"Inundation and groundwater dynamics for quantification of evaporative water loss in tropical wetlands"

Dear Dr. Liu,

Thank you for inviting us to revise our manuscript responding to the valuable comments of the reviewers. We considered all of the reviewers' suggestions and changed the manuscript accordingly.

Please find below the list of specific responses to the individual points of the reviewer's suggestions. Corresponding changes in the new manuscript are highlighted with coloured text.

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Comments to Reviewer 1

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General comment
The paper clearly and concisely documents methods for correcting several potential PET equations to actual AET, based on "extinction depth" (water distance below land surface) concepts for a tropical wetland (Pantanal) in South America. I admire the analysis in the paper…the results and conclusions are clear, concise, and well-structured. In my opinion, however, the manuscript does not represent a substantial contribution to the ET science, nor does it present new concepts, ideas and methodology. PET equations are commonly limited to AET using extinction depth / water availability functions (see Harbaugh 2005; ET models in German 2000; Shoemaker and Sumner 2006). The data and results generated by the analysis could qualify as novel and new, as they reveal subtropical wetland PET and AET rates, as well as discovery of the Turc equation as the best estimator. However, the methods employed to compute results and draw conclusions are not entirely valid, in my view, as I'll attempt to explain in more detail.
→ *The main contribution of our work is that we were able to apply and evaluate an approach to simulate first and second stage evaporation in a data scarce region, which needs only water level measurements to transfer our evaporation results to other locations in the wetland. Approaches presented in the literature so far use either meteorological variables, which are more costly and labor intensive to collect than water level measurements and/or they need soil moisture data, which are often not available in tropical wetlands. For the first time in this difficult-to-access area, our data set of myriad water bodies enabled us to shed light on the differences in ET behavior among a wide range of water body types and different inundation dynamics. We agree with the referee that the concept of using the extinction depth for determining water available in ET science is not a completely novel concept. However, using measured water levels of different types of water bodies in the Pantanal wetland, which was not done in the mentioned studies, enabled us to focus on the differences in evaporation losses of a number of water bodies and not only one location. That way we could show the importance of separately considering first and second stage evaporation.*

The paper uses pan ET rates as the "truth", or closest approximation to PET. The pan locations are not clearly labeled on Figure 1, and some meteorological conditions surrounding the pans do not resemble meteorological conditions in the wetland study area. Specifically, Section 4.1 states R2's between meteorological conditions at the pans and study area "were 0.55, 0.84 and 0.38" for Ta, RH and v; respectively, on a mean monthly basis. Pan RH is similar to your study area; however, Ta and v are different. Furthermore, R2's would likely decrease for each of these variables at weekly and daily time scales. I am convinced the corrected PET equations are a good estimate for pan evaporation limited by water depth below land surface. I am not convinced the corrected PET

equations represent subtropical wetland PET and AET for water bodies (Figure 1c) in the Pantanal, given the supporting documentation and statistics (monthly R2) in the analysis.

→ *We are aware that the class A pan located 80 km northeast of the Pantanal study area is suboptimal for comparisons with local measurements. However, it is the only data source available in the region to perform our analysis. The reader should also be aware that predictions within the Pantanal (or any other remote tropical wetlands) often require the use of meteorological data obtained from distant locations. Our analysis shows that there is some correlation to the meteorological variables at the study sites on a monthly time scale. We consider this weak correlation better than no data at all and sufficient to proceed with our analysis of the seasonal evaporation characteristics. The following section of these addressed comments also refers to this topic and explains the rationale for using mentioned class A pan data in more detail. In addition, the newly provided Figure 1 in the modified version of the manuscript includes the detailed location of the class A pan.*

Consider calibrating the PET equations to Bowen ratio AET estimates. The Bowen ratio station is at water body C (Table 3) in the Pantanal, according to the manuscript. Calibration could cover both "first and second stage (dry)" evaporation since the PET equations will differ from Bowen ratio AET not only when the sites are dry but also flooded. Novel corrections could be derived based on air temperature, VPD, RH, etc…and compared to the commonly applied "extinction depth" correction. Furthermore, resulting ET estimates will be more reliable given the location of the Bowen ratio station in wetland water body C. I suspect you can achieve your goal of deriving a "first and second stage" ET estimator for Pantanal wetlands with limited data requirements, using this modified strategy.

