Reply to general comments of J. Garvelmann:

The authors present a very interesting study about 4 sprinkling experiments with deuterium enriched water on natural snow covers with different initial conditions. The dynamics of snowpack outflow and the proportions of rainwater and melt water from the snowpack were analysed using a hydrograph separation approach based on the deuterium signatures of the sprinkled rainwater, the snow cover and the runoff from the snowpack. The results of the study provide some very interesting insights into the dynamics of water flow within the snowpack during the artificial sprinkling experiments and are therefore highly relevant for the process knowledge of runoff generation during ROS and consequently the improvement of hydrological models. The focus of the presented study is in the scope of HESS. I like the study very much. However, I recommend some revisions of the manuscript prior to a publication in HESS.

One of my main concerns about the submitted manuscript is the clear separation of experiment 1 from the other 3 experiments and the conclusions based on this one experiment having a cooler snow pack compared to the other experiments. From my point of view a snow pack described as "Snow temperature were mostly below the freezing point: : :" (page 6, lines 32 and 33) and the information from Table 3: Snow temperature -1.0°C with a standard deviation of 0.6°C cannot be called a cold snow pack. The use of the term "cool" would be probably better. The results of experiment 1 are of course distinctly different from the other experiments. However, it is just one experiment and the other three experiments show also individual behavior. A clear separation and the conclusions are therefore critical. The authors should think about focusing on the individual behavior of each experiment. This would include a more detailed discussion on the shape of the observed runoff hydrographs in Figure 4 is lacking and would improve the study considerably. Why are the peaks of experiment 1 decreasing from sprinkling period to sprinkling period, while the peaks in the other experiments tend to increase? Another point in that discussion may be the difference in the peak flows of total runoff and the rainwater fraction in total runoff. Furthermore, I highly motivate the authors to add a correlation analysis to further investigate the influences of snow pack properties (e.g. snow depth) on the observed hydrograph dynamics (e.g. lag times). This analysis would considerably improve the study and will provide further insight into the influences on different snow covers on the internal runoff generation.

The differences in total amounts of rainfall and runoff from the snowpack (page 9, lines 6 and 7 for example and Figure 5) are the reason why ROS events have the potential to generate more runoff than rainfall or snowmelt alone. Although the study in its current form is focused on the snow internal flow processes, please add a few more comments and think about extending the discussion on that aspect of the study.

There is missing a few words on the scale issue (the experiment was performed on a square meter of snow. What can be expected on a larger scale, what literature is available on the runoff generation during ROS on larger scales?) as well a few words on the effects at the edges of the sprinkled snow block. Please provide also some discussion on the snowmelt energy balance during ROS and the influences this energy (that was certainly not available during the sprinkling experiments may have on the runoff generation within the snowpack. Furthermore, there is missing at least one figure in the results section showing the deuterium signatures during the sprinkling experiments.

Finally, I recommend removing or extending the analysis discussed in section 4.4. In its current form this part is too isolated from the rest of the study. However, the results of using a traditional hydrograph separation approach with snow or snowmelt isotope signature compared to the results with the presented approach would be highly interesting. The signature of the runoff observed prior to the actual sprinkling experiments (that is clearly visible in Figure 4 for all experiments) should be used, since Taylor et al. (2001 and 2002) recommend using the melt water stable isotope signature of the snowpack for an accurate isotope based hydrograph separation.

We would like thank Dr. Garvelmann for his detailed review. We appreciate his comments and suggestions. Please find our reply to all issues below.

The experiments were divided according to different snow properties, at first place the snow density and further the thermal state. Ex 1 was conducted during "mid-winter" condition, whereas Ex2-4 were conducted during melting period, when the snow density was already high. These differences are also reflected in the results. We agree that referring to the first experiment as "cold" experiment may not be ideal. We suggest using the term "non-ripe" instead, which describes the overall snow state better.

Thanks further as to the excellent suggestions for a more detailed discussion of the result, which will in particular deliver better insight in the variability between the four experiments.

