Response to the interactive comment of D. Penna

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.

General comment

I read the manuscript by von Freyberg and colleagues with keen interest. They use high-resolution stable isotope data of stream water and precipitation collected during 24 rainfall-runoff events in a small Swiss catchment to test the usefulness of an alternative metric for studying runoff generation processes at the catchment scale. They argue that the commonly adopted tracer-based estimates of event and pre-event water fractions of stream runoff (Qe/Q, Qpe/Q), typically used to analyze the fundamental controls on catchment hydrological response, may be ambiguous because the same controls on Qe and Qpe also necessary control the total discharge Q. Therefore, the authors suggest using the fraction of event and pre-event water relative to precipitation, instead (Qe/P and Qpe/P), asserting that it may provide an alternative and more insightful approach analyze catchment hydrological response. The authors support their thoughts with clear field-based evidence and produce convincing results showing the effectiveness of this alternative metric to reveal runoff generation processes, at least in the study catchment.

This study contains a high degree of novelty, and constitutes a scientific advancement in catchment hydrological science as it can open up new ways to take the best advantage of the more and more widely adopted stable isotopes in water to investigate hydrological processes at the catchment scale.

Overall, the manuscript is very well written, logically organized, and clearly illustrated. All methods are clearly described, the authors' thoughts can be followed effortlessly, and the results are solidly supported by the data. There are some parts where a certain degree of redundancy exists but this does not hurt and may even help stress some relevant points. I have only a few comments to improve the manuscript, and I recommend a minor revision before publication.

We thank Dr. Daniele Penna for this positive 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.

Specific comments

P1L9-10, and P1L18. Here, and in other parts of the manuscript, I suggest specifying that this work focuses on the two-component hydrograph separation, which is used to estimate the pre-event and event component of stream runoff.

Thank you for pointing this out, we will change that.

*Indeed, the word "source", used, for instance, at P1L18, P2L17 is, in my opinion, a bit vague: one tracer (often isotopes), two-component hydrograph separation is typically used to estimate *time* source components of total discharge, whereas two-tracer (or more than two tracers, usually isotopes plus hydrochemical tracers), three (or multi-) component hydrograph separation is often used to estimate *geographical* source components (Klaus and McDonnell, 2013), such as snowmelt, glacier melt, or hillslope soil water, riparian soil water, shallow groundwater etc. I think that this specification and distinction should be made clear in the abstract and throughout the paper (not only in the title of Section 2.4).*

We will change that: "Tracer-based, two-component hydrograph separation uses stable water isotopes (^2H, ^{18}O) to estimate the relative time source components of streamflow, i.e. recent precipitation (event water, Q_e/Q) and catchment storage (pre-event water, Q_{pe}/Q; Klaus and McDonnell, 2013). "

P3L10. This is true and was shown in several studies. However, also the opposite can happen, depending on the specific catchment properties. For instance, in our Dolomitic experimental catchment tracer data showed that, on average, Qe increases with increasing antecedent moisture conditions mainly due to the streamflow contribution of saturation overland flow, formed by a mixture of rain water falling on the saturated areas and pre-event water exfiltrating in the riparian zone (Penna et al., 2016). This may occur also in other catchments.

Penna, D., van Meerveld, H.J., Zuecco, G., Dalla Fontana, G., Borga, M., 2016. Hydrological response of an Alpine catchment to rainfall and snowmelt events. Journal of Hydrology 537, 382–397. https://doi.org/10.1016/j.jhydrol.2016.03.040

We will include this reference into the revised version of the manuscript: "In contrast, at sites where positive relationships between Q_e/Q and antecedent wetness have been observed, it has been hypothesized that vertical infiltration must first replenish storage deficits before event water can be rapidly transported via lateral flow pathways or surface runoff (Shanley et al, 2002; von Freyberg et al., 2017), or that the expansion of saturated areas in the catchment enhances direct runoff of rainwater (Penna et al., 2016)."

P4L15-20. The general aim of the study is clear but I suggest formulating here specific objectives and/or a clear testable hypothesis.

We will re-phrase this section: "From the general concepts outlined above, we hypothesize that event and pre-event runoff coefficients Q_e/P and Q_{pe}/P may be more informative metrics for studying catchment storm responses, compared to the widely used event water fraction of discharge Q_e/Q or the runoff coefficient Q/P. In this paper, we test this hypothesis by comparing runoff coefficients Q/P and the different ratios Q_e/Q, Q_{pe}/Q, Q_e/P and Q_{pe}/P across 24 storm events and analyzing their relationships with storm characteristics and antecedent moisture. These relationships shed light on possible streamflow generation processes at our study site and highlight the potential benefits of using Q_e/P and Q_{pe}/P rather than Q/P or Q_e/Q to characterize catchment storm response."

P7, Section 2.3. I think that the selection of metrics used to characterize the storm properties and the antecedent wetness conditions are appropriate. However, I think it would be interesting to add the combination of P and SMini as a metric (e.g.,Detty and McGuire, 2010; Fu et al., 2013) and see if and how the fractions of Qe/Q, Qpe/Q, and most of all Qe/P and Qpe/P are sensitive to it.

