Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-427-AC1, 2016

Hydrology and Earth System Sciences
Discussions
Open Access

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.

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Received and published: 17 September 2016

We would like to thank Lieke Melsen for her constructive comments. We will try to improve on the raised issues.

"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

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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."

This is a very valid point. We relate the fact that the water balance does not close mainly to the calculation of the potential evaporation, which here, due to data availability, was estimated from temperature only. We will add a paragraph in the discussion on the consequences of these uncertainties for the estimation of SR.

"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."

We agree with the statement that under changing conditions storage may change. Nevertheless, in the applied method the water balance is merely used to derive an estimate of average transpiration rates. Therefore, we argue that under changing conditions, this estimate is an upper limit of the actual transpiration, whereas in reality it may be lower. In addition, a long-term water balance would not reflect the yearly variations in climate, whereas rather short term water balances may be influenced by storage changes. This is also why, in a trade-off and to keep the effects of storage change as low as possible, the water balances over 2-year periods were used. To substantiate this, to put into context and to assess the effect of storage change, please see Figure 1 below, where, for comparative reasons, we additionally estimated Su,max using a 5-year window to further reduce the influence of storage changes. It can be noted here that the green shaded area, representing the water balance-based estimates, is flatter compared to the results obtained with the 2-year water balance (maximum 500mm compared to 600mm in Figure 4 of the manuscript). This is due to more averaging by taking a longer period for the water balance estimation. In spite of that, the general patterns hold, and in our opinion supports our results. Eventually, we would like to point at the results obtained in the undisturbed reference (or control) watersheds, in Figure S8 of the Supplementary Material. These results are obtained in absence of any land use change, and thus reflect only the changes due to climatic variability (and are thus a proxy for climate influenced inter-annual storage changes). The different pattern compared to the deforested catchments then indicates the isolated effects of storage change due to deforestation and thus transpiration (under the assumption that both control and deforested catchments were subject to the same climate variability). Thus, we would argue that the changes in storage that may occur, are relatively small compared to the annual fluxes of precipitation and discharge.

"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?"

We would like to suggest simple conceptual formulations of growth dynamics, similar to the growth function applied in this case. This would lead to the addition of, at most, three new parameters. These could be free calibration parameters, but we agree that this may lead to additional uncertainty. And even though the water balance method may only give an estimation of the dynamics of the root zone storage capacity, this method may prove valuable to derive at least some information about the *shape* of the growth curve. It can also be noted that transpiration estimates are derived from the water balance in this case, but there are also (remote-sensed) products available to estimate the transpiration. In this way, issues with water balances that may not close are fully avoided.

"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)."

We will add a separate section in the discussion about the uncertainties that are introduced by 1) data used in the water balance, 2) storage changes affecting the water balance. In addition, we will elaborate in Section 4.4 on how to explicitly apply our findings in conceptual modelling.

"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?"

Indeed, there is no absolute certainty that other parameters are not affected by the land use change. Nevertheless, when vegetation is removed, it is not inconceivable to assume that the vegetation-related parameters are considerably affected. This can also be seen from the posterior-distributions of the other parameters, see the Supplementary Material. In the 2-year window calibration, all parameters were left for calibration, and they all had the freedom to change over time. Nevertheless, the root zone storage capacity showed the most dynamical character, whereas others remained more constant in time. In addition, we would expect that changes in hydraulic conductivity are tightly linked to changes in porosity. In other words, an increase of porosity is not unlikely to decrease the flow resistances and thus increase Ksat, while simultaneously reducing the storage capacity. It must also be noted that hydraulic conductivity Ksat cannot be compared directly to any of the catchment scale conceptual model parameters applied here.

"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?"

This is a valid point; we will compare the outcomes with a calibration based on a combination of KGE and logKGE to test how much this influences our results.

"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."

To avoid this, both model approaches were given the same number of degrees of freedom. In other words, both models had the same number of free calibration parameters. This is why the growth functions were fixed, and not left for calibration.

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

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We will rephrase it to be more consistent throughout the manuscript.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2016-427, 2016.

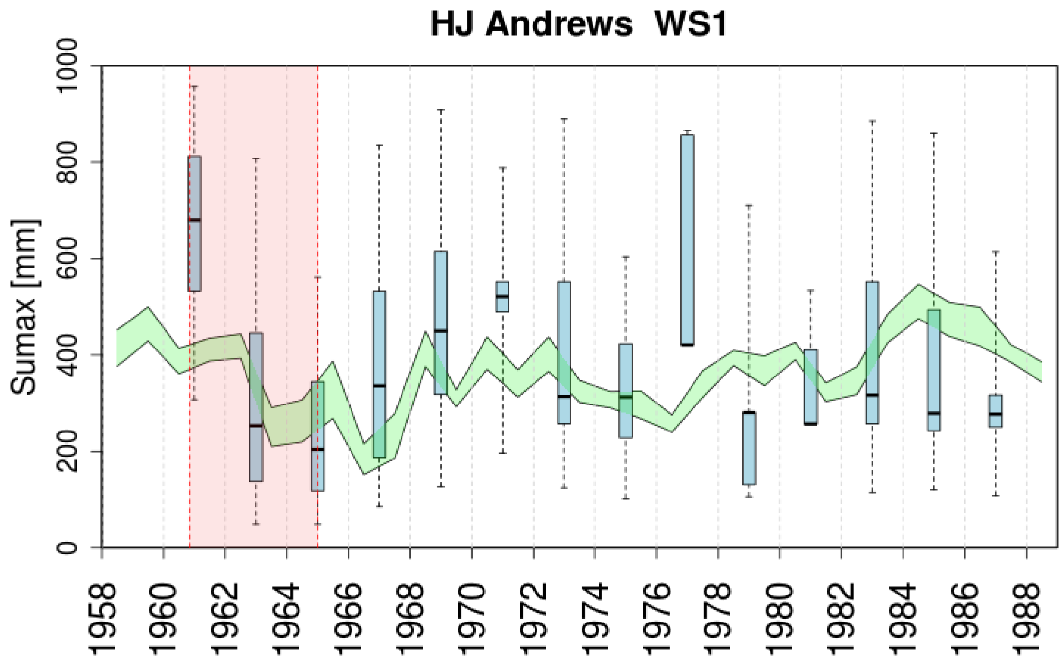

Fig. 1. Evolution of root zone storage capacity SR,1yr from a 5-year water balance-based estimation (green shaded area) compared with estimates obtained from the calibration of FLEX (blue boxplots).

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