

## ***Interactive comment on “Potential groundwater contribution to Amazon evapotranspiration” by Y. Fan and G. Miguez-Macho***

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I thank the authors for sharing their thoughts on the comments I made previously in my review. First of all, I agree with the later comment by Marc Bierkens that leaving out the capillary rise part would make the paper less interesting. I would therefore suggest the authors to take a different road in making their revisions than the one they propose in the reply (i.e., removing the capillary rise section while adding more information in other parts). However my problem with the paper lies not with the fact that the authors make a first-order estimate. Rather, it is related to the fact that the authors use a static approach to draw conclusions about the systems' dynamical behavior (capillary rise during the dry season only). Since there are only few regions on Earth which

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experience a larger seasonality in precipitation than the Amazon, this approach is likely to lead to incorrect numbers as discussed in more detail in my previous posting. In my view, this gives a false message to both the hydrological and climate community, which can and therefore will lead to misinterpretation of the results.

This can be most easily circumvented by removing any reference to the dry season in the manuscript (except introduction) and present the capillary flux part as what it really is: a steady-state analysis covering *both* wet and dry seasons. In this way my biggest concern is addressed in an simple way, and without the necessity for major changes or new simulations. While I understand that the authors would like to “sell” their results in the context of the widely discussed dry-season ET, such claims can in my view simply not be supported by the steady-state approach followed in the paper.

In summary, I suggest the authors revise their manuscript along the following lines:

1. Add one or two sentences to the abstract that state the limitations of the approach, for instance: “Future work will address the impact of non-linearity in the full saturated-unsaturated zone interaction in combination with the strong seasonality in forcing in the Amazon. These aspects are not covered in the current paper, but they can significantly alter the results presented here.” In addition, change “the water table can potentially sustain” into something like “The water table can accommodate a potential equilibrium capillary flux of  $>2.1$  mm/d averaged over Amazonia”.
2. Remove any reference to dry-season ET in the discussion of the results, and if necessary correct the numbers in the figures to represent yearly-average ET.
3. Add a discussion on why the capillary fluxes could also be over- rather than underestimated by taking recharge from uncoupled simulations along the lines suggested in my previous comment and also by Marc Bierkens. Remove references to the “conservative” character of the estimates. The fact that a cap has to be

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used on the data already proves capillary flux can be overestimated locally.

4. Add a discussion on the possible impact of dynamical effects.
5. Consider the remaining (minor) comments in my previous posting.

I believe these changes can be made relatively easy, and will lead to a manuscript that maintains the charm of using a simple approach to tackle a complex problem, while it does no longer make unsupported claims or omits discussion of the effects of non-linearity in dynamical systems.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 5131, 2010.

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