Comment:

This manuscript presents a study on the effect of deforestation on catchment hydrology. In this manuscript mainly a modeling approach is used. Using this modeling approach, the authors find that runoff increases after deforestation and also catchment travel time distributions. While this is an interesting point, I have a number of concerns with the study as it is presented now.

Reply:

We appreciate the reviewer's interest in our work and her/his thoughtful comments. We provide detailed clarifications in the list below.

Comment:

While the study uses a valuable data set is not fully clear to me how exactly this paper goes beyond the studies that have already been published using this catchment and its data set. Conclusions 1 and 2 basically confirm earlier studies, and the other conclusions are based on modeling with a number of assumptions (as discussed be-low). In general, it would be important that the authors relate their findings more to the previous findings using the same catchment to show the added value of this study clearly.

Reply:

We agree with the reviewer that the objective and novelty of our study was not communicated in a clear enough way in the original manuscript.

Briefly, previous analyses in the study catchment documented changes in the individual water balance components (e.g. evaporation and discharge, Wiekenkamp et al., 2016) and also some minor fluctuations in young water fractions (e.g. Stockinger et al., 2019) in the years after deforestation. Yet, these contributions did not provide quantitative mechanistic explanations of ***why*** *these changes occurred.*

The aim and novelty of our study is therefore to explore and quantify a possible mechanistic process that causes these changes. Our results provide some evidence that changes in $S_{U,max}$, 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 make this clearer in the revised version and we will also more explicitly discuss our results with respect to the results of the above studies.

It is also true that Conclusion #1 in the original manuscript essentially only supports the results of a previous study (i.e. Wiekenkamp et al., 2016), albeit estimated with a different method and over a different time period. We will clarify this in the revised manuscript.

In contrast, we are not aware of any study in the Wüstebach that quantifies the effect of deforestation on $S_{U,max}$, i.e. Conclusion #2. Our results, however, provide a puzzle-piece of supporting evidence for the potential generality of $S_{U,max}$ reductions due to deforestation as they are consistent with results from catchments in different climatic and geomorphic settings (Nijzink et al., 2016; cited in the original manuscript).

Comment:

An obvious limitation of this study is in the use of only one catchment. As valuable as the data set is results might not be generally valid unless they can be confirmed using a larger data set including several catchments.

Reply:

We completely agree. However and unfortunately, we are not aware of any other catchment world-wide, where the necessary data are available and the necessary conditions for such a study are met.

Briefly, which minimum information is required to do such an analysis?

At least a few years, pre- and deforestation respectively, with (1) daily water balance observations (P, E_P, Q; which are without doubt available from a many catchments world-wide), (2) (bi-)weekly tracer composition in precipitation and stream water and (3) well ***documented deforestation*** *of a* ***significant fraction*** *of a catchment and that occurred during a sharply defined,* ***short period****, which falls* ***within the time period of available water balance and tracer data****.*

For example, while for the Plynlimon experimental catchment, long time series of water balance and tracer observations are available, deforestation occurred only gradually over many years (or decades), each time only affecting a small fraction of the catchment, and was partly offset by regrowth. In addition, much of the deforestation did temporally not overlap with the availability of tracer data. In another example, for the HJ Andrews experimental watershed, where well documented, significant deforestation took place over short, well-defined time periods, no tracer data are available for that period. Similar limitations apply to other locations, making such an analysis highly problematic.

As much as we wanted to provide a more generally valid analysis, we remain limited by the available data. We nevertheless strongly believe that in such a case, detailed analyses and process understanding developed from anecdotal case studies, such as this one, can be valuable to shape our understanding of what could happen and which processes occur – without claiming generality of the phenomenon.

We also agree that the title and the framing of the original manuscript may have misled the reader into hoping for a more general analysis. We will therefore 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." to emphasize the local character of this study. In addition, we will make this clearer in the introduction.

Comment:

The most obvious effect of deforestation on catchment hydrology obviously is the re-moved interception. Here the authors largely ignore interception by the use of so-called effective precipitation. It is important to note that effective precipitation was determined by modeling using a very simple approach with a constant interception storage. This means that the results might have been implicitly affected by the calculation of the effective precipitation.

