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Discussions

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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We would like to thank Dr. Ducharne for her feedback on the manuscript. We will try to improve on the comments and raised issues.

1. We lack a lot of information regarding the models and their use. The main idea is to propose evolutions of the root zone moisture capacity (RZMC) at a yearly time step by a kind of inverse modelling using the observed river discharge of the perturbed and unperturbed catchments as input.

1.a) The simple "water balance model" allows a direct inversion of the RZMC, given parameters describing the canopy interception processes and the vegetation recovery time, and restricting the water balance to only 5 months between May and October, to get rid from the influence of snow (the experimental catchments are located in Oregon and New Hampshire):

- Unless vegetation growth is really restricted to these 5 months, this tends to underestimate the RZMC, and could explain why the Hubbard Brook estimates are so small for forested sites (23 mm on Figure 1)

We agree with it that vegetation growth is not restricted to these 5 months, but we argue that droughts are restricted to these 5 months. Changing the approach to the full year will indeed result in higher values, but only because water will be stored in the root zone (the simple method does not account for snow), whereas it is actually snow storage. Nevertheless, the actual dry periods are generally in July – August for these catchments. Thus, the deficit of E-P, which actually controls the storage capacity in the root zone, will be the largest in these periods. We would like to clarify here, that for the estimation of the mean Et the full two year period is considered, only the calculation of daily deficits of Et – P was taken over the 5 month summer period.

- The total evaporation seems to comprise only transpiration and interception loss, and neglect soil evaporation: is it justified?

It is correct that we do not treat soil evaporation as individual process. Rather, we lump the physical process of evaporation using one interception storage. This will without doubt introduce some uncertainty, but separating the processes is not really warranted by the available data and will result in increased parameter equifinality and thus considerable additional uncertainty. In addition, we argue that our transpiration estimates represent upper limits of transpiration, assuming a negligible amount of soil evaporation. In reality, the transpiration will indeed be lower due to soil evaporation. We will add a paragraph about this in the discussion.

- Transpiration depends on a potential evaporation, which is not explained in the paper: does potential evaporation depend on the development of the canopy, as could be quantified by the Leaf Area Index (LAI)? This dependence is well known fact, and can be described for instance by the crop coefficient when following the FAO guidelines of Allen et al. (1986), or as a function of LAI like in the SVAT (Soil-Vegetation-Atmosphere Transfers) models. If such dependence exists in the experimental catchments, it should lead transpiration to decrease after deforestation, and recover with vegetation regrowth, with opposite effects on runoff, in agreement with Figure 2(a-c). In this case, if the model overlooks the positive link between vegetation development and the magnitude of transpiration, it should lead to underestimate the decrease of transpiration after deforestation, and to overestimate the decrease of the RZCM to match the increased observed runoff.

The potential evaporation was determined based on a temperature based method (Hargreaves equation), and thus did not depend on vegetation. We will add this information in the Methodology. Also, the water balance based model used transpiration estimates, which were exclusively based on the observed water balance. Here, potential evaporation is thus not needed to determine the mean transpiration and was only used to scale the long-term mean value of transpiration to a daily time series.

- A Monte-Carlo approach is used to assess the effect of the 3 parameters involved in the model (see Table 2) and this allows deriving a very useful uncertainty range around the estimated RZCM. Yet, no justification is given regarding the selected range for these parameters, which is a strong constrain to the uncertainty.

We would like to refer to Figures S9-S26 in the Supplement. Here, all posterior distributions of the parameters are shown. It can be seen that none of the parameters has an extremely narrow posterior distribution close to one of the bounds of the prior distributions (i.e. upper and lower limits), which would point towards too narrow prior distributions. Only in a few instances, the distributions are close to values of zero, but negative values are not possible for these parameters (e.g. Figure S9b and S9f.) Thus, in general the applied parameter ranges were sufficient for the calibration.

1.b) The other four models are published conceptual hydrological models, and are calibrated over consecutive 2-yr windows to match the observed water discharge. These models seem to describe the full hydrological year, including the periods of snow, which is a significant difference with the previous approach. Even if some information is given in the Supplementary (but not at the same level for all the models), the reader should find in the main text if the snow is explicitly described, and how the evapotranspiration is calculated (in particular how it depends on the vegetation development, for the same reasons as explained above).

