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Interactive comment on "The evolution of root zone moisture capacities after land use change: a step towards predictions under change?" *by* Remko Nijzink et al.

Anonymous Referee #1

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The study of Nijzink et al. investigates the role of root zone moisture capacity on hydrological functioning, in particular after land use change (deforestation). The set-up is clear and the manuscript is well-written. I think this paper fits well in HESS, although some clarifications are needed.

General;

1) The first thing that struck me when getting introduced to the catchments that were used in this study (Table 1) is that the water balances are not closing. For the Hubbard catchments this is hard to check since only PET is given and AET will be lower, for the HJ Andrews catchments, on the other hand, water is 'lost'. Of course it is not a big surprise that a water balance is not closing, given the uncertainty in the observations,

but it becomes tricky when the water balance is used to determine the moisture storage capacity (although you could say that this is also the case for hydrological models that are based on the water balance and that are calibrated on such data). The potential 'disinformation' in observations might influence your estimation of Su,max. I would at least expect a discussion of this potential source of uncertainty, and an estimate of the influence on the results.

2) Lines 7-18 on page 10 show a difficulty of the water-balance method to identify Su,max; you have to assume no storage change. The Introduction describes the importance of flexible Su,max for changing conditions; e.g. land-use change and climate change. And this is where it becomes difficult; under a changing climate (no steady state conditions) you can no longer assume that there is no storage change. In other words; to me it seems that the method to identify Su,max based on the water balance is not applicable in a changing climate.

3) As a proof of concept, a model was included with a dynamic Su,max, which was calibrated by expert-eye to fit the SR1yr-values that were obtained by the water balance method. I agree that a proof of concept is a first step in increasing the process-representation in hydrological models. I would, however, appreciate it if the authors would provide the reader with some suggestions on how to incorporate a dynamic Su,max 'more correctly' in hydrological models. Generally, I am in favor in improving realism in hydrological models, but, extra parameters imply extra uncertainty and the uncertainty should not overwhelm the (hopefully) improved model efficiency. The water balance method seems not feasible in non-steady-state conditions. Do the authors have any suggestions on how to include a dynamic Su,max, or suggestions on observations that could help in this respect?

4) Based on the remarks above, I would suggest to add a separate section to place the results in context (a sort of Discussion, but then different from the one that is included now in the Results section).

Detailed;

a) I know that in the work op Gao and de Boer-Eusink it is shown that climate mainly dictates Su,max rather than the soil. It is, however, maybe valuable to have a look at some of the work of Ilja van Meerveld, who investigated the effect of land use change on soil properties, where it is discussed that the hydraulic conductivity changes as a result of land use change. Could it be possible that the changes in Su,max that you find could actually be assigned to the wrong assumption that the Ksat does not change after land-use change? There are, of course, more parameters in a hydrological model besides a constant moisture storage capacity, that might actually not be completely constant. How can you be sure that the effect you find can only be assigned to the root zone storage and not other parameters?

b) In the calibration of the four hydrological models, two Kling-Gupta terms and the Volumetric Efficiency are used as objective function. As far as I can see, the volume error is already included in the KGE by means of the bias (Beta-term), which would mean that in your calibration strategy, you put extra emphasize on the volume error by explicitly including this term twice (or actually, three times since you use KGE twice). Why is that justified?

c) In your dynamic model, you included extra parameters to describe Su,max, and concluded that it improved the model performance for several indicators. How can you make sure that this improvement is caused by including this process in the model? I would say that for many models you can obtain a (marginal) improvement in model performance by including an extra degree of freedom (an extra parameter), independent of the process that this parameter describes or the realism of the parameterization.

d) I think the research questions in the summary do not exactly reflect the research question in the manuscript (Line 1-5 on page 6).

Overall, some clarifications are needed and some discussion could be added (points mentioned above), but I think that this paper would be of interest for the readers of

HESS.

Kind regards, Lieke Melsen (Wageningen University)

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