Manuscript hess 2016-427 by Nijzink et al.: "The evolution of root zone moisture capacities after land use change: a step towards predictions under change"
Review by Agnès Ducharne

The proper way to define the root zone and corresponding storage capacities, whether for water, or carbon, or nutrients, is a very topical research question in environmental science. This question is addressed here from the point of view of water, which is relevant for HESS, and in the framework of land use conversion, to question the need for implementing a dynamical description of the root zone moisture capacity in conceptual hydrological models. The method relies on comparing several such models (including a very simple water balance model) to long-term observations in three experimental catchments having undergone deforestation, and paired with control catchments without land cover change.

This looks like a sound research strategy, but the paper itself suffers from shortcomings, which cast serious doubts, to my opinion, on the relevance of the proposed conclusions. In particular, if the results do show a dependence of the water budget on the land cover (at least in 2/3 catchments), it is not clear from the results if this dependence comes or not from the RZCM. It is driven by the RZCM in the selected models, but this is not systematic (see my comment 2 below), and may be too model-dependent to have a broad meaning (see comment 1a).

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

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).
Some details should also be given regarding the calibration itself: How many parameters 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.

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)?
- 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?
- 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?
- 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?

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.
- 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.
- 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!
- 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.
- 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.
- 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.
- 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).
- 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.
- 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.
- 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.

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.")

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.
- 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.