

Interactive comment on “Sensitivity of young water fractions to hydro-climatic forcing and landscape properties across 22 Swiss catchments” by Jana von Freyberg et al.

Anonymous Referee #1

Received and published: 10 January 2018

General comments

This discussion paper presents an analysis of young water fractions (Fyw) in contrasting catchments across Switzerland. The paper first examines the influence of interpolation methods, flow-weighting of measurements and snow in the calculation of Fyw. The second part studies correlations between young water fractions and catchment characteristics. The authors then introduce a new metric (i.e., the discharge sensitivity of Fyw) and relate this metric to catchment characteristics. The paper concludes with a conceptualization of the relationship between young water fractions and streamflow. The paper is well written and addresses important problems in the analysis of iso-

[Printer-friendly version](#)

[Discussion paper](#)



tope data, i.e., interpolation, impact of snow and flow-weighting. Moreover, the paper presents a new concept derived from the recently introduced young water fractions. However, there are some parts that need clarification and rearranging especially in the theoretical and methodological sections.

Specific comments

*Abstract

- Page 1, line 23: suggest clarifying what “this relationship” is (i.e., the relationship between flow and young water fractions).

*Theoretical background

- Page 5, lines 17-20: please clarify that equations (3) and (4) follow from (1) and (2).

- Page 5, lines 25-27: it is not entirely clear what is meant with volume-weighting. I assume it does not refer to the isotope values themselves (so volume-weighting over several samples to obtain a weighted catchment average, as done for the precipitation isotope values), but to the weighting scheme within the IRLS algorithm.

*Data set

- Page 8, lines 6-10: suggest dropping the German terms of the soil properties as this will not mean anything to most readers.

*Results/Discussion

- Due to the concise description of the interpolation methods in the main text, it is not easy for the reader to follow the different steps of the two interpolation methods, although this would be helpful to better understand the differences between the two methods. Moreover, method 2 has been developed by the authors, so this method should be introduced more extensively in the main text. I would thus suggest restructuring the paper by moving major parts of the methodology description from the Supplement to the main text. This could be placed into a subsection of section 3 or a

[Printer-friendly version](#)[Discussion paper](#)

separate methodological section. Please also explain method 2 in a bit more detail – in the main text, this method is described with one long sentence only. The comparison between the two methods can be kept in section 4.1, which would be more consistent with presenting results only in section 4.

- Page 8, line 26: are these cumulative monthly d18O-values in precipitation (so sampling bottle emptied each month)?

- Page 9, line 25; page 11, line 5 and line 16: “statistically (in)significant” using which statistical method?

- Page 11, line 25: this is the first time the authors mention “gamma distributions”. Please clarify that this refers to the underlying transit time distribution model.

- Page 12, lines 29-31: suggest weakening this statement (“consistent with . . .”) as results from a global analysis should be compared with caution to regional analyses and the smaller Fyw in this study could also be caused by various factors other than the gradient dependence. See also page 19, lines 7-8.

- Page 14, lines 15-20: please give a bit more details on the procedure: how many measurements were on average available in each sine-wave regression after separation by flow regimes? Was the number of values sufficient to obtain reliable results? I would expect the seasonal variations to be small and potentially indiscernible under low-flow conditions, when streamflow is dominated by the well-mixed signal of slow flowpaths.

- Page 15, line 3 – page 16, line 4: suggest introducing the concept of discharge sensitivity earlier in the manuscript as a methodological (sub)section and just presenting the results in section 6.2.

- Page 15, lines 13-14: add “algorithm” to “analytic Gauss-Newton”.

- Page 17, lines 7-14: this paragraph is closely related to the paragraph on page 16, lines 16-29. I suggest moving it accordingly.

- Page 17, lines 8-10: please rephrase this sentence to clarify. Do you mean "...exhibit significant positive correlations with Fyw but also statistically negative correlations with the discharge sensitivity of Fyw."?

*Summary and Conclusions

- Here or in previous section: please discuss in a bit more detail the additional information content of the discharge sensitivity. Long-term isotope data of good resolution such as in this study are not a given, so it might be good to know if (what) Fyw can tell us more than "traditional" hydrologic indices addressing flow variability (e.g., CVQ)?

- Page 18, line 31: suggest dropping "however" as this might be confusing to the reader

- Page 19, line 9: suggest replacing "found" by, for example, "hypothesize" as this follows from the conceptual model.

*Figures

- Figure 6: it might be the pdf version, but I can barely discern light blue points.

*Supplement

- suggest adding a map showing the 22 catchments and the 19 long-term monitoring stations for d18O-values in precipitation so the reader can get an idea of the spatial coverage of the measurements. Alternatively, the station locations can be added to Fig. 2.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2017-720>, 2017.

Printer-friendly version

Discussion paper