The conceptual models applied here all use similar functions as originally proposed by Feddes et al. (1978), with the resistance for transpiration as a part of the model (see equations in model descriptions in supplementary material S2). Thus, the models reflect the vegetation influence on transpiration, whereas the potential evaporation exclusively reflects the total energy available for evaporation, which is common practice in the vast majority of hydrological models. All models also used a snow module, as we described in the manuscript (p11,line 12 ; p11, line 27; p12, line 8). Nevertheless, we will try to state more clearly in the model descriptions how evaporation and snow are determined.

Some details should also be given regarding the calibration itself: How many pa-

rameters are calibrated in addition to RZCM (Su,max) for each model? Can all of them change in each 2-yr window, or does only Su,max change? How many tested parameter sets? How many parameter sets are kept at the end of the calibration (equifinality) and what are the corresponding performances to fit the observed discharge? There is a long paragraph from p12L27 to p13L14 which is rather hard to follow for non-specialists of optimization, and could usefully be replaced by objective information regarding the qualities and weakness of the resulting calibration.

We will add the number of free parameters for calibration in the model descriptions. Generally, almost all parameters were left as free calibration parameters. All parameters in HYMOD (8 parameters) and TUW (15) were free calibration parameters. The 9 parameters of FLEX were all free for calibration, only the slow reservoir coefficient Ks was sampled between narrower bounds, which were based on a recession analysis. The HYPE model used 15 parameters for calibration. We will also add information about the number of initial model runs (100,000 runs) and the number of final feasible parameter sets. The performances for three calibration objective functions (KGE, logKGE and VE) are summarized in Figures S5-S7, for each sub-period of calibration.

1.c) Another model is used, and presented in 3.5. It's an adaptation of FLEX, one of the above four models, in which an a priori rule for RZCM recovery with time after deforestation is added. First, it would probably be clearer if this model was presented just after the others. Second, much information, again, is lacking:
- How is the evolution Imax described since it also varies with time (p15L17-18)?

We will clarify how Imax changes in time in that model. We applied the same growth function (Equation 11), with growth parameters a and b set to respectively 0.001 [day-1] and 1 [-].

- How are the parameters a and b of Eq. 11 selected? The resulting values are only given in the caption of Fig8, but don't they deserve some analysis? Do they relate logically to the recovery times that are discussed in section 4.3?

We will clarify this, but we would also like to refer to lines 12-16 of page 15. The parameters were determined based on a qualitative judgement (thus, just with the 'expert-eye') as it was just meant as a proof-of-concept. We fully acknowledge (p.15, l.20-27) that this is a mere exploratory analysis and a more thorough analysis, which may also include explicit and more detailed process understanding on root development, may be needed to have more adequate values for the growth parameters.

- How is decided when is RZCM minimum, and which is the minimum value, since Eq. 11 only describes the increasing part of the variations shown on Figure 8?

The minimum and constant values are determined in the same way as the shape of the curve, with qualitative judgement.

- Fig 8 shows performance criteria with and without the dynamic formulation of Su,max: to which period do they correspond? We must assume that the period is the full observed period for each catchment, but does it make sense for HB5, where half of the full period is before deforestation? Couldn't it be interesting to test the proposed function over the recovery period only?

The performance criteria in Fig. 8 correspond to the period just before the treatment until 15 years after the treatment. Therefore, it was not for the full observation period, also for Hubbard Brook WS5. To be more precise, HJ Andrews WS1 was evaluated from 01-10-1960 untill 30-09-1981, Hubbard Brook WS2 from 01-

10-1962 untill 30-09-1983, Hubbard Brook WS5 was evaluated from 01-10-1982 untill 30-09-1999. In this way, we tried to 'zoom in' on the recovery period, just as you suggested, see also page 14, lines 22-25. We will make this clearer in the revision.

2. The conclusions are too frequently not supported by the Figures. Examples:
- p17,L3-4: "the three deforested catchments in the two research forests show generally similar response dynamics after the logging of the catchments (Fig.2)." No, for each of the rows/signatures, you can find one outlier over the three catchments.

This is why we stated it as 'generally similar response dynamics'. We never claim the responses are exactly the same for all the catchments. We will rephrase this to 'on balance similar response dynamics'.

