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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.

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Response to Referee 2

General Comments:

This well written and structured article describes an interesting soil physical based modelling study on water travel times and water ages at four different sites in northern latitudes. The model simulations were done for an extensive period (multiple years) giving insights in both short-term and seasonal dynamics. The topic is in my opinion of interest to HESS readers and after minor revision suitable for publication. Below are

my suggestions and comments for improvement of the paper.
Response: We thank the anonymous Referee 2 for taking the time to review our manuscript and for their generally positive feedback on our study.

Major Comments:

1. The description of the data should be more extensive (Methods section 2.2 and 2.3). The soil hydraulic parameters used for the modelling are not mentioned in the text/table. While a reference is made to Spenger et al. 2018, having this information available in this paper really helps with the interpretation (how different are the sites for example) without having to refer to Spenger et al. 2018. Furthermore, I suggest to include also other parameter values like maximum canopy storage, infiltration capacity (if applicable or a statement that overland flow does not occur). With respect to infiltration capacity; what about frozen soils at these sites?
Response: We will add a new table listing the applied model parameters: Depths of the soil horizons, Mualem – van Genuchten parameters (θ_s: saturated water content, α: air entry value, n: shape parameter), saturated hydraulic conductivity K, interception capacity and canopy coverage. The infiltration capacity results from the soil hydraulic parameters. We will add the following to address soil frost: "Soil frost does usually not occur at Bruntland Burn and is rare at the Dorset site due to the insolating effect of the snow cover. At Krycklan, soil frost was shown to not induce surface runoff but soils at the forested site remained permeable (Stähli et al., 2001; Laudon et al., 2007).
Finally, I recommend to add some more info about the model (run), like: - What was the parameter set (it is mentioned in the paper, but not specified)? - Was there a spin up period? - What was the internal time step of the model (I guess it was forced with daily throughfall and evapotranspiration)? - Programming language, open source?
Response: We will provide the info on the temporal coverage in the first paragraph the last subsection in the methods. We will add the following sentence to include the info

on time steps and parameters (now listed in a table): "The model was run on daily resolution and the applied parameters are listed in Table 1." The SWIS model is written in Python code.

2. In section 2.4 it is not very clear to me how MTT and water ages were derived exactly. In lines 3-4 "Tracer concentrations: : :Figure 1 left)." it is mentioned that tracer concentrations in the output fluxes were normalized by the infiltrated tracer mass (Ij(t)). Do you mean that the mass flux (of R, T and E) was normalized by the total mass recovered at these boundaries (of R, T and E)? If so, this could be stated more clearly in my opinion (as equation?). Now it seems the normalization was done by the total infiltrated tracer mass on tracer concentrations Oj. This also applies to the description of the calculation of water ages. In lines 8-10 please state more clearly why MTT analysis was limited to the period 2012-2015.

Response: The tracer concentration of each flux was normalized by the total infiltrated tracer mass to get relative concentrations of each traced flow path (E, T, R). Unfortunately, we cannot follow how the Referee gets to the interpretation that we would have referred the concentrations in the flux to the total recovered mass flux in the corresponding flux. We do not see where we potentially provide unclear description with that regard as also the Referee seem have understood the procedure given the second part of their comment. Anyways, we will change the sentence for clarification: "Tracer concentrations $O_j(t)$ in the output fluxes for each day after introduction of the virtual tracer Ij at time t0 were normalized by the infiltrated tracer mass of the tracked precipitation or snowmelt event (O_j (t)/I_j (t0), Figure 1 left)." To clarify that the tracer needs have entirely left the soil storage to calculate MTTs, we will change the sentence as follows: "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."

3. What controls travel times and water ages in the Discussion (and Results) could be expanded to soil hydraulic parameters, for example what about differences in saturated

hydraulic conductivity or saturated water content between these four sites? I strongly recommend to include these soil parameters (and advection dispersion parameters) in the analysis, since the focus of the paper is on soil physical based modelling.

Response: We will add a table with the soil physical parameters and we will add the following sentence in the method's section: "In accordance to Vanderborght and Vereecken (2007), we set the dispersivity parameter to 10 cm at all sites.

4. The SWIS model solves the Richards and advection dispersion equation, and the same set of water flow and transport parameters are used for the slow and fast domain. What about possible preferential flow/ macro-pore flow at these sites, when the Richards and advection dispersion equation are probably not applicable? I recommend that the authors elaborate on this in the Discussion section.

Response: We will add to the discussion: "The applied model approach cannot account for preferential flow, but the conceptualization of two pore domains with different water flow and transport dynamics enabled simulating bypass flow. This conceptualization was shown to be superior to a conceptualization of a uniform flow (Sprenger et al., 2018b). Additional inclusion of preferential flow into the model domain would come on the cost of model complexity and pose problems of parameter identifiability."

Minor comments:

1. First sentence in the Abstract, please rewrite the part "undergo intense respond"

Response: We changed the sentence as follows: "As northern environments undergo intense changes. . ."

2. My suggestion is to move the Study sites description (2.1) from the Methods section, to a new section.

Response: We will move "Study sites" to a new section.

3. There seems to be a lot of overlap in the dots of Figure 2, 3 and 6. Is there a way to

avoid this; different markers, make some transparent?

Response: We will increase the transparency and make the points smaller to reduce the overlap.

4. Discussion, line 4: please use references instead of “(introduced in section 1)”.

Response: Will be changed it as suggested

5. Section 4.4 lines 2-3, what about the often observed exponential decay of root distribution with depth?

Response: We assume that the Referee means that the root distribution is follows an exponential distribution. We will add the following sentence: “An exponential distribution would not change much regarding the water uptake patterns, as already the linear assumption results in 96% of the water being taken up in the upper 15 cm.”

6. Section 4.2 line 9; “Due to exchange between fast and slow flow domains: : :”, it would be good to mention in the paper on what time scale this exchange works/ how fast is this process?

Response: We will include more info on the exchange via gas phase in the methods section as follows: “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 Vanderborght 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 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."*

7. The following publication may be of interest irt the work described in this manuscript: van Verseveld, W. J., Barnard, H. R., Graham, C. B., McDonnell, J. J., Brooks, J. R., and Weiler, M.: A sprinkling experiment to quantify celerity–velocity differences at the hillslope scale, Hydrol. Earth Syst. Sci., 21, 5891-5910, https://doi.org/10.5194/hess-21-5891-2017, 2017.

Response: Thanks for the suggestion, we will have a close look at the given reference and see if we can include it into the discussion

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