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Hydrology and Earth System Sciences
Discussions

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 #1

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This paper analyses the results of a deforestation experiment on the Wüstebach experimental catchment using conceptual flow and transport models. Overall the paper uses interesting approaches, but needs better structuring, and a more appropriate title. My major comments are as follows:

1. This paper essentially focuses on the Wüstebach catchment, a 0.39 km2 catchment in Germany. Clearly this catchment is a small and specific area. Whatever is found on this catchment cannot have general relevance, considering the place and scale dependence of hydrological processes. The title, however, is very trenchant and gen-

eral, which contrasts with the specificity of the case study. I suggest a more specific title, more reflective of the individual headwater catchment that has been used in the analyses.

2. The introduction is very general, and projects a status quo that is much broader of what is needed to introduce this specific study. The readers unavoidably end up asking themselves: what is already known about this specific catchment? The Wüstebach catchment has been the object of a countless number of studies, which analysed the results of the deforestation experiment, and modelled its behaviour using many modelling approaches (https://experimental-hydrology.net/wiki/index.php?title=W%C3%BCstebach_long-term_experimental_catchment). In particular, Wiekenkamp et al (2016) already analysed issues related to water balance, potential evaporation, and water storage associated to deforestation. I can see that the authors here use different methods. However, that in this catchment "Deforestation reduces the vegetation-accessible water storage in the unsaturated soil" (part of the paper title) is already clear from Wiekenkamp et al (2016), and other studies (e.g. works by Stockinger) have analysed the isotope data and evaluated MRT. These references are cited in the current manuscript. But they are not discussed to provide a clear motivation for the current paper, and to justify the novelty of the results.

3. The motivation for the choice of methods is unclear. E.g. why conceptual models, and SAS are chosen for the problem at hand? Wouldn't the result be obtainable with much simpler methods? It seems to me that if I look at the abstract or conclusions, a simple water balance calculation, and simple hydrograph separation techniques could have been sufficient to end up with the same outcomes. I understand that the authors are proposing a more elaborate approach. But why is that necessary?

4. The authors mention that they use an extensive multi-objective strategy, but in the end they use a single objective function. True that the objective function aggregates multiple objectives, but this cannot be defined multi-objective optimization, which would

require determining the Pareto-front between the various objectives.

5. I would have preferred to see separate results and discussion sections, to clearly see separate the outcome of this work from the outcomes of other works. Currently the blend adds to the confusion of not being able to appreciate the value of the current work compared to earlier work on the specific catchment.

6. Not clear to me why the Su,max of the model would be reflective of available water storage. For example, the model could have an Su,max of 200, but the variable storage between the reservoir can vary between e.g. 30 and 40 over one year, or between 10 and 150. So, Su,max sets an upper bound, but the real variability of the storage can be much smaller. The observation that 10.000 mm of water is necessary to attenuate the isotope signal suggests that indeed Su,max can be much larger than the dynamic range experienced by the catchment.

7. In terms of isotopes, it seems from the figure that there is an increase in the variability of the inputs. This leads to the question of how different are the inputs in the two periods, and whether the increase in young water fraction can be partly attributed to non-stationary inputs.

8. From the uncertainty analysis, it appears that some parameters, e.g. Rs,max, are poorly constrained. But I am guessing that this parameter can strongly affect the behaviour of the Su reservoir, which is the key storage analysed in this paper. Any comment on this?

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