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Hydrology and Earth System Sciences
Discussions

Interactive comment 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.

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Review of the manuscript 'Studying catchment storm response using event and pre-event water volumes as fractions of precipitation rather than discharge' by von Freyberg et al. (hess-2018-401)

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.

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. 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).

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

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

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

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.

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.

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

P8L6. So, in the last 2.5 hours?

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

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.

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 ($p>0.01$)”.

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

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.

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

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

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.

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

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

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

Fig. 7. The caption is not complete.

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