Responses to Editor & Anonymous Reviewers

Editor Letter With Responses:

Dear Editor:

The overall opinion of the 4 reviewers is that paper is well written, interesting, and appropriate for the journal. I agree with the reviewers. They have also made useful suggestions for improving the manuscript which the authors should incorporate.

> **Thank you for the comments. We have addressed the suggestions of the reviewers, as described below.**

I think most of the suggestions made by the reviewers are relatively minor and something that the authors can carry out, as they have already indicated that they will do. In summary, these are the main areas that I have identified (and the authors agree on these based on their responses) for modifying the manuscript:

-Reviewers 2 and 4 have mainly very minor comments. The authors should incorporate these in the revised manuscript, as they have already indicated.

> **We have done this**

-While reviewers 1 and 3 have mostly minor comments, they also have some methodological concerns. I think, however, that the authors have explained in their responses some of these concerns and have indicated modifications to make to the manuscript to make more clear their methodology and address the concerns of reviewers 1 and 3. I suggest that the authors implement what they have suggested to do here.

> **We have made the modifications and addressed the comments.**

-The authors should implement the specific or minor comments made by reviewers 1 and 3.

> **Done**

-Reviewer 3 is also concerned about the wordiness of the paper. The author should keep this in mind in making their modifications.

> **We have streamlined the manuscript**

-Additionally, reviewer 3 suggested that the literature review be expanded to include more references from peer-reviewed journals. I agree with the reviewer here, and the authors do as well.

We have added many more references

-Reviewer 3 also makes some suggestions to tables and figures in the manuscript that should be incorporated.

This has been done

Reviewer #1 Comments With Responses:

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 have added these details in the text (lines 328-436).

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 have included new text in the manuscript to support our reporting of ET- and GE-induced losses (both as depths and volumes) of tank surface water storages (lines 328-436, lines 556-643).

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 actual S_y decreases with decreases in stage, then using an assumed value = 1.0 (like we did) would lead to an overestimation 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.**

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). Please see the revised text regarding the Sy estimates (lines 328-436, lines 556-643).

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.

The methodology does assume absence of overland flow on days when it is not raining, but not an absence in subsurface flow. The subsurface flow is the groundwater exchange term in our equations. We have now stated this more clearly in lines 278 – 281.

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

In the text, the phrase is "between the two variables," so we believe that "variables" is correct here.

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

Sorry for the typo -that should be Section 4.1.2 – we have made that change. We have also added the scale bar.

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.

Done

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

Thanks for noticing. We have made 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 have added a schematic (Figure 4).

Reviewer #2 Comments With Responses:

Manuscript is written well, and acceptable for publication except few comments.

Thank you.

1. In the text, Figure 11 has been cited (Line 10; page 12143), while only total 10 figures have been provided (page 12165). Please correct.

Thanks for noticing. We now use the correct figure numbers (and one new figure has been added).

2. Line 27: After D. L. McLaughlin, there should be a full stop (.). Prior to Data analysis, no breaks.

Done

3. Reviewer suggests authors to revisit the manuscript to correct syntax errors.

Done

4. In general, manuscript is written well, and provide in-depth insight of rainwater harvesting system. 5. Reviewers also suggest adding some additional references: Rockstrom et al. (2009); Rockstrom et al., (2002); Pandey et al. (2013); Pandey et al. (2011); Va der Zaag adn Gupta (2008); Mialhe et al. (2008). These all studies are very relevant to this articles, and deserve citation. Mialhe et al. (2008) did explore the a similar research in South India.

Thank you for the suggestions. We have added the references.

Reviewer #3 Comments with Responses:

I. Summary of the review
This work attempts to increase our understanding of rainwater harvesting tank systems used in regions where biophysical and sociological factors are relevant. In particular authors focus on the water-exchange dynamics of these systems both at tank and catchment scales. The work is of interest and suitable for this journal as it deals with a relevant topic, and clearly has the potential to contribute to the science of socio-hydrology.

Thank you

Nonetheless some issues need to be addressed prior to acceptance for publication. In particular I have certain concerns with some methodological aspects, which are described below.

We have addressed these comments below.

