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Discussions

# *Interactive comment on* "Seasonal partitioning of precipitation between streamflow and evapotranspiration, inferred from end-member splitting analysis" *by* James W. Kirchner and Scott T. Allen

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Review for manuscript Manuscript ID: HESS-2019-420 – Seasonal partitioning of precipitation between streamflow and evapotranspiration, inferred from end-member splitting analysis

Best authors and editors, Thank you for the opportunity to review this interesting manuscript. Authors present a concept of "end-member splitting" to study partitioning of precipitation to different fates (streamflow or evapotranspiration) and how this



partitioning varies in different seasons. To demonstrate the use and usefulness of their concept, they use a long-term hydrometric and water isotope dataset from the well-studied Hubbard Brook experimental watershed. With the analysis, the authors are able to analyse percentages of precipitation (importantly with uncertainty estimates) that end up in different hydrological fluxes in different hydrological seasons, even in different months. They compare the resulting partitioning with young water fractions, new water fractions using the same dataset, and report roughly similar outcomes from different analysis.

In my opinion the work is of great interests to the hydrological community. The work brings forward a data-based technique to study how water is stored in catchments over seasons, which has so far been difficult to demonstrate without numerical modelling. Also, the analysis gives a new handle on seasonal water sources for evapotranspiration, which I think is ecohydrologically highly relevant. Even though the authors modestly don't want to emphasise novelty of the work, I think the analysis and examples they present a great framework (and an example dataset after the data and scripts gets deposited and made available) to run similar analysis in other regions of the world and improve our understanding about catchment-scale hydrological partitioning. As similar water isotope data are increasingly being collected in different environments, the analysis presented here could be readily transferred elsewhere. My only concerns are related to how snow processes are accounted for, see comments for details.

The paper is well written, although the structure of the paper is unorthodox; equations in the introduction, methods and results combined in a long "proof of concept" chapter. However, I think the paper flows well, probably better than cramming all equations to a methods chapter. In my reading the calculations are valid, and figures are of good quality. I recommend the manuscript to be published, and offer some suggestions for further consideration.

Sincerely, Pertti Ala-aho

comments: The only real concern I have is related to winter isotope end-member in you proof-of-concept analysis: you have samples for winter precipitation (snowfall) but the water that catchment receives to further partition between streamflow and ET is snowmelt. There is evidence that snowmelt water is enriched in heavy isotopes compared to cumulative snowfall (Koeniger et al. 2008, Claassen et al. 1995, Ala-aho et al. 2017, Earman et al 2006). Particularly so for catchments with significant snow interception, which I understand is the case in Hubbart Brook catchment (Penn et al 2012). The (few) studies reporting canopy-influenced snow enrichment over the whole snow season show enrichment of 2-3 ‰ in d18O, translating to ∼15-25 ‰ in d2H. I suppose you could test for snowpack isotope enrichment in Hubbard Brook with snowmelt data from Hooper and Shoemaker (1986)? If your winter precipitation end-member would indeed turn out biased because of snowpack enrichment in heavy isotopes, this could make the isotope signature in your winter end-member pretty close to what you have for the dormant season (Fig. 3a and 3b). With regard to your proof-of-concept analysis, I guess such shift would influence your reported partitioning percentages and related uncertainties (more winter signal and more uncertainty throughout the system?). More generally, this would bring uncertainties in site comparisons between snow-influenced and snow-free catchments. You touch upon biased sampling of end-members on P22L654, and I would like to see further discussion if and how systematic bias caused by heavy isotope enrichment in seasonal snowpack might influence your analysis.

P3 L60: if by “this example” you refer to the proof-of-concept analysis in the paper, to be precise you study where winter season precipitation, not snowmelt, ends up.

P4 EQ7: Q_a*n_A->M: M should be X?

P12 L342-P13 L371: not sure how interesting this analysis is, in context of the message you put forward in the paper. I suggest to consider if this analysis is crucial to your work - leaving it out would further streamline the paper.



L373: I would not think it is striking that large part of the snowy season precipitation leaves the catchment during rainy season because of snowmelt in April and May happens in the rainy season, as you discuss later on in the chapter.

P13 L378 do you imply that ∼30% of winter precipitation is lost to evaporation/sublimation over the snow season? According Penn et al. (2012) seems like snow interception sublimation can account for 10-30% of snowfall in the region (not accounting for sublimation from ground snow), and numbers alike are typically reported (Varhola et al 2010). Relating this with your ET composed of snowy season precipitation 18 +-18% in Fig. 4, bulk of winter-sourced ET would be from snow sublimation (sublimation by definition sourced from winter precipitation), which leaves little to none winter precipitation for summer ET. This does not seem intuitive, referring to your very nice Allen et al. (2019) paper. From the water balance equation (13) sublimation should be embedded in the total ET, but to me the numbers don’t add up. Can you please clarify how your analysis takes snow sublimation into account?

Fig.6: typo in the caption, Figure x6

L548: do you mean “precipitation”, instead of “discharge”?

P20L596: “we” instead of “I”?

P26 L770-779: I was surprised by the discrepancy between your analysis the Allen et al (2019) results, even though the environment is different. Looking forward to the end-member splitting analysis with xylem and soil water data.

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