Comment:

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, 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.

Reply:

We thank the reviewer for her/his interest in our work. We highly appreciate the thought- and insightful comments and the overall positive assessment of our manuscript. We will address all comments in detail below.

Comment:

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?

Reply:

We agree, as also pointed out to Reviewer #1, that the research objectives and in particular the novel aspects of our analysis were not defined clearly enough in the original manuscript.

*Briefly, previous analyses in the study catchment documented changes in the individual water balance components (i.e. evaporation and discharge, Wiekenkamp et al., 2016, 2020) and also fluctuations in young water fractions (Stockinger et al., 2019) in the years after deforestation. In contrast, the aim and novelty of our study here was to establish a link between these observed changes in the water balance components and deforestation-induced changes in (sub-)surface system properties (and thus model parameters) and to explore and quantify possible mechanistic processes that cause these changes, eventually providing a possible explanation of **why** these changes occurred.*

*Our results provide evidence that changes in $S_{U,max}$ (and to a minor degree, in I_{max}), a hydrologically relevant and directly quantifiable subsurface property/model parameter, can explain much of **why** the hydrological response as well as travel times and young water fractions changed after deforestation.*

We will clarify this in the revised manuscript.

Comment:

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

Reply:

We agree that Figure 2 can be improved. We will adapt as described in detail below.

Comment:

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 good 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?

Reply:

We agree. We will provide some more information on that in the model description section. Briefly, the model used is a current-generation catchment scale, process-based, semi-distributed model based on conceptual parametrizations and which is coupled with a tracer tracking module formulated based on the SAS-function concept. As such it is valuable to quantify catchment-scale water balance and tracer dynamics. In particular and most importantly, instead of merely aggregating largely unknown surface and subsurface heterogeneities, it allows to account for their integrated effects on the hydrological response. The main limitation of such a model approach is missing spatial detail and the use of catchment-scale effective parameter that sometimes cannot be well constrained by the model calibration process.

Comment:

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?

Reply:

The reviewer is right in assuming that the model also provides age estimates of evaporative fluxes (and any other system internal flux). In a deliberate decision we chose not to use these model outputs in the analysis as these outputs do not have any direct data support and are thus subject to considerable uncertainty. While stream water age estimates are of course also subject to uncertainties, they are more reliably and directly constrained by stream water tracer samples. We will clarify this in the revised manuscript.

The model is in fact already used to estimate the partitioning, although not directly between Q and ET, but instead (in fact containing the equivalent information to Q/E_A) expressed as the runoff ratio C_R (i.e. Q/P or $(1-E_A)/P$). The runoff ratio is also one of the calibration objectives in our model implementation (i.e. $E_{R,CR}$) as shown in Table 3 and Figure 5 and discussed at several points in the original manuscript (e.g. l.454-456 or l.488-490).

In addition, we believe that the analysis is already quite comprehensive and the manuscript rather long, as also remarked on above by the reviewer. Additional information will make it difficult to keep the manuscript focussed.

Comment:

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?

Reply:

This is a very interesting observation. We are similarly surprised by the strong effect and also remarked on that in the original manuscript (l.421). One statement we can make here, and which is supported by data, is that the model results suggest that the reduction of $S_{U,max}$ in the completely clear-cut riparian zone was considerably higher than on the only partially deforested hillslopes (see Table 2, Figure 6 and l.509-511). While this indicative ranking and also the strong reduction of $S_{U,max}$ in the clear-cut riparian zone are plausible (~ 130 mm; l.511), the ~ 75 mm reduction in the only partially deforested hillslope zone (l.510) indeed generates questions. Of course, it cannot be excluded that deforestation affects other properties in the subsurface and that these changes may feed back onto $S_{U,max}$ (as discussed in l.503-505 in the original manuscript). An alternative explanation could be that after deforestation in 2013, the remaining forest on the hillslopes, that was homogeneously planted in 1946 (Graf et al., 2014) was thinned in 2015, thereby likely reducing catchment-scale vegetation water demand and thus also $S_{U,max}$, as compared to the pre-deforestation period. We, however and unfortunately, do at this point not have the necessary data to further test and substantiate this hypothesis.

We will extend the discussion on this in the revised version of the manuscript.

Comment:

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.

Reply:

We agree, that a comprehensive selection of results is presented and that the manuscript may benefit from a bit less detail. We will therefore remove the results/discussion on the differences between TTD and RTD and try to shorten some of the other aspects.

Comment:

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

Reply:

*Probably it is indeed intuitively obvious. However, to our knowledge it has not yet been explicitly shown in a detailed analysis. To highlight the direction of the effect, we suggest to adapt the title to: "Reduction of vegetation-accessible water storage capacity after deforestation affects travel time distributions and **increases** young water fractions in a headwater catchment."*

Comment:

50-56: very long sentence. Consider breaking it

Reply:

Agreed. Sentence will be rephrased.