→ *We chose water table measurements for correcting PET with a simple water availability model, because (1) according to Shoemaker & Sumner (2006), the water table was the data type with the most correction ability at sites that become dry during some parts of the year, (2) water table measurement is possible at various locations in the study area at relatively low costs, and (3) they can also be derived by satellite data allowing for large scale model applications (e.g. MODIS, Peng et al. 2005 or Feng et al. 2012). We did not consider site specific correction factors such as Ta, RH, VPD, etc. stated in the literature (German 2000, Shoemaker & Sumner 2006) since their measurement would require more time and expensive installation for meteorological stations. Necessary data for calculating these correction factors were only available at three of our study locations. Further measurements of meteorological variables at the other locations were not undertaken due to operational costs and no possibility for long-term maintenance during the two years of data recording at the remote locations. We want to clarify that our analysis already included first and second stage evaporation. We did not mention it in the submitted manuscript and explicitly included this information now in the revised manuscript in section 3.2. We are aware that the Bowen ratio station inside the wetland would be more reliable for evaporation estimates if there had been enough data available. There were more than three years (38 months) of class A pan data available for choosing the best PET model (modified Turc) for our study area. We then tested the Turc model by evaluating its results with the Bowen ratio station inside of the study area for the period with available data (26th January until 8th May 2007). With a 3 % deviation from the measured cumulative Bowen ratio results the modified Turc model was still the third-best model with a MAE of 0.46. Since for that evaluation the Bowen ratio station provided not even four months of data for the wet season at location C, we decided to rely on 38 months of class A pan data, even given its location north of the Pantanal.*

Specific comments
Use active voice when writing. The paper needs a global edit to address this issue. For example, the first sentence in the Abstract could be rewritten as "Characterizing hydrologic processes within tropical wetlands is challenging due to their remoteness, complexity and heterogeneity."

→ *A global edit of the current manuscript in terms of active/passive voice has been conducted with tracked changes in the revised manuscript. The first sentence of the abstract was changed according to the reviewer's suggestion.*

Abstract Line 10: I disagree with the statement that "As yet, no adequate method exists for determining second stage evaporation without soil moisture data, which are usually unavailable for remote tropical wetlands." A similar statement is made on lines 22 and 23 in the Introduction. German (2000) and Shoemaker and Sumner (2006) both present several PET corrections/models that were adequate for estimating first and second stage AET, while not requiring soil moisture data.

→ *The referee is right. According to our statements above, we included mentioned references (German 2000 and Shoemaker & Sumner 2006) in the state of the art in the introduction. We reformulated our description of the research gap we address in our paper, and focused on presenting our approach to calculate second stage evaporation in remote tropical wetland areas using only measured water levels at different types of water bodies and additionally allowing for a transfer of simulated AET to other locations in a data scarce environment. The corresponding sentences were also changed in the abstract.*

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Comments to Reviewer 2

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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).

→ *Regarding 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 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) across a range of wetland typologies based on limited data, which is typical for many tropical wetlands. We tried to develop an approach that 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. In the revised manuscript this aim was reformulated more clearly in the last paragraph of the introduction section. Regarding the decline and recovery of the slopes implied in Fig. 7, 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 basic model is somewhat problematic because it neglects the water table effects of specific yield (i.e., drainable porosity, which is ~ 1.0 in open water, and ~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 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 up to the point that vadose zone water becomes an important component of total water use, and can informative even after that point (e.g., Loheide et al. 2005).

→ *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 provide a new figure (Fig. 2) 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 these additional sources of uncertainty were mentioned in the revised manuscript in section 5.2.2, where wetland vegetation was already explained as an uncertainty due to simplification of the model structure.*

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.

→ *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 provided in the 4th paragraph of section 5.1.*

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.

→ *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 to select a reasonable PET model and apply it to the water level dynamic 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. These statements were included in section 5.3 in the revised manuscript. We agree with the reviewer that groundwater flows are relevant for the local water table dynamics, which was demonstrated in Schwerdtfeger et al. (2013) and mentioned it in the 4th paragraph of section 5.1 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.

→ *Since we reformulated our research gap the mentioned formulation does not appear in the new manuscript.*

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 discarded the last sentence of the introduction section. We did not make corresponding changes in the abstract since a similar statement there is given already.*

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 provided citations for the Bowen ratio methods with a sentence describing it in section 3.2. Furthermore, the referee is absolutely right concerning the sub-millimeter resolution. We changed it to millimeter resolution in the end of section 4.2 in the revised manuscript.*

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 was changed accordingly in the 3rd paragraph of section 5.1.*

The parenthetical statement on the last line on page 4032 makes no sense to me.

→ *The statement was rephrased in section 5.2.1.*

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 method and*

included our definition of process-based in the last paragraph of the introduction in the revised manuscript.

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 is much clearer. We changed the graph as suggested (Fig. 8). Accordingly, the term "hydroperiod" was 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).*

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We would like to express our appreciation to the Reviewers and HESS Editorial Board for their time and efforts on this manuscript.

Sincerely yours,

Julia Schwerdtfeger, Mark S. Johnson, Eduardo G. Couto, Ricardo Santos Silva Amorim, Luciana Sanches, José H. Campelo Júnior and Markus Weiler