We did a correlation analysis of initial snowpack properties (snow height, density, LWC) and the measures of runoff response (lag time, velocity) as suggested (Fig. 1). However, we are convinced that the number of experiments is not sufficient to inform such an analysis thoroughly; In particular given that one of the experiments is distinctively different from the others, the analysis will result in high but ill-founded correlations. Even if we appreciate the general idea of such an analysis, we suggest – for the above reasoning - not to present data as those exemplarily shown below.

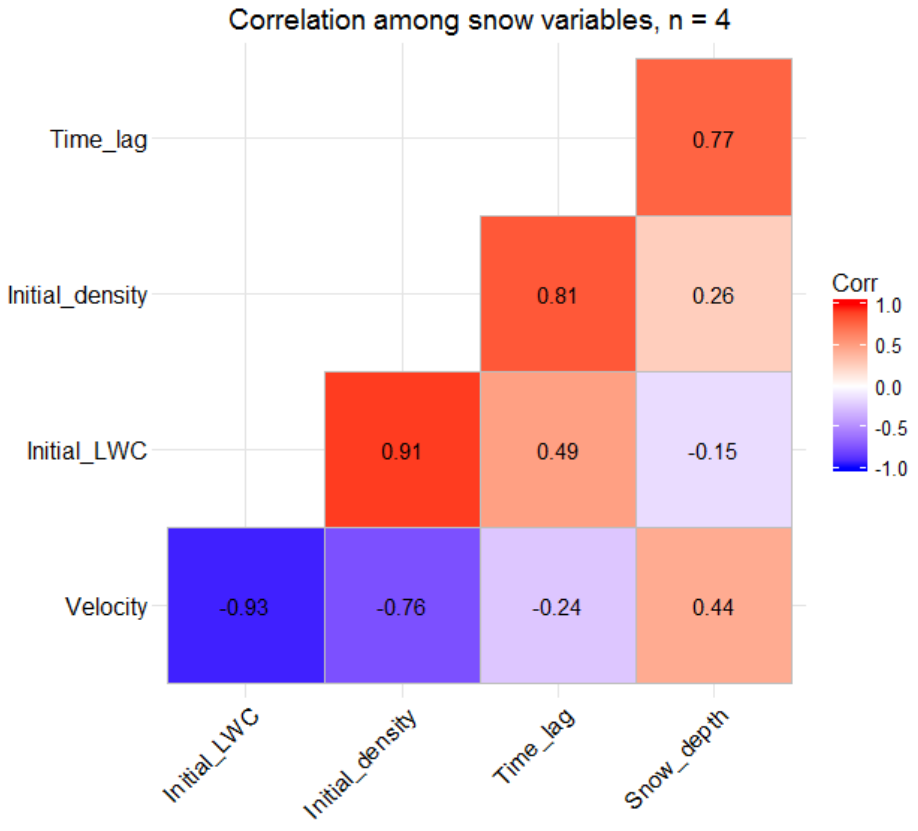

Fig. 1 - Correlation analysis between snow properties (Initial LWC, Initial density, Snow depth) and runoff data (Flow velocity, Time lag)

We will further expand the discussion on the possible implications of the study results on the catchment scale. We argue that some of the described mechanisms in the point scale have implications on the catchment scale, however processes such as overland flow or lateral flow in snow further add to the complexity of runoff generation if concerned with the catchment scale. The presented hydrograph separation technique is transferable to larger scale, if the natural rain has constant isotopic signature (McDonnell et al., 1990). The results will be further discussed and compared with earlier studies (Dinçer et al., 1970; MacLean et al., 1995) *which have addressed runoff composition within snow covered catchments.*

Unfortunately the energy balance could not be meaningfully calculated because of missing short and longwave irradiation data inside the rainfall simulator. But we have prepared the plot of the deuterium signals as recommended which can be seen below in Fig. 2.

