Response to reviewer 3, Daniele Penna

We thank Daniele Penna for revising and commenting the manuscript. We have reproduced those comments below (in normal type), with our responses (in bold).

General comment
First of all, I apologize with the Authors and Editor for my late review.
This is a very interesting manuscript that focuses on the role played by rain-on-snow (ROS) events in enhancing snowpack outflow and thus snowmelt, ultimately contributing to stream runoff. I worked for some years in a snow-dominated catchment and I had the opportunity to observe the significant impact that ROS events have on the catchment hydrological response in the melting period. Therefore, experimental work that provides a better understanding of the controls on snowmelt contribution to streamflow during ROS events is welcome and certainly appreciated by the readers of HESS. The manuscript is well written, solidly structured, nicely illustrated, with updated and relevant references, and the data well support the results interpretation. I basically agree with the comments by the two other Reviewers and I overall like the response of the Authors. I have only a few specific comments that I hope can contribute to improving the manuscript. In the end, I recommend a minor revision before publication.

Specific comments
- In agreement with Reviewer 1, I also noticed the lack of a clear and testable research hypothesis stemming from the knowledge gaps defined earlier in the Introduction. The Authors replied that the main hypothesis ": : :is that vegetation and elevation substantially affect the generation and the isotopic composition of snowpack outflow, and thus snowmelt contribution to streamflow. In my opinion, this reply is not fully satisfactorily.

First, "vegetation" is quite a vague term in this context: reading the rest of the manuscript and knowing the area it is clear that this term refers to forest trees but, in principle, this could be valid for understory vegetation as well. So, I suggest being more specific here.

> **We will clarify in the revised manuscript that vegetation is meant to be forest canopy.**

Secondly, what does it mean that vegetation and elevation affect snowpack outflow generation? I guess the Authors mean outflow amount or volumes, but again this should be specified. Most importantly, this is only the general hypothesis. I suggest to complement it with some specific hypotheses or specific research questions that better address the core of this work and around which the Results and Discussion section could be built.
For instance, one specific research question could focus on the role of rainfall characteristics and initial snowpack properties on the variability of snowpack outflow volume.

Another specific research question could deal with the spatial variability of snowmelt contribution to streamflow in the catchment (comparison of hydrograph separation results among the three sites) and a third one to the temporal variability of snowmelt contribution to streamflow (comparison of hydrograph separation results among different ROS events). These are only suggestions but I think that structuring the Results section so that its parts reflect the specific questions posed at the beginning would tell a clearer story and accompany the reader in a more linear way.

Thank you for this suggestion. We will propose a more general research hypothesis and include four more specific research questions at the end of the introduction.

- I think that what would be really interesting and novel is the application of three component hydrograph separation to quantify the proportion of rainfall and pre-event snowmelt during ROS events. As far as I understood, the instrumental design and the sampling scheme would allow for the application of this mixing model that, of course, requires the availability of a second tracer. Is there any additional tracer available? Is the application feasible? Are there theoretical or practical constraints that prevent this analysis? I wonder if the Authors already planned a follow up of this study considering this aspect. A comment on this is welcome.

We have also measured major anions and cations in all samples and it might be possible to use an additional tracer (such as magnesium, calcium) to separate the pre-event signature (high solute concentrations) from the rainwater and snowpack outflow signature (low solute concentration). We are planning on performing such an analysis, which, if it works, will result in a separate publication.

- At lines 268 and 269 the Authors stated that the pre-event tracer signature (by the way, talking about isotopes I think that the terms "signature" and "composition" are more appropriate than "concentration") was determined by sampling the stream on the day prior the ROS event. In a previous study in a snowmelt-dominated Alpine catchment (Penna et al., 2016, JoH, https://doi.org/10.1016/j.jhydrol.2016.03.040), we compared two different methods to determine the pre-event stream signature for two component hydrograph separation during snowmelt, i.e. the average of several samples taken during baseflow and a sample taken before the snowmelt-induced runoff event. We found, in some cases, marked differences in the estimated snowmelt proportions in the stream using the two methods, and we related these differences to the fact that streamflow may have contained a small amount of residual snowmelt water at night, especially late in the melt season, so that meltwater influenced the isotopic composition of the stream between melt events. In the case presented by the Authors, the ROS events occurred in winter (Jan-March) and so this effect might not be so important but, nevertheless, I wonder if this effect could happen here as well at least in the late winter events (e.g., March). A comment on this could be useful.

We will replace "concentration" with "composition" or "signature" in the revised manuscript.

For five of our six ROS events, pre-event water signatures were very similar ($\delta^{18}O$=-11.5±0.3 ‰, $\delta^{2}H$=-81.5±1.4 ‰), so that using a single pre-event water signature (determined from baseflow samples) would not substantially change our hydrograph separation results.

For event #5, pre-event water signatures were slightly lighter ($\delta^{18}O$= -12.8 ‰, $\delta^{2}H$= -86.3 ‰) than the average of the other five events due to very light rainfall during the preceding event #4. If the pre-event water signature would correspond to the mean value ($\delta^{18}O$=-11.5±0.3 ‰, $\delta^{2}H$=-81.5±1.4 ‰), we would underestimate the snowpack outflow contribution during event #5 (i.e., absolute percent differences were between 16 and 530). However, in our analysis we treat the stream water sample prior to each ROS event as our pre-event water regardless of whether another event occurred beforehand. Thus, we consider each event independently and not relative to baseflow conditions.