Reply:

While we agree with the reviewer that interception plays a role in forest systems, we respectfully disagree with the statement that the "most obvious effects" of deforestation relate to interception. Leaving aside the fact that, of course, visually the effect is large, many studies world-wide demonstrate *that transpiration not only exceeds interception evaporation (e.g. Jasechko et al., 2013; Coenders-Gerrits et al., 2014; Schlesinger et al., 2014; Wei et al., 2017; Mianabadi et al., 2019), but that transpiration is actually the largest water flux from many terrestrial systems (Jasechko, 2018).*

The reviewer's notion that we "largely ignore interception" is probably a misunderstanding as we use a standard technique, successfully tested and used in current-generation catchment-scale model formulations to account for interception (e.g. Fenicia et al., 2008; Samaniego et al., 2010; Gao et al., 2014; Nijzink et al., 2016). It is true that we use an interception capacity I_{max} which defines the ***maximum*** *volume of water that can be stored in the interception storage reservoir $S_I(t)$ at each time step t before overflowing, as defined by equations 9, 17, 18 and 19 in the original manuscript. At each time step t, this reservoir fills (via precipitation P(t)), drains (via evaporation $E_I(t)$ and overflow $P_E(t)$) and stores water (whatever water volume does not overflow as P_E and cannot be evaporated as E_I within a time step is carried over as storage $S_I(t)$ to the next time step).*

It is thus important to note that ***not the interception storage $S_I(t)$****, i.e. the intercepted volume of water at each time step, is constant,* ***but the maximum volume of water I_{max}*** *that can be stored in the reservoir at each time step is constant. In other words, the degree of filling of the interception reservoir varies each day up to a maximum volume of I_{max}.*

The modelled fluxes related to interception evaporation are broadly consistent with plots-scale observations thereof: the model suggests a mean ratio P/P_E ~ 1.35 (5/95th interval: 1.19 – 1.51) for the growing season, while Stockinger et al. (2015) reported an observed P/P_E ~ 1.41 (± 0.19).

The underlying problem that prevents a more detailed formulation of this process is that, on larger scales, such as catchments, we have by far insufficient data (even in experimental catchment such as the Wüstebach) to warrant a meaningful, more detailed formulation of the interception process (and many other processes), as also described with an illustrative example in Hrachowitz and Clark (2017; see section S2 of the Supplementary Material therein).

The limited influence of interception evaporation on $S_{U,max}$ and the associated interpretations are explicitly described in lines 411-415 and shown in Figures 4b and 4c in the original manuscript.

Comment:

The isotope data is used to parameterize the passive storage volume. As I understand, this passive volume is only used for groundwater storage. This makes me wonder whether any passive storage is also being considered for the unsaturated storage. Sorry in case I missed this, but I am feeling a bit confused here. It is also not clear to me how mixing between active and passive storage has been calculated.

Reply:

We do not consider an explicit passive storage volume for the unsaturated zone. In reality, there may be such hydrologically passive volumes (i.e. dS/dt = 0) in the unsaturated zone, though.

The first one is the water stored in pores at water content below the permanent wilting point and thus essentially bound indefinitely, as it cannot be drained nor extracted by vegetation. The absolute magnitude of this volume is, depending on the wilting point, very small and thus considered to be negligible here and in similar studies (e.g. Birkel et al., 2010; Harman, 2015; Benettin et al., 2017 and many others).

The second storage compartment that could be seen as a hydrologically passive storage volume is the unsaturated zone ***below*** *and/or* ***outside*** *the root zone. In that zone, vegetation cannot extract significant volumes of water below field capacity. The pores therefore remain filled to field capacity much of the year, except for moments when a wetting front passes through. However, exchange (i.e. "mixing") with water in the "active" stores can and does occur. Indeed, we have previously added such a hydrologically passive mixing volume, representing the transition zone between the root zone and the groundwater, in a similar study (Hrachowitz et al., 2015). However, we found that the additional parameters cannot be reliably constrained with the available data, thereby increasing model uncertainty.*

In the present study, we also tested initial model formulations including such a store. However, it was found that it does not improve the results nor that it is meaningfully constrained by the available data. Here in the Wüstebach, the passive mixing volume in the unsaturated zone has an upper plausible limit of <1000 mm due to the position of the groundwater table (see also replies to Reviewer #1). In the way the model is implemented now, this additional mixing volume is implicit in $S_{S,p}$*. In other words, were we to introduce an individual passive unsaturated mixing volume of ~1000 mm,* $S_{S,p}$ *would be reduced by approximately the same value. Such a trade-off, however, does not influence any of the results, as water going through such an unsaturated passive volume, subsequently only passes through* S_S*. Thus, the two can be aggregated without introducing relevant uncertainties. The same implicit reasoning was applied in many successful previous tracer studies (e.g. Birkel et al., 2010; Harman, 2015; Benettin et al., 2017 and many others).*

