

## ***Interactive comment on “Stable water isotope tracing through hydrological models for disentangling runoff generation processes at the hillslope scale” by D. Windhorst et al.***

**Anonymous Referee #2**

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The paper puts forward a nice data set and a nice modeling application; however, suffers from poor presentation and structure. The novelty and portability of the study look limited as the study is currently packaged. Further, it is not clear what the central experiment really is in this study. I recommend substantial re-writing and re-structuring to help address these concerns. Without that, the study comes across as presenting another model application to another field site when it should be clear that the experiment is on the added value of these wick-samples soil data on improving model structure and/or defining process representation. As this is a relatively short paper, it should not be an issue to extend the study and consider such aspects.

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### General comments

The recommendations of considering “constrained, more complex modeling approaches” (P5182L16) makes me anticipate that the study will look at models of various complexities and assess the added value of isotopic data. Further, by line 22 on the same page, I get the feeling we already know much about the hydrology of this sight. Altogether, it leaves me wondering what we are learning (or where the novelty lies) in this study. More to the point: in what way does this study help progress understand in hydrology as a science? How are the central findings (or approaches) portable to other regions of the world? The authors should consider re-packaging the presentation to better highlight the general contribution of this study to the field of hydrology (the central novelty). As it is currently written, the lead in to this paper reads like a site-specific study, which I think is selling the work short (and not overly relevant for HESS).

Mixing results and discussion into one section further weakens the storyline and presentation of this study. It puts up a red flag to me that not much is going on here more than a model application. Model applications are typically not sufficient these days as standalone publications. I am wondering what the central experiment of the study really is. From the introduction and build up, I am anticipating a model complexity vs. gained process knowledge type experiment; however, in its current form, the study is far from delivering such an experiment.

Statements like the study is highlighting “the general suitability of high resolution soil water isotope profiles to improve our understanding” (P5195L16) really leave much to be desired. The model and setup of the manuscript should be shifted to quantify exactly how much the high resolution isotope profiles contribute. This may require additional modeling runs in revision in addition to repackaging the current results. Are these data reducing prediction errors or improving timing estimates or make parameters easier to define in the model? Such quantifications should be put forward. Then, in a separate discussion section, the merits or the cost-benefit of gathering such wick-sampled data (which are quite difficult to obtain) relative to the process understanding (and subsequent

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improvements) can be more fully considered. This increases portability of the study since it helps readers assess if they would benefit from employing such setups relative to what they already know hydrological processes in their own region of interest. Also, it could assist them in defining models of appropriate complexity.

Editorial/specific comments

P5180L5: Change to “processes involved”

P5180L25: Would this be a chance to highlight or mention other possible calibration targets? Consider connectivity (like recent work by Jencso) or travel times (like recent work by Hrachowitz or Heidbuchel)?

P5181L26: that is an awkward abbreviation for 1960’s... Double check with HESS standards.

P5182L10: Change “this” to “these”

P5182L22: wording is awkward in here. . .

P5184L10: Again, I am anticipating a model comparison study investigating the role of model complexity due to these statements.

P5186L2: I wonder how much the average reader cares about “your” understanding of hydrological processes within “your” study area. How portable are your findings?

P5186L18: There is nothing in this subsection?

P5188L25: Did you explicitly investigate the impact of these sizes? What is the smallest size you need to account for mixing processes of stable water isotopes?

P5192L9: “Our first hypothesis I” is poor wording (it implies multiple I hypotheses).

P5195L5: Rather than “stressing”, why not inherently demonstrate it? You have a really great tool to explore (quantify) the “importance of accounting” for various processes and the added value obtained through these data.

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P5196L10: if the digits are significant, it should be “1.0 years”

P5196L20: is  $a^{-1}$  the appropriate abbreviation here?

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 5179, 2014.

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