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Comment on hess-2021-362

Anonymous Referee #1

Referee comment on "Effects of spatial and temporal variability in surface water inputs on streamflow generation and cessation in the rain-snow transition zone" by Leonie Kiewiet et al., Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2021-362-RC1>, 2021

General comments

The main objective of the study was to improve our understanding of runoff response to year-to-year variations in precipitation phase and magnitude at the rain-snow transition zone. The authors addressed two main research questions related to 1) spatial and temporal distribution of surface water inputs (SWI), defined as sum of rainfall and snowmelt at the rain-snow transition zone and its variation between particularly wet, dry, rainy or snowy years and 2) the response of stream discharge to the above variations in SWI.

Authors used observations from eleven weather stations and snow depths measured using lidar survey to set up a distributed snowpack model iSnobal/Automated Water Supply Model in a semi-arid, headwater catchment of Reynolds Creek, Idaho, USA. Authors found that runoff in a snow-rich year was almost twice as high as in a rainy year, despite similar SWI, although they did not find any relation to annual snowfall fraction. In addition, dry-out date of the catchment was positively correlated to the melt-out date.

In my opinion, authors did an interesting work. I certainly agree that the focus on the rain-snow transition zone is important since this zone might extend to higher elevations due to climate change which will influence the water balance and timing of catchment runoff. Although the results are not surprising as they mostly confirm our existing knowledge, I found the study important and particularly novel (see also comments below), thus appropriate for HESS. Since the study investigated the effect of SWI on runoff just for one experimental catchment and few years, the ability of results generalization to different regions and climate is limited. Therefore, I have some comments listed below, which should be addressed before I can recommend the manuscript for publication.

Specific comments

In my opinion, the novelty of the study should be better described. I agree that the focus on the rain-snow transition zone is important and particularly novel, but I would encourage authors to better highlight research gaps and how the study goes beyond to what has been done in the past. Therefore, some additional justification can be added to introduction section (e.g., after research questions).

Although authors used frequently applied iSnobal/AWSM model, which is well enough described in the literature, it would be good to provide the reader with more specific information about generating snowmelt runoff, which is specifically important for SWI calculation. For example, how does the model calculate snowmelt? For rain-on-snow situations, is the rainwater directly added to SWI at the specific time or is it temporarily stored and delayed in the snowpack? Does model account for refreezing? Does model consider sublimation from snowpack and canopy interception? These details are not fully described in the current manuscript, but I think they might help the reader with better understanding of how the SWI were calculated.

L 197: As authors correctly stated, the use of only one lidar survey to describe the snowpack spatial distribution for all study years brings some uncertainty. I see the point that the topography is the main control of snowpack variability. Nevertheless, the meteorological controls might be important as well, such as wind speed and direction influencing snow redistribution and accumulation on leeward sites of slopes. What is the prevailing wind direction? And was it same for all years during snowfall events (and thus likely causing same snowpack distribution)? I would like to see a bit more discussion related to the topic.

Table 2, Fig. S4: The model performance for north-facing stations and in the "Upper region" (Table 2) in the water year 2011 is relatively poor when comparing simulated and observed SWE values. In addition, even for one single station, simulations for some years are well enough, while this is not the case for another years (e.g., jdt1 and jdt4). Is there any explanation for both temporal and spatial differences in model performance? How confident are observed SWE data for individual stations?

The conclusion that the snowfall fraction is not correlated to annual runoff or day of stream drying is certainly important, but maybe not such surprising. The snowfall fraction does not contain the information about total amount of snowfall, but only its relation to the total amount of precipitation. It means, that a year with high snowfall fraction is not necessarily the year with overall high snowfall. Therefore, it would be maybe interesting to select more characteristics describing the snow conditions in different years (such as amount of snowfall during cold season, annual maximum SWE, amount of snowmelt in spring etc.) to better show whether or not the cold season snowfall could positively influence the stream drying compared to the same amount of rain. Perhaps, the results can be shown in some table (heatmap) of paired correlations between individual characteristics.

L 286-288: This part would maybe deserve a bit more attention since it touches the important issue of catchment storage and its "memory effect". I found this partial analysis interesting (despite the fact that results did not confirm an effect of "previous water year precipitation"). Therefore, I suggest some extension of the related text.

L 297-300: For day of stream drying, would it make more sense to account for sum of SWI preceding the day of stream drying instead of annual sum of SWI?

Although, I found the reasoning presented in results and discussion sections correct, the supporting illustrations are, in my opinion, less informative and I am not sure whether they fully support all the results and interpretation. For example, one of the main conclusions is that temporal distribution of SWI is more important than its total amount. While I agree with that, it is difficult to me to clearly see this in figures which mostly shows only time series (Figs. 4 and Fig.5). I do not have any clear suggestion how to make figures more informative and supporting the results, but I would encourage authors to reconsider their illustrations and perhaps add another figure which would better show how the timing of SWI influence the runoff response.

Technical corrections

L 116: The decrease in streamflow should be expressed in mm/decade to be comparable with other characteristics.

L 138: "stage height-discharge relationship". Maybe more common term "rating curve" would be better.

L 193: "Trujillo et al. (2019, manuscript in preparation)". As it seems from references, this paper has been already published.

Fig. 6a: The annual discharge is related to the precipitation at jdt125 climate station. Why not to show catchment mean precipitation instead? If I understood correctly, the model interpolates station data to a catchment scale using some kind of elevation dependency. Therefore, to show catchment precipitation in Fig. 6a makes more sense to me to make it better comparable to catchment runoff.

Fig. 6b: What the triangles represent? Maybe, there is a mistake in the figure as they represent "other years", but different symbol is used in the legend.