Comment:

70-71: unclear sentence

Reply:

Agreed. Sentence will be rephrased.

Comment:

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

Reply:

We are not sure what the reviewer is exactly referring to as "slang" in this sentence: "Transpiration extracting soil water below that therefore effectively generates a root-zone water storage reservoir between field capacity and permanent wilting point that is characterized by a storage capacity $S_{U,max}$, i.e. a maximum vegetation-accessible storage volume, and that is at any given moment filled with a specific water volume $S_U(t)$, depending on the past sequence of water inflow and release."

It is true that the sentence is partly a repetition, but we believe it is necessary to explicitly clarify the difference between $S_{U,max}$ and $S_U(t)$. We will try to rephrase the sentence.

Comment:

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.

Reply:

This is indeed a delicate problem, mainly due to the unfortunate and confusing terminology available to us. In fact the term "storage" can be used in ways that have subtle differences. Consider the following simplified, illustrative thought experiment, distinguishing 4 situations:

(1) a small storage reservoir that is half-filled with water and from which water can only slowly drain at the bottom. Water will stay and mix in this volume for some time (i.e. little storage leads to old ages that eventually leave the reservoir) before being drained.

(2) In a second step, this storage reservoir is filled and overtops. Much of the overtopping water has little opportunity to mix with older water in the reservoir and bypasses the storage volume (i.e. at such high storage states, much of the outflowing water will thus be very young)

*(3) a second storage reservoir that is **larger** than the one above is, just as above in (1), half-filled with water and from which water can only slowly drain at the bottom. Water will stay and mix in this volume for some time (i.e. little storage leads to old ages that eventually leave the reservoir). However, the difference to (1) is that although the degree of filling of the storage reservoir is the same, the actual storage volume is larger, which leads – under complete mixing to older ages than in (1), as reflected in basic equation for mean turnover times T=volume/flux.*

*(4) In a last step, this larger storage reservoir is filled and overtops. Much of the overtopping water has little opportunity to mix with older water in the reservoir and bypasses the storage volume (i.e. at such high storage states, much of the outflowing water will thus be very young). However, under most conditions there is still **some** exchange with the old water in the large storage reservoir. Although younger than in a half-filled large storage volume (3), the outflowing water may still be older than the outflowing water from the full small storage volume (2).*

In summary, while the use of the term storage in the Harman (2015) paper refers to the degree-of-filling of a storage reservoir with given size, i.e. (1) and (2), we use the term storage here to refer to the absolute size of a storage reservoir, i.e. (3) and (4).

We will clarify this in the revised manuscript.

Comment:

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 pane l– but then it is almost impossible to inspect the results. Subplot (d) is uninformative because tracer in stream water 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?

Reply:

We agree that efficiently showing the most relevant results over longer time periods for multiple variables in figures is challenging. We also agree that Figure 2 can benefit from some adaptions. We therefore propose the following changes.

Subplot (a): we will replace daily values by monthly values. This should make the plot easier to read.

Subplot (c): we think it is important to give the reader an overall impression of the hydrological response to be able to place it into sufficient context. We thus would like to keep subplot (c) as is, but to add either (1) an additional subplot, showing zoomed-in version of one individual year pre- and one year post-deforestation, or (2) detailed individual plots for each year in figures in the Supplementary Material. We will test and evaluate both alternatives.

Subplot (d): We had a long and intensive discussion on exactly this question and, in the end, made the deliberate decision in the original manuscript to show the tracer signals in the way we did. Of course the reviewer is right in saying that detailed, small scale fluctuations in stream water tracer cannot be *seen in this figure. The underlying question for us was, which information is in fact the most relevant to convey here. We finally decided that it is more relevant to illustrate the degree of damping that precipitation tracer signals experience before they reach the stream. This is a direct indicator of the age of stream water and can only be seen when both variables are plotted at the same scale. In the case of this study catchment, when the tracer signal is attenuated to a degree that the stream water composition plots almost as a straight line, little fluctuations around this line are difficult to interpret: how much of it is due to real effects? How much is due to mere noise, resulting from observational uncertainties? This is also discussed in some detail in the original manuscript (l.474-479). However, we also acknowledge that the reader may want to see exactly this detail. Thus we propose the same as for subplot (c) and we will add an additional subplot with a zoomed-in version for one individual year pre- and one year post-deforestation. In addition, we will follow the reviewer's excellent suggestion to integrate the precipitation signals to monthly time-scales for better readability of the figure.*

Comment:

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).

Reply:

Deforestation reduces the catchment-scale pore volume that can be accessed by roots, i.e. $S_{U,max}$. This also reduces $S_{U,max}$ as mixing volume. The size of a mixing volume directly affects water ages (see replies above). Changes in water age structure are thus a consequence of a reduction of this mixing volume. We will clarify this in the revised manuscript.

Comment:

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 uncorrect?

