OVERALL COMMENTS

This study's objective is to quantify the water budget of four rainwater harvesting (RWH) tanks and assess the combined impacts of the RWH tanks on the hydrology of a small budget. The authors use field measurements to attempt to quantify inflows and out- flows, and conclude that the RWH tanks significantly decrease runoff and increase groundwater recharge over the catchment. As the authors note, the hydrology of RWH systems is poorly understood (e.g. Glendenning et al., 2012), and therefore the accurate quantification of the water balance at both a tank and catchment scale would be a valuable scientific contribution. I feel that HESS is an appropriate venue for this study, which studies the hydrological impacts of small-scale anthropogenic modification at both local and catchment scales, with relevance for region, national, and global agricultural water use.

Overall, the paper is well written, interesting, and sheds light on an important topic.

Thank you.

However, I have some concerns regarding the authors' estimation of evapotranspiration and groundwater exchange, specifically related to the specific yield (Sy) parameter, and the potential errors that this may introduce to tank- and catchment-scale results based on this evapotranspiration and groundwater exchange. For this reason, I suggest the editor consider the revisions suggested below prior to making a final decision on this manuscript.

We appreciate the reviewer's comments regarding the specific yield parameter, S_y, and have addressed each of the specific comments below accordingly. In doing so, we believe the paper's findings regarding groundwater and ET contributions to tank water budgets are better supported.

SPECIFIC COMMENTS (major)

The bulk of the authors' results are based on a modified version of the White method, which is widely used for estimating the proportion of evapotranspiration which comes from groundwater recharge. As noted by several studies (Loheide et al., 2005; McLaughlin & Cohen, 2014), accurately estimating specific yield (Sy) is critical for accurately quantifying fluxes using the White Method. Sy is a particularly important parameter in this study, as it controls both the estimated evapotranspiration (ET; via Eq. 1) and groundwater exchange (GE; via Eq. 2)

In this study, the authors assume Sy to be a constant 1.0, and mention some potential problems with this assumption, including referencing a study by McLaughlin & Cohen (2014) (hereafter M&C). M&C also find that using a constant value of Sy can lead to overestimation of ET (and, by the same logic, GE in this study). For example, in Figure 5, the authors note that calculated ET rates are only reasonable when inundated area is >25% of the maximum observed inundated area. It appears that areas with unreasonable values (to the right of dashed lines) represent

~ 25-50% of the total time monitored, and include ET estimates up to 30 mm/day (see Tank 2). While seasonal averages compare favorably to PET, as noted in section 4.1.2 of the text, estimate appear to get less and less accurate as the growing season progresses. Because Eq. 1 and Eq. 2 are based mostly on the same parameters, this indicates that during the periods when ET estimates are unreasonable, GE estimates would also likely be off, potentially by a factor of 2-3x.

We fully agree that using a constant value of S_y can lead to an overestimation of ET (as a rate per unit area; see below) at low stage, and state such in the text, as the reviewer points out. However, ET can also be amplified at low stage due to an oasis effect, in which advection of dry air from exposed areas can increase ET rates in flooded areas beyond typical values (Drexler et al. 2004, Paraskevas et al. 2013), as we have also pointed out in the text, lines 365-369). It is difficult to say which effect dominates, but as described below, we don't think that the effect of Sy is significant in our study.

Regarding S_y effects, it should be noted that the mechanism that creates lower S_y at lower stage is rapid (almost instantaneous) equilibration with belowground water levels in exposed areas that are adjacent to flooded areas. The spatial extent of this equilibration is determined by the hydraulic conductivity of the soils. In McLaughlin and Cohen (2014), the sites (North Florida cypress dome wetlands), were dominated by highly conductive sandy soils (Ksat = 1.13 – 6.42 m/day), likely expanding the area of equilibration. In contrast, our study area in South India is characterized by clay-dominated soils. We performed two slug tests, one in the Alfisol (tank 1) and one in the Vertisol soil (tank 3), and Ksat was estimated to be 0.17 m/day in the Alfisol and 0.024 m/day in the Vertisol soil. Note that these values are compatible with other reported Ksat values for Alfisol and Vertisol soils and are 1-2 orders of magnitude less than those for the M&C sites. These very low Ksat values are indicative of clay soils in which rapid equilibration (if any) is likely limited to small edges. We therefore believe that the effect of equilibration on overall flux values would be negligible, thus making an Sy value of 1.0 a reasonable assumption. We will add these details in the text.

Regardless of the spatial extent of equilibration, because of reasons articulated below, the losses in surface water that occurs due to ET and GE are still valid and accurate components of the tank water budget. If exposed areas are equilibrating with flooded areas, then the measured surface water decline will include both the direct flux (ET or GE) in the flooded area (Sy = 1) and the subsidy (indirect flux) to equilibrate those exposed areas where Sy < 1. However, the loss in surface water depth is still loss due to a particular flux (ET or GE), just over a greater footprint (i.e., direct fluxes in flooded areas + indirect losses to equilibrate flux-driven declines in adjacent areas). Therefore, when we convert ET and GE depth losses to surface water volume losses using stage-to-volume relationships, the estimates are accurate, and useful for discussing the proportions of stored surface water lost due to various water budget components. We will include new text to support our reporting of ET- and GE-induced losses (both as depths and volumes) of tank surface water storages.

