Reponse to Reviewer #1

In addition to the rapid response provided in the interactive discussion, we amend our first response here, pointing out how we changed the manuscript following the reviewer's comments. A summary of the reviewer's comments are shown in italics, our first response in normal text and our last response in bold.

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.
When revising the manuscript, we took care in the wording to prevent confusion about measurement or model results.

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 introduced a definition of "snowpack runoff", see P2, L26 and we now consequently write "snow lysimeter", see for example P8, L24.

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.
Please find the revised section on P9, L4-10.

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.
Please find the revised section on P10, L6-26, in particular P10, L19-21.

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.

Please find a more in-depth discussion of the verification station on P12, L5-20 and a discussion in general about the validity of our model study in a new Discussion section, see P20-21. There, we also discuss that preferential flow paths seem to form especially in relatively large grained snow, and when strong layering is present. That makes it unclear to what extend they played a role in the event in the study, as new snow generally has small grains and low stratification. As an additional verification check, we discuss the RMSE of measured and modelled snow height for the stations during the melt phase, which indicates how accurate the model is able to simulate the melt phase. This can be found on P14, L10-25.

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

Specific points

Abstract: should be significantly shortened. It is partly repetitive and does not concisely focus on the major points of the paper.
Please see the largely rewritten abstract.

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.
Please find a revised Introduction on P3-4, in particular P3, L22-P4, L6.

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.
We did not change the manuscript regarding to this point, as readers can refer to the interactive discussion.

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.
We checked the MeteoSwiss station Jungfraujoch. Half cloudy is a good approximation of average cloud cover in the weeks before the event. During the event, Jungfraujoch reported full cloudy conditions, except for October 9. A closer inspection revealed that in between the snowfall and the rain period, skies cleared up for several hours in some parts of Switzerland. To reflect on this, we repeated the simulations, setting cloud cover back from 1.0 to 0.5 for the period October 9, 11:00-October 9, 17:00, but results are marginally affected by this change. This can be explained by the fact that cloudiness is most relevant at night, when there is no incoming short wave radiation, whereas the effect during the daytime is much smaller due to incoming short wave radiation.

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.
Please find our revised text on P9,L14-19.

- 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.
Please find the revised section on P5, L16-P6, L6.

Why aren't there any preferential flow paths for the sites studied here? Please see our response at major point (4).
Please find our newly added Discussion section, discussing preferential flow in particular, on P20, L10-26.

This entire section discusses "regression coefficients". I suggest that the section is rewritten to more clearly explain the outcomes of this experiment.
We rewrote large parts of this section, P17-18.

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.
We revised the text by using "melt at the snowpack base" instead of "basal melt", see for example P13, L14.

Is the extra runoff due to the destruction of the snow matrix? And not rather due to 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.
Please find the revised text on P17, L20-23.

What does "applied" mean in this context? Applied denotes the application of the linear regression equation.
Please find the revised wording on P18, L20-22.

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.
Please find the revision on P20, L1-4.

This paragraph is weak and could almost be completely removed.
We removed the paragraph.

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.
We revised the conclusions.
It is difficult to provide a general effect, as the effect of a rain on snow event on snowpack runoff turned out to be dependent on precipitation and melt rates as well as snow depth. See P 21, L15-21 for some general conclusions.

Reponse to Reviewer #2

We thank the reviewer for his comments. We provide a detailed response below in italics.

- Also throughout the paper present and past tenses are used – typically past tense is used throughout a paper.
We revised the text, regarding this point. We kept present tense in case results are discussed that are still valid, and past tense when referring to the event or methods.

Specific Comments

ABSTRACT
- The abstract is too lengthy and goes into too much detail. The structure of the abstract should be brief using 2-3 sentences to describe problem, 2-3 sentences to describe what was done, 2 sentences the most significant results and 1 sentence to summarize the impact of the results. Please eliminate the multiple paragraph structure.
We rewrote the abstract.

- 4973-1: use of the word “probably” infers that you have not answered this part of your question
We rewrote the abstract.

INTRODUCTION
- 4973-24: Why is the stored rain water “important”? consider removing the “important”
It was meant to express that this effect is not negligible. Due to the confusion it raised, we deleted “important amounts of”. See P4, L8

- 4973-25: I believe you would like the word “extent” not “extend”
Corrected, see P4, L9.