Reply:

*It is correct that quantifying changes in $S_{U,max}$ does not **require** an a priori check if the general water balance partitioning pattern changed. However, under relatively stable climatic conditions (as in the study catchment during the study period), changes in the partitioning are a reliable indicator of changes in some (a priori unknown and unquantified) system properties (and thus model parameters), frequently related to changes in vegetation cover (e.g. van der Velde et al., 2014; Jaramillo et al., 2018). If, in contrast, no such changes in the partitioning are observed, changes to system properties are much less likely.*

We therefore wanted to first establish that there actually was a significant change between the pre- and post-deforestation partitioning pattern detectable with our method (and not only with the method used in Wiekenkamp et al., 2016) and to use this as basis for the subsequent hypothesis that deforestation affects $S_{U,max}$ and that this effect can be quantified.

Comment:

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

Reply:

We agree, this looks confusing. This is not an overbar – the italic style of this MS Word font, when used in the equation editor only appears to have an overbar. It is in fact only a normal omega character.

Comment:

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"

Reply:

This is indeed an excellent suggestion. We will make this more explicit and add the above reference.

Comment:

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

Reply:

We initialized the model using different initial isotope compositions for the individual storage components: assuming that only groundwater flows from S_S sustain low flows during the dry season, we used the mean isotope stream water composition of the time steps with the 5% lowest flows as initial composition in S_S and $S_{S,p}$. For S_U we used the long-term volume weighted mean precipitation value of the months April-September, preceding the start of the model in October. S_I and S_F were, due to their small sizes, assumed to be empty. The model was then primed with a 5-year warm-up period before 10/2009, which was a copy of the data from actually available observations.

We will add this information in the revised manuscript.

Comment:

385: I suggest replacing "the data" with "results"

Reply:

Agreed.

Comment:

408: start with "In our study, "

Reply:

Agreed.

Comment:

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?

Reply:

We agree that these uncertainties are quite high. We still believe it is worth to report it, to give the reader a sense of a, albeit uncertain, plausible range. Due to the uncertainties, we do, on purpose, not use these values for any quantitative analysis. We have explicitly stressed and acknowledged that in the original manuscript (l.418-420). We will further clarify this in the revised manuscript.

Comment:

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

Reply:

We will adapt Figure 2 as described in our reply to one of the comments further above.

Comment:

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

Reply:

Agreed.

Comment:

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

Reply:

Ok. We will add histograms to also show the associated frequency distributions in these plots.

Comment:

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

Reply:

We will adapt Figure 6 as described above to make it easier to see.

Comment:

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

Reply:

Agreed. Figure 2 will be adapted as described above.

Comment:

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

Reply:

We are not sure what the reviewer exactly wants to express here. In our understanding, a value of 120 is very close to the central value (i.e. 149) of the interval spanned by [-59, 239] and thus also falls into this range.

Comment:

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?

Reply:

*We will adapt Figure 6 as described above. It is true the parameter L_P does change, but, and this seems to be a misunderstanding, it is by far **not** the only parameter with a significant change as shown in Table 2 in the original manuscript.*

The parameters that are directly related to vegetation ($S_{U,max}$, I_{max}) change the most and they do so in a more pronounced way in the fully deforested riparian zone, i.e. the range of $S_{Umax,R}$ is reduced from 194-287 mm to a range of 53-122 mm, while $I_{max,R}$ is reduced from 0.8-4.5 mm to 0.0-0.9 mm.

The reductions in the partially deforested hillslope parameters are less pronounced but still statistically significant ($p<0.05$) for $S_{U,max,H}$ from 233-309 mm to 118-249 mm and for $I_{maxh,H}$ from 0.8-4.5 to 0.1-1.7 mm.

Most other posterior parameter distributions (i.e. parameters not directly related to vegetation) do not experience a significant change (Table 2).

Comment:

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

Reply:

Agreed. Figure 2 will be adapted as described above.

Comment:

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.

Reply:

We will try to shorten this section. In particular, we will remove the analysis and interpretation of RTDs. However, we would really like to keep the mechanistic interpretation of the results, which is ultimately the part of the objectives of this paper.

Comment:

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

Reply:

The young water fraction was here defined as the fraction of water younger than three months. This value was here directly extracted from the associated travel time distributions (TTDs). We will clarify this in the revised manuscript.

Comment:

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

Reply:

This is correct. We will clarify this in the revised manuscript.

Comment:

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

Reply:

Ok.

Comment:

70: the vegetation...presentS

Reply:

This is a misunderstanding. The sentence should read as: "The vegetation, i.e. a collective of individual different plants within an area of interest that is present at any given moment at any given location, has survived."

Comment:

264: it is reaches

Reply:

Will be corrected.

Comment:

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

Reply:

Will be corrected.

Comment:

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

Reply:

Will be corrected.

Comment:

425: title: Deforestation effects “on the catchment model” sounds a bit odd

Reply:

Will be rephrased.

References:

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