The mixing between active and passive stores was done using a standard SAS-function: a technique that has been well described and successfully tested in a large body of literature cited in the original manuscript (e.g. Botter et al., 2011; van der Velde, 2010; Benettin et al., 2013, 2017; Harman, 2015; Rinaldo et al., 2015 and many others), but also reproduced in this manuscript in some detail (section 4.2.2 and in particular l.336-339). Briefly, the estimated outflow $Q_S(t)$ *from the active groundwater store* $S_{S,a}(t)$ *at each time step t is a composition of ages that is sampled (i.e. "mixed") according to the SAS-function from the* ***total*** *groundwater storage, i.e.* $S_{S,T}(t)=S_{S,a}(t)+S_{S,p}$*. We will further clarify this in the revised version of the manuscript.*

Comment:

Another crucial issue is the assumption that roots only take from the unsaturated zone. While this might be the case for the direct uptake, indirectly the uptake of water from the unsaturated zone will have the effect that an upward gradient is established which will cause groundwater to rise into the unsaturated zone. So, indirectly roots can access water from the saturated zone. This process seems to be ignored here.

Reply:

We agree that roots can create suction pressures that, together with pure capillary rise and upwelling groundwater, can result in upward fluxes. While the latter is explicitly accounted for in the model as flux $R_{S,R}$ *(Figure 3; Equations 11, 13 and 24; l.261-262), the first is implicit in how* $S_{U,max}$ *is estimated: it is the volume of water that was in the past accessible and accessed by vegetation. It therefore includes any upward capillary water fluxes.*

Comment:

Based on the points above, I would argue that the calibration parameter S_Umax is difficult to interpret. It sounds like it is the size of the unsaturated storage, but I would argue it is more a parameter describing catchment functioning.

Reply:

As clarified above (as well as explained in the original manuscript and the references therein), $S_{U,max}$ has indeed a clear and hydrologically meaningful definition. It is the maximum water volume that can be stored in the pores of the unsaturated root zone between field capacity and permanent wilting point at the catchment scale and, most importantly, which is ***accessible and accessed by roots*** *(sometimes also referred to as "available water" or "plant available water"). This vegetation accessible water volume at the catchment scale is therefore a meaningful measure for the catchment-scale effective influence zone of roots, implicitly accounting for spatial heterogeneity of soils as well as for the spatial heterogeneity in the depths and densities of root systems. It is also a subsurface property that regulates much of the hydrological response and which could be, provided we have suitable observation technology available at some time in the future, practically obtained by measuring* ***all*** *roots and* ***all*** *soil porosities in a catchment multiple times over a certain period. As it could be* ***directly measured****, albeit only theoretically at this point, it* ***represents also a real catchment-scale quantity*** *and not only some abstract number. Note, that $S_{U,max}$ does not describe the unsaturated zone outside/below the root-zone. Also note that $S_{U,max}$ is a conceptual volume (and as such it is a theoretically directly measurable quantity, as explained above) that does not necessarily match to a specific position in the soil. It is rather a catchment-integrated pore volume that aggregates all locations and depths where roots are present.*

Comment:

Overall, I am afraid that the results obtained in the study are both catchment- and model-dependent. The authors need to make a more convincing case why their results are generally valid.

Reply:

We agree, the results depend on the decision taken and choices made throughout the modelling process. This is the case for any modelling study. For this reason and to limit uncertainties we only report results from processes that could at least to some degree be confronted with data. For example, we estimated $S_{U,max}$ (1) from water balance data alone and (2) as model calibration parameter and then compared these values. In spite of uncertainties, they are broadly consistent with each other. Similarly, we only analysed stream water travel time distributions and not, for example, evaporation travel time distributions, as in the model only the stream water isotopic composition could be directly confronted with and compared to observed data. Furthermore, in an attempt to make the model as robust and reliable as possible, we constrained, evaluated and tested the model with an extended multi-objective and multi-variable calibration strategy, that goes far beyond of what many other modelling studies do. Lastly, we have also devoted the entire Section 5.4 to a detailed and honest discussion of the limitations of the modelling experiment, stressing that the results and interpretations are of course model-dependent (l.599). Given the above points, we nevertheless think that the results provide a rather reliable test of our research hypotheses. However, as with any other hypothesis, the test could be even stricter if the necessary data were available (Hrachowitz and Clark, 2017).