- p18, L24-26 (regarding Figure 4): "Comparing the water balance and model-derived estimates of root zone storage capacity SR and Su,max, respectively, then showed that they exhibit very similar patterns in the study catchments." This is abusive since TUW and HYMOD completely miss the difference between HJA and HB, and HB5 doesn't show a clear response to deforestation against inter-annual variability for most models. When discussing Figure 4, the focus is put on the differences in RZCM due to deforestation and recovery. Yet, these differences are much smaller than the ones between the sites, and have a similar magnitude as the inter-annual variability for the two Hubbard Brook catchments. This should be taken in consideration in the discussion.

We would like to point out that we discuss the pattern, thus the dynamics, not the absolute values. Especially TUW and HYMOD show a bias (mostly due to the absence of an interception storage) compared with the water-balance method, but still show

similar dynamics (decreasing during deforestation and a gradual increase afterwards). We discussed the possible reasons for the difference between the HJ Andrews and Hubbard Brook catchments (p19, line 5-11 and p20 line 16-18), but we will elaborate more on this in the revision. Briefly, HJ Andrews has a strong seasonal regime, whereas in Hubbard Brook the precipitation is more equally spread throughout the years. Therefore, HJ Andrews has a high need of large root zone storage capacities to allow access to sufficient water throughout the relatively long dry summer period, whereas the Hubbard Brook catchments can survive with much smaller storage volumes, due to significant summer rainfall and thus shorter dry periods that need to be bridged. We agree that inter-annual variability is high, but this is also the reason why we carried out the trend analysis with the undisturbed reference watersheds. In this way, the influence of inter-annual climatic variabilities should be filtered out.

- p20, L23-26: "It can be argued, that a combination of a relatively long period of low rainfall amounts and high potential evaporation, as can be noted by the relatively high mean annual potential evaporation on top of Figure 4b, led to a high demand in1985". But the top three plots on Fig 4 are so small we can't see much!

We will make the plots bigger for clarity.

- p21, L3-4: "Generally, the models applied in Hubbard Brook WS2 show similar behavior as in the HJ Andrews catchment." It's far from being obvious for HB5.

This is absolutely correct and therefore, we do not state this.

- p22, L16-17: "The results shown in Figure 4 indicate that these catchments had a rather stable root zone storage capacity during deforestation" (for HJA and HB2).

Deforestation is indicated by a red band, and we clearly show a decreasing, not stable, RZCM during deforestation in HJA; for HB2, we don't see anything because the y-axis range is too large.

We will rephrase this; we basically meant from more or less halfway the period of deforestation (for HJ Andrews just after 1964, and Hubbard Brook WS2 1967). We will try to make the plots clearer as well.

- p23, L24-28: "Evaluating a set of hydrological signatures suggests that the dynamic formulation of Su,max allows the model to have a higher probability to better reproduce most of the signatures tested here (54% of all signatures in the three catchments) as shown in Figure 9a. A similar pattern is obtained for the more quantitative SRP (Figure 9b), where in 52% of the cases improvements are observed." This is abusive because your get degradation of the performance for 46% of the signatures in Fig9a, and 48% in Fig 9b, which is far from being negligible. If you look at HB5 only, the degraded signatures dominate, which contradicts the conclusion at p24, L27-29.

We only stated what we found and never deny that 46% and 48% of the signatures show a decrease in performance for the two metrics. Moreover, it is also for these decreasing performances that we added the discussion starting from p24, line 13 until p25, line3, where we explained the origins of these decreases. The statement on p24, line 27-29, also refers to the rather light colors of red and blue, which indicate probabilities around 0.5 and SRP values around 0, thus not a strong preference for one of the two models. We will further clarify this in the revision.

- p24, L6-7: "In addition, a dynamic formulation of Su,max permits a more plausible representation of the variability in land-atmosphere exchange following land use change". Where does this come from? Provided that no signature in Fig 9 and Table

3 addresses the variability of land-atmosphere exchanges (all the signatures describe elements of the streamflow time series).

We will remove this sentence.

- p24, L9-10: "Fulfilling its function as a storage reservoir for plant available water, modelled transpiration is significantly reduced post-deforestation, which in turn results in increased runoff coefficients": if I see well on the very small Fig 2c, the results show exactly the opposite for HB5.