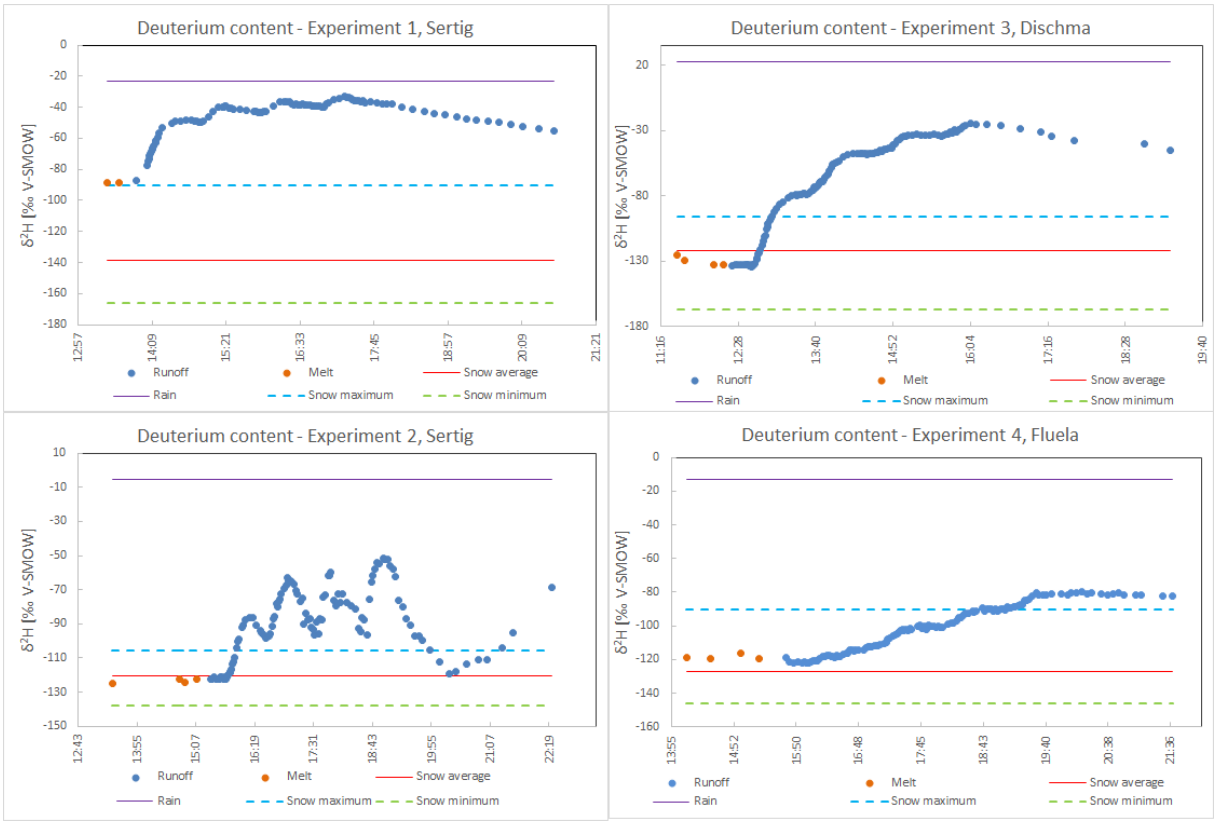

Fig. 2 – Suggestion of deuterium signature plot from all experiments.

We agree that chapter 4.4 should be extended since the new approach was introduced. The main message of this chapter is that using the pre-experimental meltwater deuterium content as a reference value for Eq. 1 only entail negligible differences in time lags. But noticeable difference may occur in the amount of rainwater in the total runoff. We will accentuate these findings in the chapter and also refer to Taylor et al.(2001, 2002) *in the context of our results summarized in Tab. 7.*

Reply to specific comments of J. Garvelmann:

I recommend the revision of the title of the presented study. Currently it is misleading, since the results of a number of artificial sprinkling experiments are shown and not the findings during a real ROS event.

We agree that the title could refer to the sprinkling experiments more specifically. We suggest changing the title to: "Rainwater propagation through snowpack during rain-on-snow sprinkling experiments under different snow conditions"

In the introduction section there is missing more information about the previous modelling work (page 2, lines 9-12) as well as more details about the different flow concepts (page 2, lines 28-31).

We will revise the introduction accordingly.

There is missing some important literature (page 3, lines 1-5). Taylor et al. (2001 and 2002) point out that for hydrological applications (in their case isotope based hydrograph separation too) a correct representation of the snow pack is absolutely crucial. They recommend using the melt water stable isotope signature of the snowpack for that purpose.

