Hydrol. Earth Syst. Sci. Discuss., 11, C1620–C1626, 2014
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Hydrology and Earth System Sciences Discussions
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Interactive comment on "Inundation and groundwater dynamics for quantification of evaporative water loss in tropical wetlands" *by* J. Schwerdtfeger et al.

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Received and published: 30 May 2014

We would like to thank anonymous referee #2 for the valuable review. Her/his comments significantly helped to improve the quality of our study and the manuscript. Below we provide our responses to the reviewer's comments:

1) Speaking about the heterogeneity of tropical wetlands in the outline of the paper, we were referring to the heterogeneity of the inundation process of tropical wetlands rather than on spatially varying properties such as elevation or soils. However, inundation dynamics are a result of the variability of these properties and they are therefore implicitly

included when we monitored the heterogeneity in inundation dynamics. This is considered in our model using water table measurements (inundation and groundwater) of different locations in the study area. We are aware that for modeling evaporation for a single location with detailed meteorological variables there would be more reliable ET models available requiring usually a higher data input. On the one hand, these data were unfortunately not available for our study area. On the other hand, the aim of the present study was not to develop the most realistic and process-based model for estimating ET, but rather to develop a model that is capable of simulating ET that captures the dominant controls (inundation dynamics) based on limited data, which is typical for many tropical wetlands. We tried to develop an approach, which can be transferred to a larger scale in remote areas where data is usually scarce. Our simplified approach will contribute important knowledge about the water balance of tropical wetlands for an estimation of evaporation losses. We will emphasize this aim more clearly in the revised manuscript. Regarding the decline and recovery of the slopes, we argue that they are based on measured data in the study area, which could also be validated with a second groundwater probe.

2) The referee is right. We wanted to present a conceptual model for the general procedure of drying and rewetting and did not consider the measured slopes. When we developed our conceptual model we did not include changing slopes for the drying and rewetting phases of the groundwater in relation to the surface water level slopes, nor did we present a model considering absolute scales and units. Based on this valuable comment by the reviewer, we will provide a new figure (Fig. 1 of reply will replace Fig. 2 of manuscript) of our conceptual model taking into account the acceleration of water table decline. Furthermore, this acceleration can also be observed in our measured data, as mentioned by the referee. Data for our study area were not available for considering the water consumption by Phreatophytes and/or specific yield in open water/soil/sediment. Thus these processes could not be included into the model structure, but we will add a discussion about these additional sources of uncertainty to the revised manuscript.

3) We agree with the referee. The recession rate is probably not only due to evaporation losses but also due to groundwater losses and specific yield effects. Although it was beyond the scope of our measurements, the referee correctly points out that it is necessary to mention these two additional impacts on the recession rate in the revised manuscript, which we will provide.

4) Considering the modified Turc method for estimating evaporation losses for our study area we could show that this model provides reasonable results for our study site that can be seen as representative for the Northern Pantanal wetland. The referee is right that this does not necessarily mean that the same PET model provides compelling results for other remote tropical wetlands. We did not consider the modified Turc method as the best model for all tropical wetlands, but we are convinced that our general approach is applicable in other tropical wetlands as well. Of course, the best model, either Turc or another PET model, must first be selected and validated for a specific area. The choice of the PET model also depends on the data availability. As mentioned in section 3, we agree that groundwater flows are relevant for the local water table dynamics, which was demonstrated in Schwerdtfeger et al. (2013). We will include a discussion about this important aspect in the revised manuscript.

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 sentence will be rephrased according to the referee's suggestion.

The end of the discussion suggests that 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. - -> The referee is right. We will discard the last sentence of the introduction section and include the statement in the abstract.

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). -> We will provide citations for the Bowen ratio methods with a sentence describing it. Furthermore, the referee is absolutely right concerning the sub-millimeter resolution. We will change it to millimeter resolution.

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 referee is right. To describe the dry season as the period that controls the strength of the deviation of AET from PET sounds more reasonable considering the scope of our study. The manuscript will be changed accordingly.

The parenthetical statement on the last line on page 4032 makes no sense to me. -> The statement will be rephrased to: "Due to the lack of local class A pan recordings for our study area, we used class A pan data from the nearest station (80 km north of the study area) based on the assumption that the climate classification still is the same. For both locations, the climate classification after Kottek et al. (2006) is defined as Aw, which means equatorial savannah with dry winter and minimum precipitation < 60 mm".

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. -> In our model structure we considered the impact of inundation dynamics as well as water table changes on evaporation losses from the wetland. Both are important processes of the wetlands water balance. Therefore, we claim that our model is more process-based than other empirical methods.

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. -> Thanks to the referee #2 for this valuable suggestion. This way the plot will be much clearer. We will change the graph as suggested (Fig. 2 of reply will replace Fig. 8 of manuscript). Accordingly, the term “hydroperiod” will also be used in the text. Figure caption: Relationship between total AET [mm] and hydroperiod (annual duration of inundation [days]) for studied water bodies (permanent, ephemeral, floodplain)

References cited in this reply

Kottek, M., Grieser, J., Beck, C., Rudolf, B., and Rubel, F.: World map of the Köppen-Geiger climate classification updated, Meteorologische Zeitschrift, 15, 259-263, doi: 10.1127/0941-2948/2006/0130, 2006.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 4017, 2014.