As discussed in one of the replies above, as much as we would like, the available data does not allow us to generalize the result. As also mentioned above, we will therefore emphasize the local character of the results more in the revised manuscript.

References:

Benettin, P., Soulsby, C., Birkel, C., Tetzlaff, D., Botter, G., & Rinaldo, A. (2017). Using SAS functions and high-resolution isotope data to unravel travel time distributions in headwater catchments. Water Resources Research, 53(3), 1864-1878.

Birkel, C., Tetzlaff, D., Dunn, S. M., & Soulsby, C. (2010). Towards a simple dynamic process conceptualization in rainfall–runoff models using multi-criteria calibration and tracers in temperate, upland catchments. Hydrological Processes: An International Journal, 24(3), 260-275.

Botter, G., Bertuzzo, E., & Rinaldo, A. (2011). Catchment residence and travel time distributions: The master equation. Geophysical Research Letters, 38(11).

Coenders-Gerrits, A. M. J., Van der Ent, R. J., Bogaard, T. A., Wang-Erlandsson, L., Hrachowitz, M., & Savenije, H. H. G. (2014). Uncertainties in transpiration estimates. Nature, 506(7487), E1-E2.

Fenicia, F., Savenije, H. H., Matgen, P., & Pfister, L. (2008). Understanding catchment behavior through stepwise model concept improvement. Water Resources Research, 44(1).

Harman, C. J. (2015). Time-variable transit time distributions and transport: Theory and application to storage-dependent transport of chloride in a watershed. Water Resources Research, 51(1), 1-30.

Hrachowitz, M., & Clark, M. P. (2017). HESS Opinions: The complementary merits of competing modelling philosophies in hydrology. Hydrol. Earth Syst. Sci, 21(8), 3953-3973.

Hrachowitz, M., Fovet, O., Ruiz, L., & Savenije, H. H. (2015). Transit time distributions, legacy contamination and variability in biogeochemical 1/fα scaling: how are hydrological response dynamics linked to water quality at the catchment scale?. Hydrological Processes, 29(25), 5241-5256.

Hrachowitz, M., Benettin, P., et al. (2016). Transit times—The link between hydrology and water quality at the catchment scale. Wiley Interdisciplinary Reviews: Water, 3(5), 629-657.

Jasechko, S. (2018). Plants turn on the tap. Nature Climate Change, 8(7), 562-563.

Jasechko, S., Sharp, Z. D., Gibson, J. J., Birks, S. J., Yi, Y., & Fawcett, P. J. (2013). Terrestrial water fluxes dominated by transpiration. Nature, 496(7445), 347-350.

Mianabadi, A., Coenders-Gerrits, M., Shirazi, P., Ghahraman, B., & Alizadeh, A. (2019). A global Budyko model to partition evaporation into interception and transpiration. Hydrology and Earth System Sciences, 23(12), 4983-5000.

Nijzink, R., Hutton, C., Pechlivanidis, I., Capell, R., Arheimer, B., Freer, J., ... & Hrachowitz, M. (2016). The evolution of root-zone moisture capacities after deforestation: a step towards hydrological predictions under change?. Hydrology and Earth System Sciences, 20(12), 4775-4799.

Rinaldo, A., Benettin, P., Harman, C. J., Hrachowitz, M., McGuire, K. J., Van Der Velde, Y., ... & Botter, G. (2015). Storage selection functions: A coherent framework for quantifying how catchments store and release water and solutes. Water Resources Research, 51(6), 4840-4847.

Samaniego, L., Kumar, R., & Attinger, S. (2010). Multiscale parameter regionalization of a grid-based hydrologic model at the mesoscale. Water Resources Research, 46(5).

Schlesinger, W. H., & Jasechko, S. (2014). Transpiration in the global water cycle. Agricultural and Forest Meteorology, 189, 115-117.

Stockinger, M. P., Lücke, A., McDonnell, J. J., Diekkrüger, B., Vereecken, H., & Bogena, H. R. (2015). Interception effects on stable isotope driven streamwater transit time estimates. Geophysical research letters, 42(13), 5299-5308.

Van Der Velde, Y., Torfs, P., Van Der Zee, S., & Uijlenhoet, R. (2012). Quantifying catchment-scale mixing and its effect on time-varying travel time distributions. Water Resources Research, 48(6).

Wei, Z., Yoshimura, K., Wang, L., Miralles, D., Jasechko, S., & Lee, X. (2017). Revisiting the contribution of transpiration to global evapotranspiration. Geophysical Research Letters, 44(6), 2792-2801.