Detty JM, McGuire KJ. 2010. Threshold changes in storm runoff generation at a till-mantled headwater catchment. Water Resources Research 46: W07525. DOI:10.1029/2009WR008102

Fu C, Cheng J, Jiang H, Dong L. 2013. Threshold behavior in a fissured granitic catchment in southern China: (1) analysis of field monitoring results. Water Resources Research 49: 1–17. DOI: 10.1002/wrcr.20191

We agree that the combined metric of *SMini* and *P* might provide an interesting analysis, however, in our case the event-to-event variations in *P* are much larger than those in *SMini*. Therefore, the correlations with the combined metric (*P+SMini*) are very similar to those obtained for *P*.

P18L11. I agree but AP7 (and AP indices in general) is only a surrogate of the catchment antecedent wetness status (Ali and Roy, 2010) and therefore this relation could not be robust and reliable "fingerprint". Maybe a sentence on this could be added.

Ali, G. A. and Roy, A. G.: A case study on the use of appropriate surrogates for antecedent moisture conditions (AMCs), Hydrol. Earth Syst. Sci., 14, 1843-1861, https://doi.org/10.5194/hess-14-1843-2010, 2010.

We will add this information to acknowledge the comment of the reviewer: "Under the assumption that *AP7* is a reliable surrogate for catchment antecedent moisture, the slope of the *AP7*-Q_{pe}/P relationship could be considered as an index of how antecedent moisture alters the fraction of the catchment in which stored, pre-event water can be efficiently mobilized by streamflow. "

Minor comments and technical corrections

P1L9. It is not immediately clear if the terms "streamflow" and "discharge" are used interchangeably or if they imply a different meaning. In the first case, I suggest to use one term consistently. In the second case, I suggest to indicate the possible distinction.

In our analysis, the term “streamflow” usually refers to the hydrological behavior or status of a system (i.e., the streamflow regime, the streamflow hydrograph), whereas “discharge” refers to the variable *Q* that is used for our conceptual and theoretical explanations. We will clarify this terminology throughout the manuscript.

P5L3. I suggest replacing “soil depths are shallower” with “soils are shallower”.

We will change that

P8L6. So, in the last 2.5 hours?

Not necessarily. If a drift control was measured before the beginning of an event, the time interval becomes 3 hours.

P10L21. Please, report the p-value here as well.

The p-value was <0.0001 in all cases. We will add this information.

P12L7. “river”: earlier in the manuscript the authors used the term “streamwater” (eg, P7L17), so I imagine (also considering the catchment size) that the term “stream” is more appropriate here.

We will correct that.

P14L2-3. As far as I understand, the authors here mean “but are not, however, statistically significant (p>0.01)” or “but are, however, statistically not significant (p>0.01)”.

Thank you for catching this error. We will correct that.

P14L26. I suggest adding a reference here. Examples might be McGlynn and McDonnell (2003), James and Roulet (2009), Muñoz-Villers and McDonnell (2012).

James AL, Roulet NT. 2009. Antecedent moisture conditions and catchment morphology as controls on spatial patterns of runoff generation in small forest catchments. Journal of Hydrology 377(3-4): 351–366. DOI: 10.1016/j.jhydrol.2009.08.039

McGlynn, B. L., and J. J. McDonnell (2003), Quantifying the relative contributions of riparian and hillslope zones to catchment runoff, Water Resour. Res., 39, 1310, doi:10.1029/2003WR002091, 11.

Muñoz-Villers LE, McDonnell JJ. 2012. Runoff generation in a steep, tropical montane cloud forest catchment on permeable volcanic substrate, Water Resources Research 48: W09528. DOI: 10.1029/ 2011WR011316

These studies did not explicitly analyze the relationships of antecedent wetness metrics to *Qpe/P*, which is discussed in this section. Therefore, these references are not directly relevant here.

P14L28. “which sum to the runoff coefficient itself: Q/P=Qe/P+Qpe/P”. This has been said more than once before, and can be dropped.

We will change that.

P15L3. I suggest replacing “tightly” with “strongly”.

We will change that.

P18L18. I suggest replacing “forever” with “also for large values” or something similar.

We will change that: “We also note that ~~none of~~ these four ~~relationships can remain linear forever, because all of these~~ ratios are logically constrained to be ≤1, and thus they must become asymptotic at some point.”

Table 2. I suggest dropping the second and the third column (Q and P) because already reported in Table 1. This can improve the readability of the table.

We agree and will change Table 2 accordingly.

Fig. 2. The label of the last panel should be "d)" and not "e)".

We will correct that.

Fig. 3. So, if I understand well, this flow duration curve is a combination of two distinct periods. I wonder whether it would be more appropriate to show two curves for the two periods separately.

Since we do not consider the two periods separately in the analysis, we would like to refrain from showing two flow duration curves.

Fig. 4. In the label of the two y-axis correct "18Q" with "18O".

Thank you for catching this error, we will correct Fig. 4.

Fig. 7. The caption is not complete.

Sorry, the last two lines of the caption ended up on the next page of the automatically-generated PDF.