This potential issue casts some doubt over the authors' other interesting results. Figure 6 shows a general decrease in groundwater exchange over the course of the growing season which is very interesting, particularly the shift from outflow to inflow seen at Tank 4. However, this shift may be driven by increasing overestimation in GE over time, which (as discussed above) is likely due to error in the estimation of specific yield, rather than actual increases in total ET or GE. I feel that results from the periods during which estimates of ET and GE are unreliable should not be included in subsequent graphs. Or, at the very least, it

should be noted (perhaps by shading in the background of plots) the periods during which Sy estimates (and therefore ET and GE estimates) are inaccurate.

Here the reviewer suggests that that our findings of decreasing GE outflow with decreasing stage (Figs 6 and 7) and the subsequent switch to inflow may be an artifact of not correcting for S_y. However, if <u>actual</u> S_y decreases with decreases in stage, then using an assumed value = 1.0 (like we did) would lead to an <u>overestimation</u> of GE outflow at lower stage. If anything, then, our results may actually be underestimating the extent to which recharge decreases over time.

Regarding the switch from outflow to inflow noted for Tank 4, the only way in which incorrect Sy estimates can lead to a switch in GE from outflow to inflow, is if Sy*24h < the sluice outflow (GE = Sy*24h – S0). However, the switch in Tank 4 occurs much after the sluice outflow stops on 12/22, and thus this would not be a valid reason for the switch.

Finally, as we describe before, (a) Sy decreases due to equilibration are expected to be minimal in our study due to low conductivity soils and (b) Regardless of system S_y, it is valid to report losses of surface water to GE and ET because it is a loss that is happening from the open water.

M&C were able to correct for inaccurate Sy estimates at low tank water levels using an interpolation between estimated soil specific yield and open water specific yield. It is not mentioned whether the authors attempted this correction, but it may improve the reliability of both ET and GE estimates. Considering that the authors generated a stage-inundation relationship as part of their methodology, they should have all the necessary input data to carry out this correction and potentially improve their results. Even better, or if the Sy of the local soils is unknown, calculations could be carried out with a range of Sy values, which would also improve the study by providing a rough estimate of the uncertainty associated with the authors' estimated water balance

The method M&C used for correcting inaccurate Sy estimates is based on the assumption of rapid equilibration between inundated and non-inundated areas, which is valid for the sandy soils in their study, but does not hold for the clayey soils at our site (see response to the first comment for soil hydraulic conductivity values).For our soils, we contend, based on the low hydraulic conductivity values, that there will be minimal equilibration across flooded and non-flooded areas (the mechanism to reduce Sy). The stage inundation method and the soil Sy methods that the reviewer describes is based on using an estimate of equilibrated area, which M&C assumed to be equal to the entire wetland area, an assumption that does not hold for our much larger and much less conductive tank bed. Thus, the measured stage-inundation relationship cannot be used for correcting Sy values. Of course, there are data-intensive approaches to estimate site-specific stage-Sy relationships (see M&C); however, our dataset precluded such analyses. We will include some brief text stating these possible methods and limitations. We will clarify this in the revisions.

We are confident, however, that our Sy values do not require significant correction due to the much lower K soils, as described above. We also contend that our reported surface water losses as depths and volumes are the actual and most relevant losses of water available for irrigation (be it direct and/or indirect flux-driven losses). We will make both of these points clearer in the revisions.

SPECIFIC COMMENTS (minor)

One major assumption of the authors' methodology is that there is no surface inflow to the RWH tanks on days when it is not raining, meaning both overland flow and subsurface runoff occurs over very short time intervals. This should be stated more clearly in the methodology.

We will do this

Pg. 12125, line 12: "variables" should be "variable"

We will do this

Pg. 12131, line 19: Section 4.3 is referred to, but does not exist – I believe this should be Section 4.1.2? A scale bar should be added to Figure 1a

We will do this

Figure 2b is the same as Figure 2 in Van Meter et al. (2014) ES&T – this should be cited appropriately in the figure caption.

We will do this

Section 4.2.3 refers to Figure 11 several times – I believe this should be Figure 10.

Thanks for noticing. We will make the change.

I may be interpreting the x-axis on Figure 3 incorrectly, but it looks like it goes from 0:00 (midnight) to 12:00 (noon) to 0:00 (midnight) – meaning a single day. However, as drawn, it includes two "Night" periods, one beginning shortly after noon. Please check this axis.

Thank you for noticing. There was an error in that axis, and we have corrected it now.

I found myself a bit confused by the inputs and outputs used for each step of estimating the water balance components, more so for the catchment-scale scenario study than for the individual tanks. I suggest that the authors include a simplified diagram (boxes and arrows) showing the calculation of each component of the water balance, and then how they are estimated in both the NT and WT scenarios

Thank you for the suggestion. We will add a schematic.