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Interactive comment on "Water ages in the critical zone of long-term experimental sites in northern latitudes" *by* Matthias Sprenger et al.

Anonymous Referee #3

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This study presents interesting insights on water age dynamics in vertical soil profiles. The authors build on previous model simulations (Sprenger et al., 2017) at 4 different northern-latitude sites based on the use of a 1-D physically-based model (SWIS). While in the previous publication the authors focused on flow and isotope transport dynamics, here the focus is on the modelled age dynamics. The article is very well written and easy to follow. Results are clearly organized and fully explained. I think this manuscript will be highly appreciated by the scientific community, therefore I recommend it for publication on HESS.

In revising the manuscript, I invite the authors to consider the following comments:

1) Highlight that results are based on a model and its assumptions: All the results are based on the implementation of the SWIS model. This model was shown (Sprenger et

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al., 2017) to provide reasonable soil moisture and isotope simulations. The model is evaluated on very valuable isotope data, but they only come from a single soil depth as no measurements are available at different depths or in the fluxes E, T and R. Hence, the age dynamics explored by the authors go well beyond what can be constrained by data (as typically happens in transport problems). I believe that rather different age dynamics (particularly the short-term dynamics) could likely yield equivalent model results in terms of isotope dynamics. This is fine and I do not invoke a sensitivity analysis, but keeping this uncertainty in mind, I encourage the authors to revise sentence like "Such a clear influence of vegetation on travel times" (P17L20) and to use more frequently expressions like "the model suggests that. . . " rather than "median age was. . .". Some critical discussion of the general validity of the analyses at the beginning of the discussion section would also help follow the discussion.

2) Additional insights on the SWIS model: As the paper is entirely based on the use of the SWIS model, I wonder whether further model descriptions exist that could be made available to the reader. The cited paper by Mueller et al., (2014) only includes a very short description of the model (it is just a sub-subsection of the paper!). As a reader, I came up with several questions (e.g. how does the vapour exchange simulated by the model may affect the age dynamics? How is interception modelled? How is recharge (and its age) partitioned between the different flow domains?) and it would be nice to have additional references where to find the answers.

3) Clarify the "inverse storage effect": The authors often mention the "inverse storage effect" (for example at P2L18, P14L4, P19L23) as described by Harman (2015). I think the original meaning of that terminology may have been partially misunderstood. The authors note that recharge is typically younger during higher storage periods. However, this is not enough to determine an "inverse storage effect" as recharge can be younger simply because soil water is younger (e.g. after a storm event). My understanding of what was originally intended by Harman is that during high storage conditions there are structural changes in the water transport mechanisms that lead to the activation of

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faster flow pathways, ultimately causing a disproportional increase of younger water in recharge (or ET) than in the soil storage. I think the paper would benefit from improved clarity on this point.

4) Simplify the Discussion: I found the discussion section rather long and often not reflecting the section titles. For example, section 4.1 “What controls soil water storage and water ages?” includes a very large number of remarks on general storage and age dynamics (and page 15 looks like a single paragraph of 35 lines). I think the authors could improve the discussion by better focusing on: what makes this study different from existing studies on water age? What is found here that was not known before? For example, part of the discussion on the two water worlds hypothesis (P15L22-33) resembles the one already presented by Sprenger et al., 2016, Rev of Geophysics. Then, some sentences (e.g., P14L17-20 P17L3-5, P18L10-15) express results that are somewhat expected in hydrologic transport processes and could be much shortened (I think it is well established that when it rains there is younger water that infiltrates into the soil and so the soil storage becomes younger, while during dry periods soil water becomes older – and so the fluxes out of the soil).

SPECIFIC COMMENTS

Page 2, Line 5: I think a reference to earlier papers would be in place here (e.g. van der Velde 2012, Water Resour Res, Botter et al., 2010, Water Resour Res)

P2L22: I think the reference to Berghuijs and Kirchner (2017) is not in place as the paper does not discus storage variations, which are instead the crucial point in the concept of the “inverse storage effect”.

P4L35: MTT usually refers to the mean transit time, so a reader that does not go through the methods will likely assume that those are mean transit times. No big deal, but I wonder if there is a more unambiguous acronym that could be used (and I am fine if the authors prefer to keep as is).

P4L34-36: I think some quick explanation on why the median is selected as travel time/age metric instead of the “traditional” mean transit time/age would be useful. The authors could specify that the median transit time (or age) is insensitive to what happens to the older component of the distribution (older than 50% of the particles). This makes the estimate more robust against the uncertainty on older water ages, but results in a “partial” metric that does not take into account the entire shape of the distribution (indeed, just the first 50%). On this, a reference to Benettin et al., 2017, Hydrol. Proc. would probably be more appropriate than Benettin et al. (2015).

P5L9: this sentence is unclear to me. To compute the median, you should only need to reach 50+% of the recovery. Instead, to compute the MTTD you need to average the entire breakthrough curves.

P5L24: technical correction: do you mean that distributions of median travel times and median water ages were derived using a cosine kernel density? I guess the age and travel time distributions were derived as described in the previous section.

Figure 5: could you show somewhere the partitioning between storage in fast flow and slow flow (maybe a figure in SI?). This would help understanding the dynamics in the total storage. Ideally it would be nice to see how E,T and R fluxes are partitioned between fast and slow domain, but I see that the article already includes many figures.

P16L17: here the authors state that “ET fluxes do not usually withdraw water from a well-mixed pool”. But does this mean that the pool is not well-mixed or that ET does not withdraw water as in a well-mixed system? I think Figure 7 clearly shows that the soil water storage is not a well-mixed pool, but the problem of how the fluxes draw water out of the available soil storage is a separate problem that I think is not specifically addressed by the authors.

P17L1: is rooting depth the only difference between the two sites at Bruntland Burn? Is it possible that the different E and T fluxes could also play a difference between the two sites?

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