We will add this information and refer to the corresponding studies.

From my point of view the use of "deuterium content" (page 4, line 9 for example) or "deuterium concentration" (page 4, line 30 for example) are not appropriate. Please use "deuterium signature" or "deuterium value" instead and correct throughout the whole manuscript.

Thank you for this notice. We will consider using the term "deuterium signature" as suggested.

Please provide a few more words about the melt runoff and its isotopic signature that was recorded already before the actual experiment started (page 4, line 9).

We will add more information about the isotopic signature of pre-experimental meltwater.

There are missing the information about the meteorological conditions prior and during the sprinkling experiments carried out.

A short comment about the meteorological situation during the experiment will be included in the description of individual experiments.

Is c-solid (page 5, line 32) the average deuterium signature of the pre-experimental snowpack? Please specify. More information about the isotope signature (page 6, line 25) of the sampled snow profile would be very helpful.

Indeed, c-solid represents the average deuterium signature of the pre-experimental snowpack. More information will be added.

Why was the deuterium value of the sprinkling water +22.61 per-mille VSMOW during experiment 3?

It was important to maintain a minimum difference of 60 per-mile between sprinkling water and the solid snow. This difference was considered appropriate for a suitable rainwater separation. Setting of

maximal difference was not necessary, therefore it was not necessary to maintain the absolutely identical isotopic value of the sprinkling water for all four experiments.

The paragraph on page 8 on lines 8 to 14 is very confuse and hardly understandable. Please revise for more clarity.

We are sorry if this section caused any confusion. The paragraph will be revised for better reading.

Do you refer to a certain experiment or to all experiments on Page 8, line 18?

Here, we refer to all experiments. The sentence will be revised for better clarity.

Please mention clearly that the preferential flow may be due to the rapid development of fast flow paths in the snowpack when rainwater is infiltrating for more clarity (page 9, line 10). Please provide a more comprehensive discussion on the hydrological response of the snow pack (section 4.2). Please provide more details about the Colbeck (1975) study.
Here some examples that may be relevant, among others of course, in order to improve the discussion on this aspect: Average liquid water holding capacity of 7% of an isothermal snowpack (Singh et al., 1997). Liquid water retention storage between 2% and 52% depending on snowpack conditions (Anderson, 1973). Kattelmann (1997): water outflow from 1 to 2 m snowpack between 4 and 6 hours after onset of rainfall.

Thank you for these suggestions which will be considered to improve the discussion.

The description of the methods are confuse at some points. Please provide the information on the methods used in the study in a very clear way. There are mixed some results and discussion (page 7, lines 15-17 for example). I recommend a careful proofreading of the final version of the revised manuscript prior to re-submission.

We will follow the above suggestions and the manuscript will be revised carefully, including an English language check.

Technical notes:
Page 3, Line 23: average winter air temperature and mean annual winter precipitation for example.
The basic nomenclature will be unified.

Page 4, Line 30: Was the deuterium signature of snow melt water or sampled solid snow later melted in the lab prior to analysis?
All frozen samples in the plastic bottles were melted in the lab prior to the analysis.

Page 5, Line 11: Date analysis would be the more adequate title of this section.
Thank you for the suggestion..

Page 6, Line 9: Please revise equation 5 (Q-rain-in).
Thank you for the notice. The subscript $Q_{rain\text{-}in}$ will be revised.

Page 5, Line 19: “was” instead of “were”.
We could not find any “were” in P5L19, but on P6L19. We use the plural form of the word “data” and the related plural verb form “were”.

Page 8, Line 20: rain water
We would prefer to keep “rainwater” as it is through the entire manuscript.

Page 8, Line 30: deficit instead of deficiency
This will be corrected. Thank you.

Page 8, Line 32: ": : :rainwater contribution, however, increased …"
A comma will be added to the sentence in the revised manuscript.

Page 9, Line 8: The title of section 4.2 is confused. Please revise.
The section title will be revised.

Page 9, Line 22: Please provide some literature at this point.
Some relevant references will be added. E.g. (Fierz et al., 2009).

