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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Upscaling land-use effects on water partitioning and water ages using tracer-aided ecohydrological models" *by* Aaron A. Smith et al.

Anonymous Referee #2

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Summary:

This paper evaluates the performance of an ecohydrological model equipped with water isotope module (EcH2O-iso)in simulating soil water, evapotranspiration, recharge or runoff, groundwater, and water ages across different spatial scales (250, 500, 750, and 1000m). Moreover, the study addresses three main research questions that will be revealed using the model, applied in an intensively monitored catchment in northeast Germany. They conclude that the model with a coarser resolution (1000m) was unable to replicate the observed streamflow and distributed isotope dynamics and simulates higher evapotranspiration, lower relative transpiration, increased overland flow, and

slower groundwater movement. In addition, they also conclude that water isotopes provide effective calibration constraints for larger resolution model and help understand the influence of grid-resolution on the simulation of vegetation-soil interactions.

Assessment:

The topic of the paper fits well in HESS and is of great interest for the HESS readers. The findings are also interesting for readers working on the ecohydrological model development, land-surface fluxes quantification, and the use of water isotopes in hydrological applications. The paper at the present state still needs some revisions. The authors need to elaborate more on the method section and the conclusion does not summarize all three research questions brought up in this paper. I provide my general and detailed comments below and would ask the authors to take these comments into consideration as they revise the paper.

General comment: L refers to Line and P refers to Page.

1. In my opinion, the title is not mirroring the aim of this paper. The title is: upscaling land-use effects on water partitioning and water ages using tracer-aided ecohydrological model (L1) while the aim of this paper is to explore the changes in the skill of an ecohydrological model in capturing flux, storage, and mixing dynamics across spatial scales (L65). When I read the paper thoughtfully, the paper discusses the performance of the model to simulate the water fluxes and ages across different spatial scales. Therefore, I suggest to change the title.

2. The study tries to answer three research questions (L74-80) and they are explained in the discussion section. For the conclusion section, however, a summary of the answers for these research questions is not clearly presented. I could not find a conclusion for research question two or maybe I miss it.

3. The EcH2O-iso model consists of three main parts, which are the energy balance model, the water balance model, and the water isotope module. I understand that the

idea of the paper is not to discuss model development in a detailed manner as it was discussed in the previous studies (e.g., Maneta and Silverman, 2013 and Kuppel et al., 2018). However, there are not so many hydrological models equipped with water isotope module compared to the climate model. I expected that the authors would provide more information about how isotopic mixing and fractionation are performed in the model. Please elaborate more about the mixing and fractionation in all water components, such as in precipitation, transpiration, soil, storage, and GW. Also what methods do the authors use to calculate isotopic fractionation in different fluxes such e.g. the Craig-Gordon method, Keeling plot, or a steady-state method introduced by Dongmann et al. (1974) to calculate the isotopic composition of leaf water? How to define the isotopic composition of river water since it is mixing between groundwater, precipitation, and river from the upstream. When river water flowing in the channel, does it undergo evaporation fractionation, or neglected?

4. In the discussion section, the authors found that for forests, the evapotranspiration is higher, the recharge is lower, and the transpiration fraction is lower compared to croplands (L407-409). They argue that the lower Tr ratio is due to the interception. Could the authors elaborate more on why and what are the possible reasons? Also please provide all the evapotranspiration fractions here (soil evaporation, transpiration, and interception for crops and forest). I found in many studies that taller plants transpire more water than shorter plants (e.g., Oaks vs. wheat, Xu et al., 2008; Zhang et al., 2011, cotton field vs. corn and soybean, Kool et al., 2014). For some field measurements, the transpiration fraction for forests is 65-76% and 60% for grass (Choudhury and DiGirolamo, 1998; Blyth and Harding, 2011).

Line by line comment:

L1: for the abstract, please see the general comment.

L15: is it not better if the authors write “different scales” instead of only “scale”?

L65: here the readers can see the aim of the study, which differs from the title (upscal-

ing land-use effects instead of evaluating the model skill in simulating different water fluxes across different spatial scales).

L76-77: the second question is not clearly answered in the conclusion.

L86-87: the authors may rephrase the sentence into: “The 66 km2 Demnitzer Millcreek Catchment (DMC) located 55 km east of Berlin (52°23’N, 14°14’E) is a mesoscale catchment that receives 575 mm of precipitation annually”

L87: the authors may change the words: “(372 to 776 mm/year)” into “from 372 to 776 mm/year”.

L88: I think the general term is “convective” storms and not convectional storms.

L94: missing comma: . . .unavailable, these were. . .. . .

L95: first, ERA 5 is not a remote sensing dataset. It is re-analysis product that is fussed with all types of observations (not only remote sensing). Second, it is more common and general to use the word “sensing” instead of “sensed”. I cannot provide a good argument about it but you may look at: https://www.researchgate.net/post/Remote-sensing-data-or-images-vs-remotely-sensed-data-or-images-Are-they-both-OK

L131: the authors may revise the text into: “. . .. . .during periods of streamflow measurements”. For the next sentence, the authors may revise the sentence to avoid the use of the words “periods of streamflow” two times.

