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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Testing an optimality-based model of rooting zone water storage capacity in temperate forests" *by* Matthias J. R. Speich et al.

HHG Savenije (Referee)

h.h.g.savenije@tudelft.nl

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This paper presents an optimality model of Guswa (2010) to determine the root zone storage capacity of temperate forests with understory, based on net carbon profit. It then compares it to the root zone storage capacity of a water balance model FORHYTM calibrated on: 1) locally observed total evaporation by eddy covariance; and 2) observed soil moisture.

To me, the value of the paper is not so much that reasonable results are obtained that could potentially be used to predict root zone storage capacity, but rather that by using the net carbon profit approach insight is obtained in what triggers vegetation to evolve

towards a certain storage capacity. From the results obtained in the paper, it is clear that this is not so easy to do. The net carbon profit apparently does not appear to work so well in a Mediterranean climate, and also not on sandy soils. So what we learn form this approach, is that it is apparently not as simple as that.

In my view, the weakness of the approach is that the results are subject to very high uncertainty. Both the optimality model and the conceptual evaporation model are heavily parameterized. Both the soil properties and observed soil moisture suffer from high heterogeneity (both in horizontally and in depth). Also the many coefficients for the phenology, the seasonality and the climate contain substantial uncertainty. The evaporation model, which contains 4 Jarvis-like coefficients for the vegetation in question, has many uncertain parameters as well. So, it is very hard to conclude what we are comparing. We don't know if the mismatches in Fig.7 stem from the optimization model, the water balance model, the heterogeneity in the data (particularly soil moisture and soil properties, but also the fetch of the eddy covariance) or the many parameters and empirical equations used.

This drawback is much less in the climatic water balance approach of Gao et al. (2014) [referred to in the paper], which is primarily data driven, based on the difference between evaporation and precipitation at catchment scale, although also these data have some uncertainty. Gao et al. (2014) compared the estimates of the root zone storage capacity to values of a hydrological model calibrated on the runoff of 329 catchments in the USA and Thailand. Similarly, Wang-Erlandsson et al. (2014) [referred to in the paper], using a similar climatic water balance approach for the entire globe at pixel scale, compared her results with a water balance model similar to FORHYTM, calibrated on a global data set of evaporation based on an energy-balance approach. These two approaches contain much less parameters and less empirical relations, and hence suffer considerably less from uncertainty and provide more reliable estimates of root zone storage capacity at a variety of scales, but – admittedly – present no explanation for the objectives ecosystems apparently try to satisfy.

This is what I think the value of the paper is: providing insight into what triggers ecosystems to evolve towards a certain 'optimal' storage capacity. But then, I would expect the authors to discuss more the relevant objectives of ecosystems under different constraints and also to think of other factors that may determine trade-offs. The authors should realise that getting a model to work for temperate climates is not so difficult. It is far more difficult to predict root zone storage capacity in Mediterranean, semi-arid, or tropical conditions, or under climate change; and this is what we would like to be able to do. Gao et al. (2014) and Wang-Erlandsson et al. (2016) managed to predict root zone storage capacity under present – widely varying – conditions, but without providing insight into what drives the ecosystem to converge to this capacity, besides securing sufficient water to survive (and reproduce).

A few observations, some of them minor:

1. I think that the authors conceptualised the interaction between over- and understory well. I think this is not a problem of the model.

2. I do not recommend using the word "loss". In hydrology there is no loss. Use interception evaporation instead of interception loss, as is done in line 16 of p8. But do it throughout.

3. Please don't use the concocted word evapotranspiration. You managed to avoid it almost everywhere and correctly used the term "total evaporation" or just "evaporation" instead. But it still remains in a few places: line 12 of p8, line 18 of p.9, line 13 of p22.

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