

Interactive comment on “Exploring water cycle dynamics through sampling multitude stable water isotope pools in a small developed landscape of Germany” by N. Orlowski et al.

Anonymous Referee #1

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This study presents stable water isotopic data for a catchment in Germany. While there is much variability in the precipitation isotopes, there was little in the groundwater and stream water. The manuscript is well written and easy to read, however, lacks rigor and has limited quantifications backing up the main conclusions. This makes it difficult to see novelty and assess what the actually contribution of the work is toward advancing understanding of rainfall-runoff processes in a general sense (i.e., beyond the empirical sense of this specific location).

The challenge to the authors, which will require significant revision and additional analysis, is to take their data and develop a procedure/approach overcoming the lack of

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variability. While I can appreciate that the lack of variability “restricted the use of classical isotope hydrology techniques” (P1810L25), it leads to two clear questions: (1) Why not develop a new method to leverage the data you have or (2) Why not measure other tracers (geochemicals or electrical conductivity come to mind) that better map the flow domain? The authors opt towards choice (1) which I can agree with since it is difficult to add analysis and sampling after the fact. This makes the central question of this research how to turn lemons into lemonade? Personally, I think this is a pretty important central question given the ubiquity with which stable water isotopic data is currently being collected. However, the approach developed is extremely qualitative so it leaves the reader wondering what we have learned here and how to learn the same thing at a different location. The authors do a good job highlighting the need for such knowledge (see around P1812L2-6) but the study leaves something to be desired on how to achieve such knowledge.

Of course, I do not have any great suggestions on what that method/approach could be since that is the core of the research. What I am looking for here is a methodology that can allow for comparisons of this site to others. For example, perhaps the connections to land cover could be leveraged to develop a basic mixing model allowing for characterization of impacts of spatial vegetation pattern heterogeneity on hydrological response in these types of landscapes? Then you could do some cross validation estimates (bootstrap or leave-on-out type) on the error and uncertainties incurred? Development of a modeling extension (in whatever form) is recommended here to help provide a clear hypothesis testing/quantification framework. This would help demonstrate exactly how “conducting a stable water isotope study in Schwingbach catchment helped to identify relationships between precipitation, stream, soil, and groundwater in a developed catchment” (P1832L13). That would constitute a significant contribution and move this study beyond its heavy empirical tilt (which is needed)

Good that the hypotheses are clearly stated. However, there are some ambiguous words in there that reinforce the qualitative nature of the study and it is questionable

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how testable these really are. For example in hypothesis (1), what do you really mean by strong? Does it mean high in amplitude or quick changes (steep slopes in time)? It would be good to put this in the context of something measurable or quantifiable. For hypothesis (2), there is an inherent assumption of instantaneous mixing throughout the groundwater. Early work from Sarah Dunn [Dunn SM, McDonnell JJ, Vache KB. 2007. Factors influencing the residence time of catchment waters: A virtual experiment approach. *Water Resources Research* 43: W06408] and more recent work by others (e.g., Markus Hrachowitz [Hrachowitz M, Savenije H, Bogaard TA, Tetzlaff D, Soulsby C. 2013. What can flux tracking teach us about water age distribution patterns and their temporal dynamics? *Hydrology and Earth System Sciences* 17: 533–564]; Ype van der Velde [Van der Velde Y, Torfs PJJF, van der Zee SEATM, Uijlenhoet R. 2012. Quantifying catchment-scale mixing and its effects on time-varying travel time distributions. *Water Resources Research* 48: W06536]) have really questioned such complete mixing. So, this might actually be a rather poorly constructed hypothesis. Lastly, hypothesis (3) would require measuring physical distributions of flow pathways in the subsurface at a scale not really achieved here, would it not? Why not focus in on a clear and testable hypothesis to better streamline the presentation of a central key finding? I think this will be achieved when considering the recommendations of the previous paragraph.

Lastly, the results and discussion should be separated. Having them combined contributes to the qualitative feel of the presentation. It makes the results read more like a story than a presentation of substantial findings. Further, the findings seem to echo much of what is already seen in the literature. This potentially points to a lack of novelty. With this, I think the study has a bit farther to come before it can be considered ready for publication in HESS. As it reads now, it is more suitable for a regional journal or a journal with a more empirical focus (which I think the authors can move beyond).

Minor/Editorial Comments

The title does not seem grammatically correct. Should be something like: “Exploring

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water cycle dynamics by sampling a multitude of stable water isotope pools. . .” or “Exploring water cycle dynamics through sampling multiple stable water isotope pools. . .”

The last paragraph of the introduction is awkward. Since this is the paragraph that sets the tone for the presentation, it is fairly important. What was the “former” study? Are you referencing previous work that already used these data? Please improve this (see also general comments).

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