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Interactive comment on "The importance of spatio-temporal snowmelt variability for isotopic hydrograph separation in a high-elevation catchment" *by* J. Schmieder et al.

Anonymous Referee #1

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General comment This paper analyses the spatio-temporal variability of snowmelt and of its composition in stable isotopes in an alpine catchment and takes advantage of different weighting methods to calculate the isotopic composition of snowmelt, based on melt rates, with the aim to assess the impact of snowmelt variability on the results of two-component hydrograph separation. This is certainly an original idea that tries to overcomes the issues deriving from the highly variable isotopic composition of snowmelt both in space and in time, as largely document by literature studies. Therefore, this research is surely interesting for the readers of HESS. The manuscript is well written, logically organized, clearly structured. The Introduction leads to research questions that are developed in the text, with data that well support the results. Graphs

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Discussion paper

are very well prepared and tables are generally meaningful. I have only few major comments and indications that could be useful to address in order to increase the clarity, and thus the impact, of this work (see below).

Specific comments

2, 32-35. This is a critical part of the Introduction and should be better explained. It seems to me that what stated here is more relevant for the temporal variability rather than the spatial variability. Please, specify.

3, 17-18. The third objective seems to me more a tool than a specific objective itself. I suggest to revise it.

4, 22-24. This is interesting, and I congratulate the authors for having collected both bulk snowmelt samples and sub-daily samples to assess the diurnal variability of snowmelt. However, as far as I see, no data are presented or no discussion is reported to compare the bulk with the sub-daily samples. I encourage the authors to do so because, in my opinion, knowing which variability we miss is we sample only once at the end of the day (bulk sample) instead of taking more samples during an individual melt event at the daily scale would be of great practical interest.

4, 6. According to Figure 2, Table 3 and 4, samples were collected (and hydrograph separation was conducted) for two snowmelt events in the early melt season (23 and 24 of April) and two snowmelt events in the late melt season (7 and 8 of June) but the authors talk about 'two short-term melt events (3 days)'. This is not clear because usually diurnal-melt driven fluctuation in discharge are considered as individual melt events, so I count here 4 runoff events. More importantly, results are often presented in term of 'early melt' and 'peak melt', so, I believe, averaging or integrating the two couples of events. This operation could partly mask the intrinsic variability of the 4 events and therefore I suggest to present the data and the results separately (as, for instance, in Fig. 4).

4, 15-16. Two samples only to characterize baseflow and therefore the isotopic composition of pre-event water can be too little and a potential weak point for the following calculations. This aspect should be briefly discussed in the Discussion section or in section 5.6. Moreover, a recent work analyses the impact of sampling strategy of pre-event water (before the individual melt event or before the start of the freshet period) for two-component hydrograph separation during snowmelt periods. The discussion about characterization of the pre-event water isotopic composition could start from the results obtained in the recent paper by Penna et al. (2016) (see suggested additional references below). Please note that only one sample of pre-event water is visible in Fig. 3: are the two samples so similar?

6, 23-24. The diurnal temporal variation of snowmelt isotopic signal (0.5 per mil) was used for the uncertainty of the event component in the hydrograph separation, according to the traditional approach by Genereux (1998). This is fine but one aspect is not clear to me: was the same variability used for each of the 4 events? I think it is very unlikely that the 4 of them have the same diurnal temporal variability. Instead, the variability of each day should be used for the assessment of uncertainty of each runoff event. Please, fix this or explain.

6, 24. Here, the standard deviation of the two baseflow samples s relative small and it reflects, I assume, in small uncertainty values. I wonder if, having many baseflow samples, the variability would be greater and so the uncertainty of the pre-event component (see my comment above on 4, 15-16).

11, 17-18. This is, to me, contrasting to what stated at 4, 22-24. I think that while the bulk sample integrate the diurnal melting cycle it also smooths out the variability of the snowmelt signal very much. This should be tested, reported and discussed.

The figures should be introduced following an order (1, 2, 3. . .). See, for instance at 4, 7; 7, 35;

Terminology: I recommend to consistently change the term ‘isotopic content’ into the

more physically appropriate ‘isotopic composition’. I also suggest to replace the term ‘isotopic hydrograph separation’ into ‘isotope-based hydrograph separation’.

Figures 3. Use different colours or symbols for snowmelt and snowpack to better distinguish them. One pre-event samples is missing. Moreover, since there is an equation for the local meteoric water line, I suggest to plot it instead of the global meteoric water line. . .it makes more sense.

Figure 4 and Figure 5. They are quite clear but I think that the information could be conveyed much more clearly by using box-plots instead. Please, consider changing these ‘1-D scatterplots’ (by the way, is this the right term?) into box-plots.

Figure 6 and Figure 7 are too small but this is probably due to the editorial form.

Table 1. If Figure 4 and Figure 5 are converted to box-plots this table could be probably skipped because redundant. Please, consider this possibility.

Table 2. I think that it would be more informative to report the values of the two pre-event samples individually. As I stated before, reporting the average of streamflow during the two early melt and the two late melt events is not so informative to me. Consider reporting all data in a different way (box-plots again) or even skipping this table and reporting the values of the two pre-event samples in the text.

Table 3. Could this table be incorporated as bar plot in Figure 8? Please, consider the feasibility of this suggestion.

Table 5. Why is there no uncertainty reported for the peak event water fraction? According to Genereux (1998) it can be computed. Please, fix this.

Minor comments and technical corrections

1, 11. I suggest to remove ‘unknown’.

2, 39. Explain shortly which are the mentioned shortcomings.

3, 7. For consistency, ‘describe’ should be ‘described’.

12, 15. Typo: ‘were’ should be ‘where’.

12, 29. The title is too long, please revise.

13, 2. Is ‘deployed’ the right term here?

References

The correct citation for Birkel et al., 2011 is: Birkel, C., D. Tetzlaff, S. M. Dunn, and C. Soulsby (2011), Using time domain and geographic source tracers to conceptualize streamflow generation processes in lumped rainfall-runoff models, Water Resour. Res., 47, W02515, doi:10.1029/2010WR009547.

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The correct citation for Engel et al., 2015 is: Engel, M., Penna, D., Bertoldi, G., Dell’Agnese, A., Soulsby, C., and Comiti, F. (2016) Identifying run-off contributions during melt-induced run-off events in a glacierized alpine catchment. Hydrol. Process., 30: 343–364. doi: 10.1002/hyp.10577.

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Additional relevant references that should be considered are:

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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. doi:10.1016/j.jhydrol.2016.03.040

Shanley, J. B., Kendall, C., Smith, T. E., Wolock, D. M. and McDonnell, J. J. (2002), Controls on old and new water contributions to stream flow at some nested catchments in Vermont, USA. Hydrol. Process., 16: 589–609. doi: 10.1002/hyp.312

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