L132: the authors may revise the text into: “Evaporation was prevented by applying a layer of. . .. . .”

L134: missing “were” analysed. What is direct-equilibrium method?

L135: the authors may replace the word “with” with “using”

L141: what is “cf”?

L145: what kind of components? It is an unclear word.

L146: I am just wondering if the model was designed to be forced with RCM only or can it be forced with gridded in situ observations? This study also used in situ observations and not RCM.

L149: I suggest to replace ";" with full stop "."

L172: Could the authors indicate layers 1 to 3 in Fig. S1? It will help the readers to discriminate layers.

L197: missing space.

L200: please provide a reference for the n values.

L210: they are locations and not climate zones. Also please provide the names for these five locations in the main text.

L221: How about streamflow isotopic composition?

L224: I am wondering why the use of NSE and NRMSE is inconsistent. Some variables were evaluated using NSE and some using NRMSE.

L247: I am wondering what are the variable constraints do the authors use to calibrate the model? Is it only discharge or the authors consider all variables such as GW, SM.

L260: Fig. S3 is for validation and not for calibration.

L269: this is correct: ERA5 reanalysis products.

L269-271: I am a bit confused here. How do the authors calibrate the isotopes?

L276: the authors may replace the word "very few parameters" with "a few parameters"

L277: Do I miss supplementary material B? I could not find it.

L279: two commas there are not needed.

L284: How about streamflow?

L285-287: here the authors mention soil moisture in layers 1 and 2, however, Figure 2c does not distinguish between layers 1 and 2. Where can I look at the results?

L295: I suggest to add the word "that" in between processes and were.

L311: I am wondering why the Ei is somehow twice as higher as soil evaporation and half of Tr. If I look at the land cover map (Fig. 1a), I see the land cover, in general, can be divided into half forest and half arable land. The Ei is indeed higher for the forest but it cannot exceed the TR, and EI is very low or even insignificant for arable land. What are the reasons? Do the authors think this is the general problem (underestimation of Tr) found in many models as it was discussed by Sutanto et al., 2014?

L316: If the discharge is discussed first, why do not the authors swap the Figure between e and f with a and b? Hence, Figure 3a and B will be for streamflow results.

L324: the authors may revise the sentence into ".....flow events that are not present....."

L329: It is not the correct sub-title.

L330: here the authors only mention Tr. How about the Es and Ei?

L336: I am just wondering why the authors do not use the blue color for positive and red color for negative correlation (inverse colors)? Usually red represents low value and blue represent high value.

L337: please provide the value for the fraction of Tr.

L343: from 38 mm/year to 22 mm/year is not a slight decrease, it is almost half.

L39-350: the authors claim that the decrease in annual recharge is largely linked to ET, which is mainly from high Ei. However, I cannot see the Ei results in the suggested Figures (Fig.1, Fig.4, and Fig. S5) or even in any figure.

L363: Again, here I could not see Supplementary Material B.

L370: Can the authors explain why transpiration age is longer than soil in layers 1 and 2 and in GW.

L371: I am a bit confused here. I see that it is longer and not lower. e.g. at 250 model resolution, it is 47 days while at 1000 model resolution it is 28 days.

L381: Remove Figure S5. I do not see the results in Fig. S5. Fig. S5 is for the next sentence.

L384-385: here the authors discuss the GW age, however, I could not see the results. Is it in Figure S5? Do the authors mean GW age as L3? I only see GW storage.

L393-395: I could not see the results for stream water age of 0.5 and 1.8 years during large events in Table 6.

L402: In my opinion, it is not minor variability.

L407-409: see my general comment point 4.

L467: it is “are” and not “is”

L482: remove the second is in between coupling and with.

L486: the authors may revise the sentence into: “. . .. . .scales, long-term and multi-scale data collection are. . .. . .”

L489-490: the authors may revise the text in the brackets as (e.g. spatial resolution for isotopes and temporal resolution for sapflow).

L498: in my opinion, I will limit to 500 m maximum due to the highest uncertainty in model results above 500 m (Figure 3, 5).

L500: Figures 6e-h are not the correct figures.

L516: the authors may remove the words “have been identified”

L526-527: missing “that”: . . .. . .fluxes that are additionally expected to change is not

well known.

P26: Figure 3. Swap a, b for discharge first. To increase figure readability, the map can be removed since it is already available in Fig. 1a.

P28: Figure 5. I could not see the measured SM (dashed line).

P31: Table 1. Suggestion: please indicate in the table which one is obtained from ERA5 and which one is from MODIS. e.g. instead of location N/A, why not write ERA5 or MODIS? Also please keep in mind ERA 5 is not categorized as remote sensing data.

P32: missing Table 2 borderline.

P34: Table 4. What is negative Loglikelihood? It is not explained in the text.

P35: Table 5. Please mention that Es, Ei, and Tr were partitioned from ET.

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approach, J. Hydrol., 408, 203– 211, 2011.

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