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Hydrology and Earth System Sciences Discussions

Interactive comment on "The potential of observed soil moisture dynamics for predicting summer evapotranspiration in a successional chronosequence" *by* J. A. Breña Naranjo et al.

Anonymous Referee #3

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This paper deals with the estimation of evapotranspiration at three forest stands in different stages of ecological succession. This issue is of major importance and worth publishing in this journal. The manuscript is well structured and the results are well presented. It is commendable that the manuscript is written in a compact way. However, the manuscript in its current form leaves many questions unanswered and raises some major concerns. From my point view, the model needs another model application and validation strategy or a much better line of argument to justify a publication in HESS. In the following, various aspects which led to this assessment are discussed:

1) Motivation and scope: The authors argue that succession has a large impact on soil-water-atmosphere-transport processes and that there exists uncertainty with respect to the timing of the recovery, which leads to the question of required complexity of an adequate evapotranspiration model. The large-scale problem of disturbed forests makes it necessary to derive models which are also applicable in data-scarce regions. Against this background, a very simple water balance approach is proposed to estimate the evapotranspiration in summer. Two core questions to be answered are whether such an approach is valid for forests in the drought influenced season and if it is sensitive to forest age. In my opinion, the manuscript cannot satisfyingly meet the requirements set in the introduction (see also comment 4). The context of the successional chronosequence appears somewhat contrived. This model is of such simplicity and does not include any biophysical parameter taking the vegetation explicitly into account, that it would be more interesting if this water balance approach works at all. To clarify this question it would be very interesting to apply the model at more sites with a wider range of site conditions. The issue regarding the differences of evapotranspiration within successional chronosequences can better be addressed by analyzing eddy-covariance data, and the recovering from disturbances by analyzing remote sensing data. From what is shown in the manuscript, it seems simply not to be possible to reduce the "uncertainty in estimating the timing of such long-term recovery" (p.5303, l.11f) with this model. It is furthermore questionable to me to tackle the complex problem of required model complexity with two models that just differ in an interception component which is very roughly parameterized and does not take any physiological differences between forest stands into account.

2) Data: More care should be given to the description and justification of the data used. Which eddy-covariance data were used? In FLUXNET, different types of data are usually provided which differ in the processing level. Were the often considerable measurement gaps filled? If yes, how were they filled? Large discrepancies can arise from different gap-filling methods. Was a quality control applied to the usually very noisy data and spiky time series? The energy-closure problem is mentioned and value for the amount lacking at each site are given – but are any consequences drawn from these values in the usage and analysis of the observed data?

Data from only two TDR sensors are available. Were there measurement gaps and how were they filled? Are the values from the two TDR devices averaged? Are they representative of the eddy-covariance footprint? The referenced paper by Schwärzel et al. (2011) stresses the importance of taking small-scale heterogeneity of soil properties and soil water contents into account. In the light of this paper, is it realistic to propose a model for large-scale applications in data-scarce situations which requires detailed soil measurements? It is explicitely stated in the manuscript that forest degradation and recovering is a large-scale phenomenon. What was actually the disturbance at this site? From which event(s) does it recover?

3) Modeling: Likewise, the model itself should be described more precisely and should be more profoundly justified. The assumed active root depth, Z_r, is not specified before the discussion section. The determination of the percolation, q, is not further mentioned at all. Later in the text it is stated that the model does not require soil properties; is q assumed to be independent from these? Model I and II should be given in their final form. What is the justification for the assumption of I (which is not specified precisely) equaling P for low intensity rainfall events and being limited at $0.5mm/h$ for high intensity rainfalls? Why are these values set constant for all three forest sites if the LAI and therewith the interception differ substantially due to the different age? The LAI-differences between the young stand ($LAI = 1.1$) and the mature stand ($LAI = 7.3$) are huge! LAI-time series are still difficult to retrieve from remote sensing – but would the incorporation of rough estimates not be better than one single interception value for all forest stands if succession plays such a major role?

To evaluate the model with a time step of one day, "the precipitation time series were also aggregated to daily values" (p.5307, L.23). Then, however, it is found that the mean precipitation values do not reach Gash's interception threshold, so interception is not accounted for at all using daily time steps. But why is there no interception at

low rainfall intensities? I am not familiar with the concept of Gash, but in the model description it is stated that "$I(t) = P(t)$" for "$P(t) < 0.5mm/h$" (p. 5305, l.20f), thus the whole amount of precipitation is assumed to be intercepted and evaporated; otherwise the interception and therewith the evaporation from this storage is limited to $0.5mm/h$. Thus, there seems to be a maximum threshold and not a minimum. I further conclude from this, that — although a daily time step is used — the fluxes are still related to hourly values. Would it be an option to sum up the fluxes to daily values and use an adapted interception threshold related to the daily time step, even though it would lack a direct physical meaning? Furthermore, no other time-steps are tested in the model analysis, thus conclusive results regarding the optimal time step are difficult to obtain.

4) Discussion and conclusions drawn: When deriving the second model, it is argued that an interception component is necessary since "interception will play an important role in evapotranspiration" (p.5305, l.17) in mature stands with a greater LAI. Then it turns out that the model without the interception already overestimates the evapotranspiration at the mature stand — so just other the opposite than what would have been expected — and the model deteriorates incorporating the very rough estimation of interception. Only at the intermediately aged stand, the model with the interception improves the variability of evapotranspiration in five out of six years. It is concluded that increasing the complexity of the model is not necessary. But is this inconsistent outcome not rather a sign that the simple soil water balance without the interception is problematic and, additionally, the interception is inadequately parameterized?

One of the core questions of this paper was: Is the model sensitive to forest age? In the Discussion, this question is answered by the statement that the succession has no systematic effect on the model performance. It is not mentioned that the proposed water balance method provides such a rough estimation of the fluxes that the differing behavior of the forest stands as apparent in the data cannot be reproduced by the model. It is not mentioned that the model errors are often larger than the observed evapotranspiration differences between the stands. And it is not mentioned that only the ranking of the total sums averaged of several years is consistent between observations and model results (or was that meant by "first-oder estimate" (p.5311, l.2)?). How can a model be presented as being adequate for successional chronosequences if it cannot resolve the differences? How can hydrological changes be studied in recovering ecosystems (as stated on p.5311) if the differences between the stages obviously cannot be modeled using this approach? Calling for a better examination and estimation of the effects of succession was the starting point of this study (see also comment 1)!

Further (minor) comments:

- The statement regarding the model performance with respect to the total summer AET is somewhat dubious ("ranged from 3 to 14% [. . .] with an exception of 28%") if there are only six values (by the way, a value of 24% is listed in the table).
- In the bibliography, the references Teuling et al. 2006a and 2006b are not indicated.
- The reference given for the Campbell site (Baldocchi et al., 2001; p.5304, l.4) is the basic reference for the FLUXNET initiative and not for this site.
- It would be helpful if the second plot in Fig. 2 is scaled from 0 to 300 mm (like the two other plots).
- "$Z_r = 30$ cm proved superior" (p.5309, l.19) – from which evidence is this conclusion drawn?
- The argumentation regarding the added value of this study in comparison to similar studies (p.5310) is poor and should be rewritten.
- The need to specify all variables at their first usage was already mentioned above. For the sake of completeness, the units should also be given when the symbols are explained on page 5305.

- The readability of Table 2 could be improved by avoiding parenthesis and brackets directly next to each other.
- It would be very interesting to see plots of the measured time series of precipitation, soil moisture and evapotranspiration.

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