- 4973-27: remove “is getting” and change to “becomes”; remove “wet” – isothermal describes the “wet” state of the snowpack
Thanks for the suggestions. We decided to keep the word “wet” instead of “isothermal”. See P4, L10.

- 4974-11: remove “was bringing” replace with
“brought”
Changed, see P3, L7.

- 4974-12: use of the word “roughly” – please be precise
Replaced by 800m, as reported in Badoux et al (2011), see P3, L8.

- 4974-13: What is meant by “decayed” – choose another word
Replaced “decayed” with “faded”, see P3, L9.

METHODS AND DATA

- The complexity of describing a modeling exercise makes it difficult to provide the reader with the information they need to understand your approach. The "Methods" section needs to be structured to describe each component of the work.

I would suggest you follow this structure:

2.1 Brief overview of what the methods

2.2 The model: describe the model and the required inputs

2.3 Station data and development of input data for the model

2.4 Describe the event you are going to model

2.5 Modeling result evaluation

The methodology in its current structure is too difficult to follow and confuses the reader.

We revised the Methods and Data section, partly following the suggested structure. We now start with an introduction to the methods section. Then firstly, we describe the model, followed by available data and then the model setup. Finally, the methods for performing ensemble simulations is discussed.

Specific line comments: 2.1 The description of the sites and locations is extremely important. Please include a map of the regions and location of the stations you are using. Also include a table for the stations and the sensors that are available for each location.

We provide a map now as Figure 1. We decided not to add an extra table to the manuscript, as all stations have the same sensor setup, except for WFJ.

4980-17:20 – This sentence is confusing and needs more explanation in your approach of using other station data and why you are doing so.

This point is strongly related to Major point (3) of Reviewer 1. We rewrote the paragraph providing additional explanation. The specific sentence mentioned here is also revised. Please see P10, L8-21

RESULTS AND VERIFICATION The results section contains descriptions of methodology.

- Move the event description to methodology.

As the event description is heavily using the model simulation results to explain the sequence of events, we decided to keep this section in the Results section. Please note that a summarized overview of the event has been given in the introduction.

Specific line comments:

- 4983-3: Figure 2 and 3 are repetitive

We removed Figure 3 and will make sure that Figure 2 is printed as large as possible in final type-setting.

- 4983-10: Use of the word "tiny" – "tiny" should not be used as comparative adjective, be specific

Changed the wording to: "For this event, this is less than 10% of the total rainfall amounts.", see P13, L26.

- 4683-18:19: Please refer to the figure – "curve getting steeper (Figure XX.)"

Changed, please see P14, L6.

- 4685-1:3: Identify why the boundary conditions result in errors

Prescribing the upper and lower boundary temperature results in small errors in the diagnosed energy balance, because the temperature change between time steps of the upper and lower boundary may not necessarily be accounted for by the diagnosed energy fluxes from the preceding time step. We decided *to provide explanation in the section describing the model setup. Please see P5, L18-23.*

- 4989-19: change “was having” to “had” (or similar)
We removed the sentence from the manuscript.

CONCLUSIONS
The Conclusions section is well organized and points to the proper data to support the remarks.
- Consider integrating the information in the final paragraph in the lead paragraph of the conclusions section.
We followed the recommendations from Reviewer 1 here and removed this paragraph.

Specific line comments:
- 4989-20:23 – this sentence is too long and “It was crucial” is awkward
We reformulated this sentence, please see P22, L6-10.

- 4989-23:25: This sentence appears as a conclusion, but it seem like it should be stated clearer as a hypothesis
We reformulated this sentence, please see P22, L8-10.

- 4990-6: The “Note” is not needed, it is inferred as you are drawing on only modeling for conclusions. You can address this in the previous sentence
We reformulated this section, please see P22, L18-20.

References

Badoux, A., Hofer, M., and Jonas, T., eds.: Hydrometeorologische Analyse des Hochwasserereignisses vom 10. Oktober 2011, Birmensdorf, Swiss Federal Institute for Forest, Snow and Landscape Research WSL; Davos, WSL-Institute for Snow and Avalanche Research SLF; Zürich, Federal Office of Meteorology and Climatology MeteoSwiss; Bern, geo7 geowissenschaftliches Büro; Bern, Federal Office for the Environment FOEN, in German, 2013.

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.