Response to the interactive comment of Reviewer#2

on "Studying catchment storm response using event and pre-event water volumes as fractions of precipitation rather than discharge" by Jana von Freyberg et al.

Best authors and editors,

Thank you for the possibility to review this very interesting manuscript, and apologies for the delay in my review.

The authors present a hydrograph separation studying the stream water sources in an experimental Erlenbach catchment in Switzerland. The work builds on an advanced field laboratory, enabling high-frequency determination of isotope composition in stream water and precipitation used in identifying pre-event and event water composition, respectively. Authors present an eight-month long dataset of isotope and hydrometric measurements for flow and precipitation, supplemented with groundwater level and soil moisture data as proxies for catchment wetness. As a subset of this data, they analyse 24 storms in greater detail. The results show the advantages in exploring the pre-event and event water contributions as a fraction of precipitation, not total streamflow as is typically done. Using this approach, the authors were able to infer novel insights to catchment controls on streamflow generation. I particularly enjoyed section "3.4 fingerprints of catchment response" in which the authors put forward interesting hypothesis to be tested by the hydrological community.

The manuscript is written with flawless English, and is well structured and presented. In my opinion both the collected and dataset and the following analysis are novel and of high quality, and therefore a great contribution to the hydrological sciences. I recommend this work to be published in HESS, and provide some minor remarks below.

We thank the reviewer for his/her assessment and his thoughtful comments, which we have addressed in detail below.

Comments of the reviewer are shown in italics. Responses from the authors are presented in regular font below each comment. Citations from the manuscript are in Times New Roman and changes of manuscript text are underlined.

comments:

P4L13: I would recommend the authors to better acknowledge and discuss prior work studying the Qe/P ratio in the introduction. Before this chapter, I had the impression this is being done the first time in the presented manuscript.

Indeed, except for the one study cited here (Ocampo et al., 2006), all other studies estimated Qe/P solely as a proxy for surface-runoff generating area. Nonetheless, in the revised version we will mention these prior studies earlier in the introduction, in the sentence after we introduce Qe/P. To the best of our knowledge, none of these studies, including Ocampo et al.'s study, used Qe/P the way we did, and therefore this is being done here for the first time.

P5L5 what do you mean by "saturated soils"? groundwater table is at ground level? Or that the soil type is prone to saturation? I presume that the extent of saturation would vary seasonally, so a static map for it seems simplified.

Soil surveys across the catchment landscape revealed that the soils are frequently saturated and this can be caused by both shallow groundwater tables or waterlogging of oncoming rainfall on low-permeability soils. Both processes are likely to co-occur and are difficult to separate (Fischer et al., 2015; Rinderer et al., 2017). We agree with the reviewer that a static map of soil wetness is a simplified description of the catchment wetness state, however, no data are available about the spatiotemporal variability of these areas.

What has been mapped in our study catchment, as we say clearly in the text, are zones where soil saturation is likely to occur, rather than locations that are saturated at any specific point in time.

Fischer, B. M. C., Rinderer, M., Schneider, P., Ewen, T., and Seibert, J.: Contributing sources to baseflow in pre-alpine headwaters using spatial snapshot sampling, Hydrol. Process., 29, 5321-5336, 10.1002/hyp.10529, 2015.

Rinderer, M., McGlynn, B. L., and van Meerveld, H. J.: Groundwater similarity across a watershed derived from time-warped and flow-corrected time series, Water Resour. Res., 53, 3921-3940, 10.1002/2016WR019856, 2017.

Fig.1: add a scale, the degree axis are not very intuitive of the catchment size

We will add a scale bar to Fig. 1.

P7L16: concentrations -> ratios?

We will change that: "We use the isotopic composition (δ^2H and δ^{18}O) of streamwater and precipitation"

P8L10: how is Q for each event defined and calculated?

We have moved the definitions of the start and end times of the event from Section 2.3 to Section 2.4*:* "The beginning of a storm event was the time of first rainfall, and the end of a storm event was defined as the time that (i) event water discharge declined to 5 % of its peak value or (ii) another precipitation event began, whichever came first; case (i) prevailed for 18 of 24 events." Q for each event is the sum of discharge between these beginning and ending times.

P10L1: add spacing for dates in all occurrences?

We will follow whatever format specification the journal requires, although we would like to keep this format as we use it here because it provides concise identifiers for the individual events.

P10 L15: I don't understand how the 4-hour peak Q 0.11 mm is lower than overall Q 0.5 mm. How is 4-hour peak Q defined?

These are cumulative sums, not rates (that is: total mm, not average mm/hr). 4-hour peak flow (Q_{4h}) is defined as the cumulative sum of discharge volume over a 4-hour time period. We will add an explanation to Section 2.3. Since the aggregation period is usually much longer for total cumulative discharge Q (aggregated over the entire event duration), Q_{4h} is smaller than Q.

Fig. 4: should y-axis be delta 18O?

We will correct that.

P10L 24: how about the point on the far right in both a) and b) plots? That deviates substantially from the 1:1 line.

For the event with the largest Q_e/Q values (18Aug2017), the uncertainties of the hydrograph separation with δ^{18}O and δ^2H were relatively large and thus the differences in the Q_e/Q values (i.e., the deviation from the 1:1-line) were statistically not significant (in the specific sense that they did not differ by more than twice their pooled standard errors). We will revise the text to make this clear.

P15L10 and table3: I don't find Qpe/Q data in table 3, though discussed in the text

Because Qpe/Q + Qe/Q =1, the correlation of Qpe/Q with anything will be simply the negative of the correlation of Qe/Q. We point this out several times in the paper, e.g. in the introduction (P4L5) and discussion (P14L10-11). We will add this information also to the legend of Table 3 and the caption of Fig. 7.

P17L6: I see this conclusion somewhat inconsistent with your data analysis so far. You suggest that the Pe could be explained by contraction and expansion of saturated areas, i.e. the antecedent conditions, whereas before you demonstrate and discuss how the Pe is mainly a function of the storm characteristics.

We are not sure whether the reviewer refers to Qe, Qpe or the ratios Qe/P? Regarding P17L6, which discusses Qe/P, we want to point out that some of these saturated areas form on impermeable surfaces or on waterlogged soils, which remain hydrologically isolated from the groundwater aquifer or the stream network most of the time. Therefore, the expansion and contraction of saturated areas in Erlenbach does not necessarily need to be reflected in the metrics of antecedent wetness conditions (e.g., Qini, GWini).