

## ***Interactive comment on “Inundation and groundwater dynamics for quantification of evaporative water loss in tropical wetlands” by J. Schwerdtfeger et al.***

**Anonymous Referee #2**

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The paper under consideration focuses on tropical wetland evaporation, and seeks to developed a simple and empirical two-stage model to estimate AET based on departures from PET. The rationale for this work is clear...tropical wetlands are important and under-monitored environmental resources, hydrology controls the condition and health of these wetlands, and ET is the largest and most uncertain component of a wetland water budget. The paper is relatively well written, and organized effectively, and while I was not convinced that the analysis was novel, it likely represents an important validation of simple PET and AET methods, which is particularly salient given the remote setting.

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While I was generally impressed with the scope of the work, I had several comments that I hope the authors will find constructive.

1) The authors base their analysis on the premise that tropical wetlands are heterogeneous. Moreover, they discuss the implications of their work in the context of spatial extrapolation in complex landscapes, but they basically fit one set of model parameters for the decline of AET with water table depth using Bowen ratio measurements at one location. By inference, either tropical wetlands are not all that heterogeneous, and single station towers would suffice, or the model parameterization needs to be linked to spatially varying properties (elevation, vegetation cover and type as a proxy for rooting depth, soil properties) that are not considered here. I found the extrapolation of the highly local results to be somewhat problematic, not because they were necessarily incorrect, but because I really don't have a good sense of whether decline and recovery slopes are always the same (as implied by Fig. 7).

2) The basic model is somewhat problematic because it neglects the water table effects of specific yield (i.e., drainable porosity, which is  $\sim 1.0$  in open water, and  $\sim 0.2$  in soils/sediments). The stage variation of specific yield has been the subject of considerable scrutiny in wetlands (e.g., see Hill and Neary 2004, Sumner 2008, Tamea et al. 2010, McLaughlin and Cohen 2014), and in all cases it proves to be an exceedingly important variable for predicting the effects of ET on water levels, particularly when the water table crosses the land surface regularly as is the case here. Phreatophyte water use will cause a dramatic acceleration of water table decline when the water is at and below the soil surface, but the authors present a conceptual model (Fig. 2) where water table declines actually slow post-inundation. I believe this is because of the decline in ET with the onset of water stress, but it is incorrect to conflate ET with water table changes because of the vertical dynamics of specific yield; as such, the shape of the curve relating stage to change in stage is not generally linear (see Tamea et al. 2010 for the best description of this). Indeed, the actual data (Fig. 4b) run counter to the slope changes implied by the conceptual model, indicating accelerating decline

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when the water table drops below the soil elevation (though far smaller than would be expected if the hydrology were governed by local processes only...see below), and a subsequent slowing of that water table decline as phreatophyte water use declines. For what it's worth, it's possible to use diurnal water level variation alone (provided high quality pressure transducers are available) to directly measure ET upto the point that vadose zone water becomes an important component of total water use, and can be informative even after that point (e.g., Loheide et al. 2005).

3) The fact that the water level declines are not discontinuous (as was observed in Tamea et al. 2010) suggests mixing of water level signals over heterogeneous terrain. In order for this to occur, there has to be some manner of lateral connectivity between the water bodies and adjacent uplands. While I realize that it's beyond the scope of measurements in the current paper, the authors don't mention groundwater fluxes (local or regional) as potentially important controls on water level changes. I would submit that the recession rate (1.8 cm/d) is sufficiently high compared to PET rates that some combination of groundwater losses and specific yield effects must be occurring.

4) Billing the method as general to tropical wetlands seems a little ambitious given the relatively small geographic scope of the actual measurements. While I found the utility of the Turc method to be compelling (at least vis-à-vis data from a single pan), especially in comparison with more data intensive approaches, I would be pretty cautious about that generalizing to all tropical settings. I would be even more cautious about the generality of the apparent assumption that local and regional groundwater flows are not relevant to local water table dynamics (animated by the fact that I work in a non-tropical area where groundwater exchange is paramount). On this last point, it seems relevant in the site description to provide some rationale (e.g., based on sediment characteristics) that could justify the omission of groundwater as a control on water level variation.

Minor comments: - The lack of soil moisture data is not "profound" (pg 4021). Perhaps drop the modifier. There is simply a lack of soil moisture data. - The end of the discus-

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sion suggests that the your model can be transferred to other tropical wetlands. This is not the place to articulate the utility or generality of the model since it's not yet been presented or tested. That sentiment can go in the abstract, but in the intro it makes it seem like a foregone conclusion. - It would be helpful to provide citations on the Bowen ratio method, and perhaps even a sentence describing how it works. There are many in the hydrologic community that remain unconvinced that it provides adequate performance (certainly not to sub-millimeter resolution as implied by the significant digits reported on page 4029). - The authors state that AET depends on the duration of the dry season (pg 4031). I believe that the deviation between PET and AET is a more precise statement of what the dry season duration controls. - The parenthetical statement on the last line on page 4032 makes no sense to me. - The authors assert a goal of a "process based model" (pg 4034), but I fail to see how that was achieved. The model is strictly empirical, with the empirical parameters fitted from a small data set. I believe they have been successful in showing the utility of simple empirical models, but not to develop a process-based model. - The data in Fig. 8 would be more compelling (to me, anyways) by showing explicitly the strong covariance between total E and hydroperiod (annual duration of inundation). This relationship is definitely inferred from the graph, but a pairwise plot (total ET on the y-axis, hydroperiod on the x-axis) would be clearer.

References: - Hill AJ, Neary VS. 2007. Estimating evapotranspiration and seepage for a sinkhole wetland from diel surface-water cycles. *Journal of the American Water Resources Association* 43: 1373–1382. - Loheide SP II, Butler JJ Jr., Gorelick SM. 2005. Estimation of groundwater consumption by phreatophytes using diel water table fluctuations: A saturated-unsaturated flow assessment. *Water Resources Research* 41: W07030. DOI: 10.1029/2005WR003942 - McLaughlin DL, Cohen MJ. 2014. Ecosystem specific yield for estimating evapotranspiration and groundwater exchange from diel surface water variation. *Hydrological Processes* 28:1495-1506 - Sumner DM. 2007. Effects of capillarity and microtopography on wetland specific yield. *Wetlands* 27: 693–701 - Tamea S, Muneeppeerakul R, Laio F, Ridolfi L, Rodriguez-Iturbe I. 2010. Stochastic description of water table fluctuations in wetlands. *Geophysical Research*

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