

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

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Response to Referee 3

General Comments:

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

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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.

Response: We thank the anonymous Referee 3 for taking the time to review our manuscript and for their generally positive feedback on our study.

Main Comments:

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 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.

Response: We will include in the revised version that the previous study was not limited to one depth, but we compared the observations and simulations for 10 and 20 cm soil depth: “Our previous study showed that such a conceptualization of the subsurface with two pore domains that exchange water in accordance to Ingraham and Criss (1993) via the soil gas phase improved the simulation of the soil water

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stable isotopic composition at 10 and 20 cm depth at the investigated sites compared to an assumption of uniform flow.” Further, we will consider rephrasing some parts as suggested by Referee 3. For their given example we will change to: “Such an influence of vegetation on travel times as suggested from the plot scale simulations is commonly not seen for the catchment runoff as the stream integrates...”. We prefer to keep the critical discussion of our results in the dedicated section “Limitations and outlook”, rather than splitting this section and discussing the limitations already at the beginning of 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.

Response: We hope that we can clarify the open questions with the following changes: In the method’s section to clarify the exchange via vapor phase and the impact on the water ages: “Ingraham and Criss (1993) found that two water pools approach as a function of water volumes, surface area and saturated vapor pressure (temperature) a weighted average isotopic composition of the two pools. Our previous study showed that a conceptualization of the subsurface with two pore domains that exchange water in accordance to Ingraham and Criss (1993) via the soil gas phase improved the simulation of the soil water stable isotopic composition at 10 and 20 cm depth at the investigated sites compared to an assumption of uniform flow. Therefore, we apply the same model set up of SWIS as presented in detail by Sprenger et al. (2018b) with the parameters given in Table 1. In accordance to Vanderborcht and Vereecken (2007), we set the dispersivity parameter to 10 cm at all sites. The soil physical parameters were the same for the two pore domains and the exchange was solely conceptualized

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as vapour exchange not via hydraulic dispersion. The implemented tracer exchange between the slow and the fast flow domain results in a slow approach of the virtual tracer concentrations in the two pore domains. Thus, the exchange leads towards a homogenization of water ages between the two flow domains. In line with soil physics principles, the slow flow domain is filled first and remains saturated until the fast flow domain is emptied (Hutson and Wagenet, 1995). Water flow and tracer transport occurs in both domains and recharge is generated accordingly. However, only the average recharge flux rate and weighted average tracer concentrations from both domains are provided.”

To clarify the interception module: “Precipitation was divided into interception and throughfall according to the canopy coverage (Table 1), and when the interception capacity (Table 1) was reached, the surplus infiltrated into the soil.“

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 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.

Response: We agree with the Referee and will revisit the use of the term “inverse storage effect”. We will make clear that we refer here to the increased mobilization of water in the fast flow domain during high storage that contributed to the recharge water. However, for the E and T fluxes, flow path changes will not affect the flux ages. For these fluxes, the reduced water age in the flux stems from the increased share of young water at high storage. We will clarify these differences in the discussion,

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while simplifying the discussion as requested below. For example we will include: “In addition to the general positive relationship between wetness and soil hydraulic conductivity (van Genuchten 1980), the conceptualization with two pore domains in the SWIS model allowed young water to bypass in the fast flow domain older water stored in the slow flow domain. Since the smaller pores of the slow flow domain will be filled first or stay filled while the bigger pores of the fast flow domain are not empty, the bypass will be enhanced during periods of high wetness.”

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).

Response: We agree that the discussion should be simplified and will replace the part on the basic soil hydraulic relationships with the following: “In addition to the general positive relationship between wetness and soil hydraulic conductivity (van Genuchten, 1980), the conceptualization with two pore domains in the SWIS model allowed young water to bypass in the fast flow domain older water stored in the slow flow domain. Since the smaller pores of the slow flow domain will be filled first or stay filled while the bigger pores of the fast flow domain are not empty, the bypass will be enhanced during periods of high wetness.”

We will further delete two more sentences in the paragraph. However, we do not

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agree that the section repeats already published discussion from Sprenger et al (2016, RoG), as we did not discuss the two-pore domain back than, but limited our modelling and discussion to a conceptualization with a uniform flow domain. Thus, we believe that this paragraph is quite relevant for the current discussion. We will shorten the given lines that the Referee criticized as follows: “Additionally, snowmelt led to a sharp decrease in soil water ages after a continuous aging of the water that resided in soil over the snow accumulation (Figure 5).” And we will delete and shorten the other parts the Referee referred to.

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)

Response: We will add the references as suggested

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

Response: Agreed, we will take the reference out

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).

Response: Even if the reader will not go through the method’s section, they will still read at the beginning of each section that we talk about median travel times. We further wrote under each plot and table that we present median values. We believe therefore that we can keep the abbreviation as it 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

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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).

Response: We will add: “We decided to present median values, rather than mean travel times, as the latter can be biased due to uncertainties in the long tails of the transit time distributions (Seeger and Weiler, 2014).”

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.

Response: The recovery will be <100% if the tracer is not fully flushed out of the soil storage. We will change the sentence as follows for clarification: “Since MTT would be underestimated if the cumulative normalized breakthrough curve of the virtual tracers would not reach unity (tracer must have entirely left soil storage), we limited the MTT analysis to the period from 2012-2015.”

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.

Response: Yes, we refer here to the distribution of the time variant median values and will change the sentence as follows: “Distributions of the time-variant median travel times and median water ages in fluxes and storages were derived using cosine kernel density estimations. . .”

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.

Response: As outlined above, ET fluxes from the slow flow domain are limited to peri-

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ods when the fast flow domain is empty and the model output is limited to the average values for the recharge flux of both domains. However, we will add in the supplementary Figure S 4 a graph showing the soil water storage of the two flow domains.

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.

Response: Thanks, this is a very good point and we will address this in the discussion of the revised manuscript.

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?

Response: The differences between the forested and heather site at Bruntland Burn are not limited to the rooting depth. In addition to the rooting depth, also canopy cover and the interception storage (will be shown in a new table in the revised manuscript) affecting the infiltration volumes and the E-T partitioning, are different. We will add this also in the discussion as follows: “. . .our experimental set up with two different vegetation types (differing in T rates, rooting depth, canopy cover and interception storage) on similar soil types under the same climatic forcing in the Bruntland Burn. . .”

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