II. Major comments:
1. The application of the White method: I think there are two issues here that need to be better clarify, as the method is central to the research and the results. In particular, Eqs. 1 and 2 seem to imply that sluice flow takes place only during the day. Is that the case? Is it a valid hypothesis? Could the term -S_o be in equation 1 instead of equation 2? The manuscript mentions that at certain points ET seems to reach very high values. I am wondering if this is partially due to how S_o was considered in Eq2.

Sluice flow does indeed take place both day and night. We therefore make an assumption of 24-hour sluice outflow in our calculations (see line 265). The h is the hourly nighttime drop that includes both sluice outflow and groundwater exchange (GE). Therefore, when h_n is multiplied by 24, we get the total daily drop due to sluice and GE. The remaining flux is then assumed to be ET, as captured in equation 1. To clarify, we have added text to define h_n (the nighttime slope) as including both GE and S_o (lines 306-320).

Moreover a value of S_y = 1 was adopted, whereas section 4.3 is mentioned to be a section where "important caveats regarding this assumption" are presented. There is no section 4.3, and I do not see a strong analysis elsewhere in the document addressing the assumed value. Indeed the specific yield concept applied to these equations is not totally clear to me, and I think it could be better presented in the text.

We apologize for the confusion. The section number referenced here should be 4.1.2, not 4.3. This will be corrected. We will also add text to sections 4.1.2 and 4.1.3 to better address our use of Sy = 1 and related caveats to determine ET (4.1.2) and groundwater exchange (4.1.3) as depth and volume losses of

surface water storage. For more details regarding this new text, see specific responses to comments by Anonymous Reviewer 1.

2. The approach to analyse the no-tank scenario: Authors mentioned the "Strange method" to simulate this scenario, which I think it is not well known for the community. Furthermore, the reference provided (Shanmugham and Kanagavalli, 2013) seems to be a local publication in India. Nothing is said about the method despite its application is critical, as important results and conclusions are based on the simulations of the system with no tanks using the model. How does it work? Is there any bold assumption? How should the reader approach the results based on potential limitations of the model. I think a better explanation of the model is critical.

We have added the following details.

The Strange method is an empirical method that is widely used by government departments in India, including the public works department of Tamil Nadu, for computing the runoff yield from the catchments of irrigation tanks and small reservoirs (Latha et al. 2012). In this method, daily runoff is calculated as a percentage of daily rainfall, based on tabulated values in which % runoff is expressed as a function of (a) rainfall on that day, (b) antecedent rainfall conditions, and (c) catchment characteristics (Shanmugham & Kanagavalli, 2005). For example, with a 50-mm rainfall, runoff could range from 10% for a dry catchment to 34% for a wet catchment, with the catchment condition (wet, damp or dry) being determined based on the days since last rainfall and the intensity of the preceding rainfall events. The Strange Method has been shown to provide results comparable to those obtained with the more commonly used SCS Curve Number method (Latha et al. 2012), but is more representative of the south Indian conditions that are the focus of our study.

Moreover, the classification of the landscape with three domains seems to be a very specific decision that could be better supported, both through a clearer rationale and a sensitivity analysis (for example, recharge for domains 2 and 3 are very specific. Particularly for the domain 3, a very specific value of 17% of rainfall is used).

Thank you for the comment. We now provide a figure (Figure 4) to clarify the three domains and the ways in which recharge is estimated for these domains. The figure also clearly delineates the catchment boundaries for the nested catchment water balance calculations. Our goal in this section is not to develop an exact model for the watershed, but to demonstrate, quantitatively, that tanks provide a significant recharge benefit. The three domains characterize the three areas of the watershed in which we expect the recharge function to be significantly different – the tank bed area, the command area and the rainfed catchment area. This is because the significant irrigation in command area leads to increased recharge, recharge

is higher in tank bed due to standing water, while in the rain-fed catchment area, recharge would be the typical values based on the prevailing land use. The 17% number is based on studies in the area with similar land use. Details regarding the calculations are provided in lines 472-477). We do not have enough information, or a detailed model, to do a proper sensitivity analyses in this study, but we hope to include such analysis in future work.

3. In section 4.2.3 it is not clear whether the "wasted" water from the different tanks is indeed wasted. Sluice flow from one tank could enter the next ones and be used for irrigation. The analysis seems to be only local in this regard, although the authors have point out the relevance of a systemic approach. Please clarify.