We agree with this, but please note that in the line referred to in this comment to (p24, line9-10) we exclusively discuss the results for HJ Andrews. The two Hubbard Brook catchments are discussed in the following paragraphs.

- p24, L19-21: "This can also be clearly seen from the hydrographs (Figure 10), where the later part of the recession in the late summer months is much better captured by the time-dynamic model." Personally, I see exactly the opposite, as the time-varying RZCM model in Fig 10b overestimates the peaks, which is not the case of the constant RZCM model in Fig 10a.

We are confused by this comment, as we clearly see the same considering the peaks in Figure 10b, which we also discuss at page 21, line21-26. We agree that the improvement in the lower parts of the recession (thus not the peaks), is hard to see in Figure 10b, but we still believe this statement is supported by the figure. Please note the additional white space between observed and modelled discharge in the recession of July – August in Figure 10a (time constant model) compared to Figure 10b (time-varying model). To clarify, we will add insets into figs.10a and b, zooming in to a selected low flow period.

- Finally, the conclusion relies on a selection of the results that support the assumption of the authors, without considering the results that contradict it, and without a hint of doubt. The limits of the approach (including the model dependency, the small sample of observations which are not perfectly consistent) are not all discussed, nor any alternative frameworks. The authors could for instance consider the possibility that the RZCM could remain unchanged but not fully exploited by the vegetation. This is typically what helps some types of vegetation to resist to drought conditions.

We tried to keep the discussion brief and stated here the general findings. We believe there are good reasons the results in Hubbard Brook WS5 were less clear, which we also discussed (e.g. p21, line 14 until p22, line 3). Nevertheless, we will add in the discussion and conclusion sections more on several shortcomings and limitations, additional to what we already state in the discussion. We find the remark that root zone storage capacity could remain unchanged very interesting, and we use exactly this argument in our discussion on p19, line 29 until p20, line 6. We will make this clearer in the revision.

3. Abstract:
The abstract is not very clear regarding the methods (the proposed method is not solely based on climate data as written at L8-9, but it requires information on the deforestation, based on inverting the discharge observation in the present case). Like the conclusion, it builds too much on overstatement, but there is also an annoying circular reasoning, since the main conclusion comes from the beginning (L5-7: "Often this parameter [RZCM] is considered to remain constant in time. This is not only conceptually problematic, it is also a potential source of error under the influence of land use and climate change.")

We will clarify the abstract with the remarks made here. Again, we tried to generalize, which is unfortunately interpreted as an overstatement. Nevertheless, we will add more on the methods and try to clarify.

4. Other comments:
- Trend analysis (method in 3.4, results in 4.3): is it really about trends or about variability? Can we really speak of "trends" on sub-periods as short as those highlighted in blue and green in Fig 7o and 7r? Couldn't these two periods be lumped together? Some references should be given where to find more details on the extraction and interpretation of the 95%-confidence ellipses. Finally, Fig 7 is much too small.

We agree, at first the method is applied to detect a trend. In the second step, it is used to detect homogeneous sub-periods without a clear trend. We applied the differentiation between sub-periods as objectively as possible, based on the break points in Figures 7d-f. For the construction of the 95%-confidence ellipse, we refer to Equations 9 and 10, and the FAO-guidelines (Allen et al., 1998)

- Some sentences I did not find clear, although the paper is generally well written:
- p3, L13-15: "By extracting plant available water between field capacity and wilting point, roots create moisture storage volumes within their range of influence."
- p 4, L7-8: "other species with different water demands may be more in favor in the competition for resources"
- p4, L15: "These studies found that deforestation often leads to higher seasonal flows". Do you mean higher peak flows?
- p4, 30-31: "More systematic approaches, thus incorporation the change in the model formulation itself"

- p14, L28-29: "the calibration was run with a series temporally evolving root zone storage capacities"
- p26, L27: I suggest using attributed to rather than caused by, unless a clear causality can be demonstrated.

We will rephrase the sentences mentioned here.

References

Allen, R. G., Pereira, L. S., Raes, D., and Smith, M.: Crop evapotranspiration-Guidelines for computing crop water requirements-FAO Irrigation and drainage paper 56, FAO, Rome, 300, D05109, 1998.

Feddes, R. A., Kowalik, P. J., and Zaradny, H.: Simulation of field water use and crop yield, Centre for Agricultural Publishing and Documentation., 1978.

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