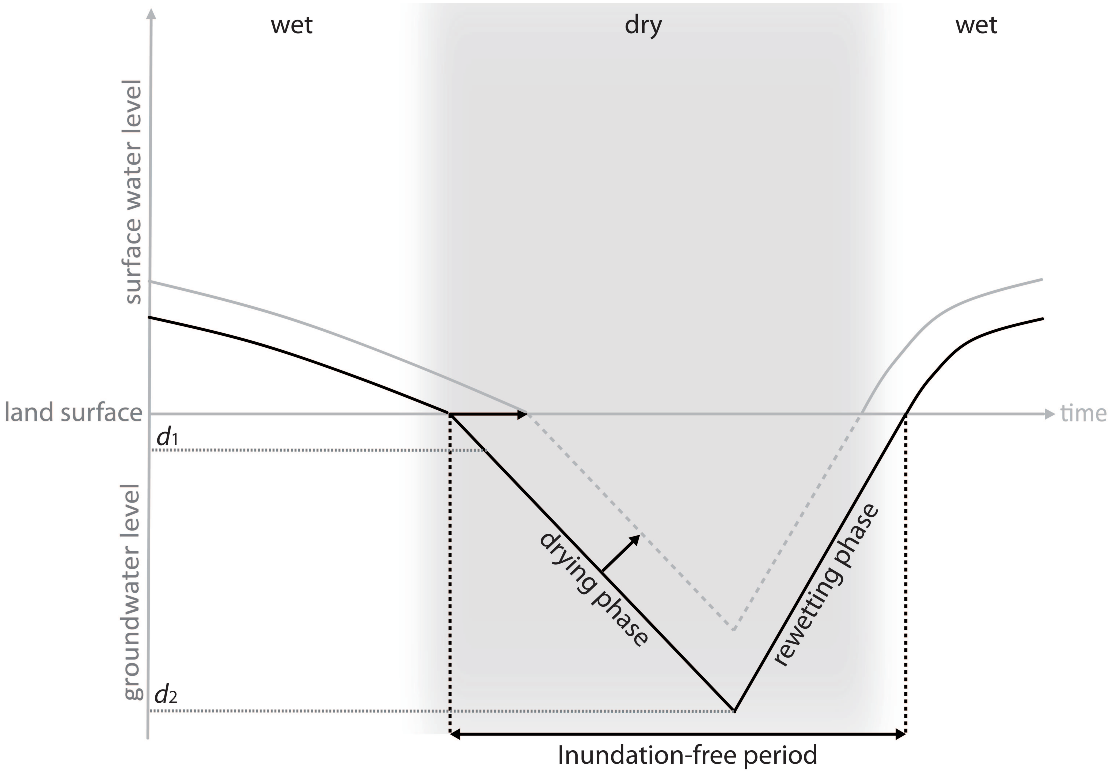

Fig. 1.

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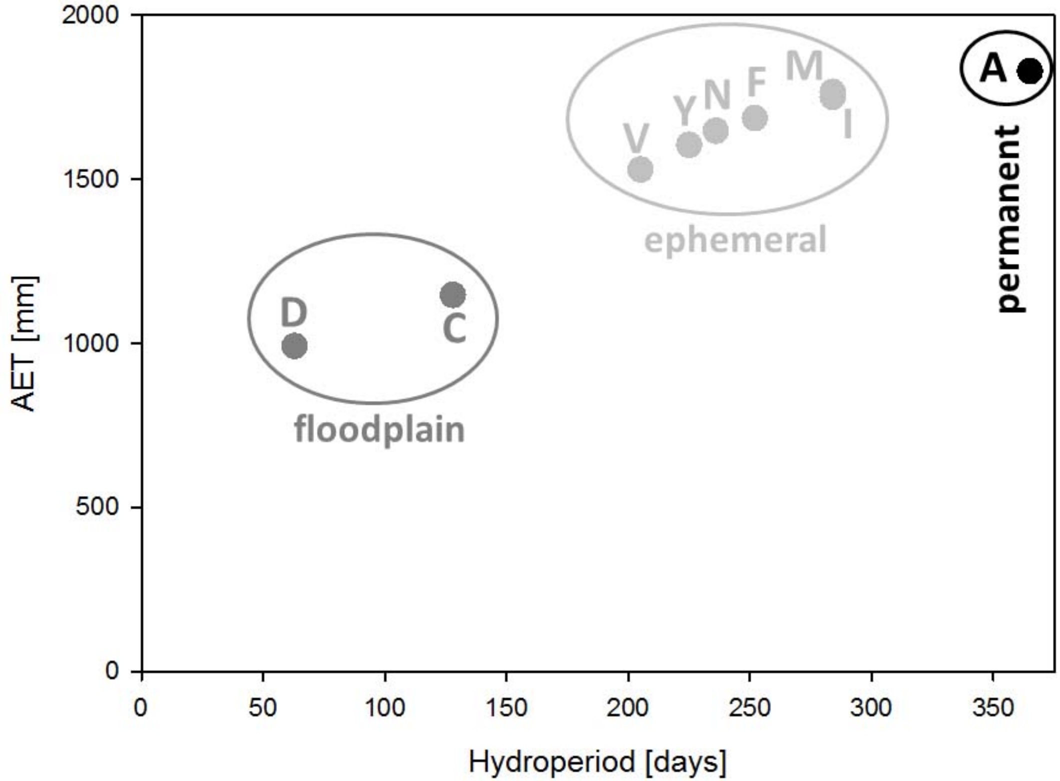

Fig. 2.