

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.

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We appreciate the thorough review and the opportunity to discuss and clarify some of the items raised in this interactive comment.

The referee notes that we i) test two models and ii) present data from only one location, which the referee questions to be sufficiently original. We would hence like to start our reply by regretting that very few research sites monitor energy and water fluxes in successional chronosequences and indeed this was the only one we could find that had enough available data for this analysis. Exactly because of this lack of

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observations (5303;16), our aim was not to simply present data, but to test a method to derive AET estimates in successional forests in which extensive micrometeorological measurements do not exist: i.e. whether and to what extent a rain gauge and soil moisture measurement might be sufficient. We comment on that in the introduction (5303;20) and conclusion (5311, l. 5-7), but acknowledge that this background needs to be highlighted better. We suggest that in the revised manuscript we emphasize this aspect more visibly in the introduction and as an objective. Following this idea it was also the specific aim to keep the water balance model as parsimonious as possible (referee comments i-4. and i-5.)

i) Modelling

1. We agree that H_r is an important parameter. For forest trees incl. Douglas fir, the majority of roots are very shallow (within the upper 50 cm). We will provide some references in the revised manuscript. However, we did test the model with the 60cm measurements but found 30cm to provide best results. An example could be included in a revised ms if required.

2./3. We agree that we should comment on variability. There will be some spatial variability of both variables, of course, but there are no obvious influences such as topography that suggest that variability is large. At each of the three sites only two point measurements are available for soil moisture, which were averaged, and one measurement for precipitation. Additional measurements over a considerable time period would have to be taken to elucidate the issue.

4. This is a common problem for flux tower data, yes. However, this site is above average compared to the non-closure at other Fluxnet sites. As we only used the data but do not run the site ourselves we don't feel comfortable with correction that would also require additional assumptions. Instead, we suggest to consider the issue better in the interpretation of our results and their uncertainty.

5. As stated we based this assumption on the literature. More complex relations could

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be tested at this site where such measurements as suggested are available and we suggest that we could take a closer look at the transferability of the 0.5mm threshold. However, with respect to the aim of the model to use only rainfall and soil moisture (but not detailed canopy exchange processes) the simple approximation seemed a logic choice.

ii) Data and minor comments See our general reply above concerning the availability of more than the three sites we considered and concerning the usefulness for areas where no eddy covariance measurements are available. We agree that all four minor comments by the referee deserve our attention and text and equations will be rephrased and edited for clarification in a revised ms.

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