Thanks for the comment. Indeed, sluice outflow from one tank can enter the downstream tank via return flows, and/or recharge the groundwater. We have modified this section accordingly.

4. In my opinion, the manuscript is a little bit wordy. When describing my minor comments, I try to identify some paragraphs whose size can be reduced, although throughout the document there are opportunities for reducing the number of words. In particular I think the conclusion section is to long as it dedicates many lines to present a kind of abstract of the work.

Thanks for this feedback. We have streamlined the manuscript wherever possible, including the conclusion.

5. The literature used in the manuscript: This is a paper that focuses on a local case study in India, and thus, a significant portion of the references seem to be local reports and publications. Scientific literature is not used to the extent of typical manuscripts in a scientific journal. I fully understand that this situation is explained by the topic of research, but I think the editor may want to make sure that the journal is OK with this

We have added the additional references to appropriate literature.

I. Other comments:
1. In the title, I would suggest changing "at tank and catchment" and simply use "across"

We have changed to say "across spatial scales"

2. I was not familiar with the term "command area". Maybe it is a good idea to clarify the concept the first time is presented (i.e., ...)

We now provide a clear definition of "command area" (fields irrigated by tank water) in lines 208-209.

3. Some descriptions in the first paragraph of section 3 are very detailed. I am not sure whether this is needed.

Extraneous details have been removed from paragraph 3.

4. I suggest removing "found to be" in line 11, page 12130.

Done

5. I would suggest using a letter different than “h” in eq.1 and eq. 2 because it is easy to get confused and read 24 hours.

We have changed *h* to h_n to avoid the confusion.

6. I do not understand what the authors are proposing in lines 7-9, page 12132. Moreover, the end of this paragraph could be reduced with the support of figure 1.

The White method cannot be used to estimate ET on days with rain. To estimate ET on those days we used interpolation. We are not sure how the end of this paragraph can be reduced as the reviewer proposes.

7. Because of the results described in line 24-25, page 12134, in Fig. 5 I would suggest to color differently the ET fluxes when inundated areas are larger than 25% of the maximum area. Moreover, how are the +/- values estimated or computed? What is the precise meaning of them?

We feel that coloring it different makes the figure look busy, and would prefer to use only the dashed line to show the difference.
The +/- values are the standard errors of the estimate when we provide only one average value over the entire season. We have added additional clarification regarding the validity of the high late-season ET fluxes in lines 520-607. Additional information is also now provided in Methods, lines 330-400.

8. Line 9 and 10 in page 12135 define again S_y. Is it needed?

Sy is now defined once only (lines 326-329).

9. In general, captions of figures are unnecessarily long. One thing that authors should do is to better use legends to explain the meaning of the different series in the plot. Currently they use the caption to do so.

The figure captions have been shortened, where possible.

10. Rewrite sentence in lines 9-10, page 12137 in a simpler manner.

We have simplified the sentence to “First, while recharge dominates the exchange dynamics of Tanks 1-3, Tank 4 is more discharge-driven.”

11. Line 20-22, page 12137. Why using "most" if only one reference is provided?

We cannot understand the question. There is no reference provided here, and this is based on our data.

12. Some tables could be simplified. Maybe the magnitudes and corresponding percentages could be presented in a single row using the value and then the percentage in parentheses.

We attempted to simplify Table 3 based on your suggestions, but the table actually became harder to read. Due to HESS limitations about not allowing lines between columns, we believe this version of the Table is the easiest for the reader to understand.

13. At the end of page 12138 authors mentioned that "These relatively small percentages contradict the established view of tanks losing a significant fraction of their water through ET". I think the sentence is more accurate if the ET vs recharge comparison is explicitly mentioned. In other words, I think the current sentence can be read as if ET in the study area is small. Indeed what happens is that the ratio of ET vs recharge is small, and thus recharge itself can be really high in the study area.

We agree and have modified the sentence accordingly following the suggestion.

14. Figure 11 is cited several times, although is not included in the manuscript.

We apologize for the mistake. We now use the correct figure numbers.

15. Line 3, page 12142. It should be Brouwer et al. (1989)

Thanks, we have made the change.

16. Table 1: What is the meaning of column %total? Is it needed?

We apologize for the confusion. That column represents the percent of the workforce that is agricultural. The heading has now been changed to clarify.