Page 9, Line 29: The point at the end of the sentence is missing.
In our version of the manuscript, the punctuation is used correctly. This might be a technical problem with the pdf viewer?

Page 9, Lines 31 and 32: This sentence is too vague. Be careful with statements on the energy exchange processes within the snow pack based on the results of the study. Please revise this sentence.
The sentence will be revised. Thank you.

Page 9, Line 32: Space too large.
It will be corrected.

Page 10, Line 3: "… refrozen or stored as liquid water in the snow pack." Please revise.
The sentence will be revised.

Page 10, Line 9: This sentence is too vague. Please revise.
The sentence will be revised.

Page 10, Line 11: Please provide information about were (section) the discussion on piston flow can be found in the manuscript.
This information can be found in chapter 4.1.

Page 15, Table 1: Please revise the dates in table 1 (missing point, space).
The dates in table 1 will be revised.

Page 15, Table 2: Is this table really needed? Please check if the content can be included to the text or added to another table.
The use of the table 2 will be once more considered.

Page 16, Table 3: Please provide SWE of the snowpack in the table. Please provide the information of the structure analysis (grain size etc.) as mentioned in the methods section. Please revise unit of bulk density (kg*cm-3 instead of kg.cm-3). Please provide percentages to allow a better comparison of the different experiments.
We think that SWE would provide a redundant information, because density and snow depth are already in the table. We do not have comprehensive information about the grain size from all experiments. The density unit will be corrected – (kg m^{-3}*).*

Page 16, Table 4: Please provide units (per-mille VSMOW) in the table. Should it be different instead of difference in the header of the table? However, please revise the header text for more clarity.
The units will be provided. We prefer using "difference" in the header as a result of subtraction. The header text will be revised.

Page 16, Table 3 and Table 3: Please thick about combining the two tables.
This comment was probably meant to combine Tables 5 and Table 6. We think that a combination of these tables would not be beneficial for the paper, because it would contain too much information. In the current manuscript Table 5 represents the results of hydrograph times and water velocity. On the other hand Table 6 represents results of water volumes within the hydrographs. We will consider a suitable combination without losing the information clarity.

Page 17, Table 5: ": : :events" in the table caption. The peak times (10 min) for sprinkling period 3 and 4 in experiment 3 seem to be wrong. Please check.
Plural will be added to the table caption. The peak times were checked and confirmed as correct.

Page 20, Figure 1: A real picture of the set-up of experiments would be nice to see.
Unfortunately, we do not have an appropriate real picture to add.

Page 21, Figure 2: The influence of rainwater isotope signature is missing. Is this figure really relevant and needed for the study?
The Figure represents the new hydrograph separation concept. However, it is just a graphical representation of formula 4 and therefore we will consider removing it.

References:

Dinçer, T., Payne, B. R., Florkowski, T., Martinec, J. and Tongiorgi, E.: Snowmelt runoff from measurements of tritium and oxygen-18, Water Resour. Res., 6(1), 110–124, doi:10.1029/WR006i001p00110, 1970.

Fierz, C., Armstrong, R. L., Durand, Y., Etchevers, P., Greene, E., Mcclung, D. M., Nishimura, K., Satyawali, P. K. and Sokratov, S. A.: The International Classification for Seasonal Snow on the Ground, 1st ed., IACS, Paris., 2009.

MacLean, R. A., English, M. C. and Schiff, S. L.: Hydrological and hydrochemical response of a small Canadian Shield catchment to late winter rain-on-snow events, Hydrol. Process., 9(April), 845–863, doi:10.1002/hyp.3360090803, 1995.

McDonnell, J. J., Bonell, M., Stewart, M. K. and Pearce, A. J.: Deuterium variations in storm rainfall: implications for stream hydrograph separation, Water Resour. Res., 26, 455–458, 1990.

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Taylor, S., Feng, X. and Renshaw, C. E.: Isotopic evolution of snowmelt 2. Verification and parameterization of a one-dimensional model using laboratory experiments, Water Resour. Res., 38(10), doi:10.1029/2001WR000815, 2002.