Minor comments and technical corrections

L126. Remove “at mid elevations”.

We will remove this in the revised manuscript.

L147-148. I suggest shortening the title.

We will change this.

L179: What is the relative measurement uncertainty of the tipping bucket?
We gave the average measurement uncertainties in L172, so we assume that this gives the required uncertainties.

L223. Remove the delta sign, it’s not needed here.

We will remove this in the revised manuscript.

L255. Replace “concentration” with “composition”.

We will replace this in the revised manuscript.

L259-260. Did the Authors/technicians apply any procedure to mitigate the carry over (memory) effect that can affect laser isotopic measurements when analysing subsequent samples with much different isotopic composition?

Every sample was measured a minimum of 6 times. To reduce the memory effect only the last 3 results were used and averaged to derive the isotopic composition of each individual sample.

Fig. 3b: Add “rainfall” before the word “retention” below the 1.1 line.

This will be added.

L445-446. I suggest skipping this, redundant with what previously mentioned in the M&M section.

We would intend to leave this in the revised manuscript, so the definition about snowpack water budget will be certainly clear to the reader.

L456. Is the regression statistically significant? Can the Authors report the p-value of this regression?

We will add the p-values of these linear regressions.

L540-542. This sentence is not necessary and can be skipped.

We will remove this sentence.

Fig. 6. It is not immediately clear to me which rain samples are, which snowmelt samples, and ROS samples and bulk snow, so I suggest making the box plot clearer.

We will do this in the revised manuscript.

In addition, did the Authors perform a statistical analysis in order to check for the differences in isotopic composition?

We will perform the analyses to check for differences in isotopic compositions between the different sources (at each site) and between the different sites (for the same source). We will include this information in the revised manuscript.

Moreover, I wonder if the slope of the regression lines in the dual isotope space (Fig. 6d-f) is statistically different between the MG site and the HG and MF sites (see, for examples, another isotopic study on rain and snowmelt in the Alpine catchment mentioned above, Penna et al., 2017, HP, https://doi.org/10.1002/hyp.11050). This could be performed and discussed in the light of the inter-site comparison in rain and snowmelt isotopic composition.

We have performed a statistical analysis and found that the slopes of the regression lines were not statistically different (p-value > 0.3). We will include this information in the revised manuscript.

L658. Replace "concentration" with "composition".

This will be changed in the revised manuscript.

L706-707. Which assumptions were violated to have unrealistic results?

The isotopic composition of the stream water and the rainwater were overlapping during event #4. We will clarify this in the revised manuscript.

Table 1: Unpaired two samples t-Tests of the differences between the four sample types (snowmelt, rain-on-snow, rain, bulk snowpack at each site) and between the three sites (HG, MG, MF site). Upper right triangle: t-values (in italic font); lower left triangle: p-values (regular font). Statistically significant differences, i.e., *p*-values < 0.01, are shown in bold font. Grey fields indicate sample combinations that are not informative.

	Location and sample type	HG_SPO Site				MG_SPO Site				MF_SPO Site		
		Snow-melt	ROS	Rain (no snow-pack)	Bulk Snow-pack	Snow-melt	ROS	Rain (no snow-pack)	Bulk Snow-pack	Snow-melt	ROS	Rain (no snow-pack)
HG_SPO Site	Snowmelt		*-1.811*	no rain	*-0.131*	*1.335*				*-0.163*		
	ROS	0.075		no rain	*1.408*		*0.987*				*-1.049*	
	Rain (no snowpack)	no rain	no rain					no rain				no rain
	Bulk Snowpack	0.896	0.169	no rain					*0.759*			
MG_SPO Site	Snowmelt	0.185					*-1.543*	*3.568*	*-0.091*	*1.262*		
	ROS		0.334			0.128		*3.289*	*1.263*		*1.469*	
	Rain (no snowpack)			no rain		**<0.01**	**<0.01**		*-2.778*			*-0.039*
	Bulk Snowpack				0.454	0.928	0.220	**<0.01**				
MF_SPO Site	Snowmelt	0.871				0.210					*-0.328*	*2.077*
	ROS		0.304				0.159			0.744		*1.740*
	Rain (no snowpack)							0.969		0.042	0.092	

Table 2: Coefficients of slope and intercept including standard error of the three sites (HG, MG, MF site). Analysis of variance (ANOVA) of the slopes of the regression lines in the dual isotope space at the HG_SPO, MG_SPO and MF_SPO sites. Upper right triangle: t-values (in italic font); lower left triangle: p-values (regular font). The three slopes are not statistically different, i.e., *p*-values > 0.01.

	Coefficients		Analysis of Variance		
Location	Slope	Intercept [‰]	HG_SPO	MG_SPO	MF_SPO
HG_SPO	8.03±0.2	13.7±2.6		*-1.602*	*-0.689*
MG_SPO	7.5±0.1	4.5±0.8	0.246		*-1.186*
MF_SPO	7.7±0.2	10.2±1.9	0.770	0.462	