thanks a lot! We fixed this mistake.*

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

*The reviewer is correct. The snow depth at the station Ried is quite uncertain due to measurement uncertainties, but those measurement uncertainties are type normal when using ultra-sonic measuring devices. In the revised version of the manuscript this is addressed in the description of figure 4.*

*We added a new figure 10 that compares the modelled SWE with SWE derived from the snow measurements of the private stations at different elevations and different expositions in the valley. Furthermore, a discussion of the new figure was added in the Results section indication, that the hydrological model using SM* and refined meteorology in general is able to reproduce the snow dynamic. We consider this comparison as the validation of the adjustment to model and meteorological data.*

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.

*This was another mistake. In a previous version we showed wind directions during two days and while shortening the manuscript we deleted the wrong one. Now we show the right and described day. Sorry for this mistake.*

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.

*We rephrased the sentence according to the reviewers suggestion.*

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%.

*Thank you for bringing this up. We clarified the conclusion, by extending this paragraph:*
*"To conclude, using standard meteorology, the peak optimized hydrological model is able to approximately reproduce the flood peak at the catchment scale. But a detailed analysis at the subcatchment scale showed that these reproductions were due to the wrong reasons: Using uniformally distributed precipitation amounts in the catchment and a runoff promoting snow cover (SF = 0.9) resulted in a correct representation of the flood peak of the Lonza, but failed to reproduce the uneven distributed flooding in the tributary rivers and strongly underestimated the flood peak at the Milibach. The optimal hydrological model reproduced flood peaks at the catchment and subcatchment scale reasonable well only after the refinement of the meteorology and a more extensive adjustment of model parameters.*
*"*

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.

*Thanks! The labels on the y-axis in Figure 10 (now Figure 11) were interchanged.*

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.

*The reviewer is right. But to clarify what is shown, we used Snow water equivalent [mm].*

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

*The question mark indicates that a second cavity circulation might be present in the adjacent valley, but we have no evidence. We added this information to the figure caption and in the text.*

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 recommend discussing this in the text.

*The reviewer is correct. The observed runoff data suddenly stops due to technical problems with the gauge. We assumed that the last measured value was approximately the flood peak. This assumption is justified as all different model versions showed the flood peak to temporally agree with the last recorded value. Still, this is uncertain. We draw attention to this uncertainty in the description of Figure 7. In addition, we added a paragraph in the discussion section about this problem.*

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

*We fixed that typo.*