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Interactive comment on "Deforestation reduces the vegetation-accessible water storage in the unsaturated soil and affects catchment travel time distributions and young water fractions" *by* Markus Hrachowitz et al.

Anonymous Referee #2

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The manuscript by Hrachowitz et al investigates the effects of deforestation on several variables of ecohydrologic interest, such as the rate of transpiration, the partitioning between streamflow and evapotranspiration, the water storage available for plant uptake and the residence times of water in the landscape. To do so, the authors use data from a highly-instrumented small watershed in Germany (the Wustebach experimental catchment, 0.39 km2) that was subjected to a permanent deforestation of 21% of its surface. The main strategy used by the authors was to develop a parsimonious catchment-scale hydrologic and transport model, calibrated on observations, and use

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it to quantify post-deforestation changes. The specific goals of the work, as stated in the introduction, are to analyse the observed post-deforestation changes through the lenses of change in the plant-available storage. The latter is a topic where the leading author has developed extensive research in the last years. Based on this main goal, the authors formulate three hypotheses that form the basis for the results and discussion.

The goals of the manuscript fully fit the scope of the journal, the data is of high quality and the methodology appears elaborate and generally appropriate. The authors produce a very large number of results, which may even be enough material for two papers, and I think this is where the manuscript becomes difficult to navigate. Although the overarching questions are clearly outlined in the introduction, the focus of the paper gets somewhat lost across the numerous results and analyses. For example (and see additional comments below), I find the water age analysis too detailed and beyond the main point of the article. I believe the focus of the manuscript should be narrowed and the material more selected to provide a cleaner storyline. Overall, while I believe these results deserve publication, I think major improvements on the manuscript structure and focus are required. I list additional comments below.

Major points

Novelty and relevance: I think the paper introduction is well developed but I somewhat miss the novelty and relevance of the results. E.g. what did we not know before? Does novelty lie specifically in the quantitative (rather than qualitative) evaluation of change?

Visual assessment of model results: Figures are all high-quality but they are not always informative. Figure 2 is particularly important because it shows timeseries of modelled and observed variables, but it is rather ineffective and it should be redesigned (see detailed comments below).

Model purpose: the model produces a large number of outputs and I appreciate that the authors clearly discuss the limitation of the modelling framework. I think it would be useful to declare upfront the capabilities of such a model. For example, given its design and the data used to assess its performance, what can the model be reasonably used for in this context? Also it seems that the model would in principle be able to estimate the age of evaporative fluxes, but the authors do not show such results. Why? And why is the model not used to estimate the (change in) partitioning between Q and ET?

Estimates of SU,max: the reduction in SU,max after deforestation is very pronounced. I'd appreciate some discussion of how a 21% reduction of forest cover may lead to a 50% reduction in SU,max. Does the location (riparian area VS hillslope) play a role? Would you expect important differences if the deforestation had occurred away from the river network? Could this model be used to make such a prediction?

Age analysis: as mentioned before, I think this is a very complete analysis but it seems to go too deep compared to the scope of the paper. There are several concepts and results that appear complex (e.g. not just the TTD/RTD ratio or the young water fraction, but also their sensitivities to wetness, before and after deforestation) and would fit a paper that specifically focuses on the effects of deforestation on catchment residence times. But given the broader objectives, a selection of the results might help clarify the message.

Detailed comments

Title: isn't it obvious that deforestation "affects" travel times? Can suggest how it affects them

50-56: very long sentence. Consider breaking it

70-71: unclear sentence

93-96: this sentence includes some slang. Consider rephrasing. Also, it partly seems a repetition of lines 49-52

98: "With increasing storage, the hydrological memory of a system increases" → can be easily misinterpreted because one may think that, for a system with a given water storage, streamflow age would increase when the storage increases, but it is usually found (e.g. Harman, 2015, Water Resources Research) that the opposite applies.

Figure 2: this figure is very important for the manuscript but I find it rather inefficient. Subplot (a) does not seem to show anything that a reader could grasp. Subplot (c) is very compressed –it is never easy to compress 7 years of data into a single panel– but then it is almost impossible to inspect the results. Subplot (d) is uninformative because tracer in streamwater is not shown at an adequate scale. Is tracer precipitation data really useful here? If you really want to show it, maybe you could report some monthly means (so we get at least the seasonality of the input) and on a rescaled y-axis? Can you also add a legend directly into the plot to facilitate understanding of what the plotted variables are?

162: "To quantify effects of deforestation on SU,max and, as a consequence of that, on the age structure of water" I find this slightly misleading as it seems that the age structure is only affected through SU,max (and not directly).

166: similar to 162, I don't see why you estimate the effects of these changes only. Aren't the two problems partially separate? You can get to (2) even without (1) or am I uncorrect?

Eq 39 and 41: what is the horizontal bar above the lowercase omega?

336: this explains how you translate a physical process into a model component. But I recommend explaining why a "passive" storage is needed. It is not a model artefact, but rather the real amount of water that is involved in the transport process. Other good classic references for this is "Birkel, C., C. Soulsby, and D. Tetzlaff (2011), Modelling catchment-scale water storage dynamics: Reconciling dynamic storage with tracer- inferred passive storage, Hydrol. Processes, 25(25), 3924–3936, doi:10.1002/hyp.8201"

353: Ok fine, but how do you initialize the model? Which d18O initial composition is assigned to the different compartments?

385: I suggest replacing “the data” with “results”

408: start with “In our study, ”

417: SU,max = 90+-149? If the standard deviation is much larger than the mean, wouldn’t it be better to avoid completely this exercise? Or maybe mention it here but not stress it later as a real result, given it comes with considerable uncertainty?

428 and 435: as noted in comments on Figure 2, it is difficult to judge from this figure because too much data is compressed in it and the scaling does not look appropriate

Figure 5: please add some legend to make the colouring more intuitive

Figure 6: why showing cumulative frequencies? I find it difficult to evaluate how much parameters are constrained from these curves.

440: “. . .show that most model parameters are reasonably well identified” but it is difficult to actually evaluate

494: if I haven’t stressed this enough, this is invisible in the figure

513: I do not see how a value of 120 falls “reasonably well” into a “plausible” range [-59, 239]

519: I get the point, but cannot really evaluate overlapping from figure 6. Rather from table 2. Then I see that Lp is the only other parameter with a significant change pre-post deforestation. Might this be worth of a comment?

534: [again on figures] “is evident”, but nothing is evident from figure 2d

541-566: this is a nice analysis but I wonder whether it is really necessary as it is more about general TTD and RTD dynamics rather than on effects of deforestation. In other words, this seems to go beyond the scope of the manuscript. A large simplification would make the manuscript easier to read.

544: please specify how you computed Fyw because a reader will expect it is estimated using kirchner's 2016 method.

33: the change in Fyw from 0.11 to 0.13 does not seem significant considering the possible uncertainties in the estimate.

Typos and language

Figure 2d: typo in the permil symbol in the y-axis label.

70: the vegetation. . . presentS

264: it is reaches

548: "Stream water can contain Fyw" is not a correct formulation. Rather "stream water can contain up to 30% of young water" or similar

532-534: please reformulate this initial sentence, which is unclear The terminology "partly much lower" (452) and "partly considerable" (572) is a bit odd

425: title: Deforestation effects "on the catchment model" sounds a bit odd.

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