Table 2: Meaning of current tank capacity? What area the implications and meaning of soil types?

Current tank capacity is based on our measurements, while historical tank capacity is the information we received from tank memoirs. Different soil types in different tank areas indicate the possibility of different groundwater exchange dynamics. We do see very different dynamics in Tanks 1 versus the other three tanks. We have added this to Section 4.1.1, and also added some

text to the table caption differentiating between historical and current capacity.

17: Could you locate the weather station in figure 1a?

The weather station is now identified in the figure as "Peraiyur Station."

18: remove "a" in "view of a tank 4" (caption of figure 4)

The caption of Figure 4 reads "Tank water level and daily rainfall for the four tanks over the North East monsoon season. Tank water level is measured from the deepest point of the tank". So, we are not certain what this means.

19. I think equations are not needed in figure 3

We have removed the equation in Figure 3, and referred to the corresponding equation in the figure caption.

20: Figure 4: Because of the temporal scale, the initial conditions for the tanks are not clear. Maybe you can talk about it. Additionally, Why water levels in tanks 1 and 2 are plotted only until January?

The tanks were dry at the start of the monsoon season – so stage was zero at start. We tried to select the wells at the deepest points in the tank so that when the sensor in the well doesn't show a reading, the tank has no water. Unfortunately, vegetation in the tank beds made it impossible to place the sensor wells in the deepest portion of the tank in Tanks 3 and 4. Accordingly, the sensors in the well became dry earlier in the season, while the tank still had some water at the deepest point. We have added this information to the figure caption for clarity.

Reviewer #4 Comments with Responses:

Dear Professor Biggs:
Thank you for your comments and suggestions. We will use them to improve the manuscript on revision, as described below.

The paper quantifies the water balance of a nested system of small reservoirs (aka tanks) in southern India. The authors used pressure transducers to measure water level in four tanks over a wet season, field measurements of tank capacity and sluice outflow, and a simple method (White) to estimate recharge and ET. The study is the only one I know of that systematically measures the water balance of a cascade of tanks, and adds very useful information to understanding of such systems. The authors are to be commended for making a comprehensive set of field measurements and analyzing me in a useful way. The paper is very well written and easy to read.

> **Thank you**

I have mostly minor comments, with some more substantive questions about interpretations of "waste" flows and management implications.

Specific comments: see attached PDF for additional details and suggestions. I think there may be some errors in table 2, the ratio of irrigated area to water surface area. See comment in the PDF.

> **We have added responses to specific comments on the pdf below.**

A little more information (one sentence) on the Strange method would be helpful. Is it a regression equation? Water balance similar to Thornthwaite?

> **We have added the following details:**
> **The Strange method is an empirical method that is widely used by government departments in India, including the public works department of Tamil Nadu, for computing the runoff yield from the catchments of irrigation tanks and small reservoirs (Latha et al. 2012). In this method, daily runoff is calculated as a percentage of daily rainfall, based on tabulated values in which % runoff is expressed as a function of (a) rainfall on that day, (b) antecedent rainfall conditions, and (c) catchment characteristics (Shanmugham & Kanagavalli, 2005). The Strange Method has been shown to provide results comparable to those obtained with the more commonly used SCS Curve Number method (Latha et al. 2012), but is more representative of the south Indian conditions that are the focus of our study.**

L566 refers to "surplus sluice outflows". Next lines say that they will be lost to evaporation or runoff. But runoff is already low (5% of precipitation) and the surplus could also recharge groundwater through channel infiltration or infiltration in irrigated fields, which would not be "wasted". Flow out of the watershed could be important for downstream users, as suggested in other parts of the text. I would argue that excess sluice outflows are only "wasted" if they end up in pools and evaporate, or perhaps if they are evaporated by riparian systems downstream of the tanks and don't contribute to "beneficial ET", sensu Molden. We don't really know what happens to those extra sluice flows, and they may be beneficial or not.

Thank you for the comment. We absolutely agree and have modified the section accordingly.

The authors should refer to other work on watershed-scale water balances of tanks and smaller water harvesting structures in southern India, including:

Thank you for the references. We will add them to the Introduction.

Batchelor, C. H., Rama Mohan Rao, M. S., & Manohar Rao, S. (2003). Watershed development: A solution to water shortages in semi-arid India or part of the problem? Land Use and Water Resources Research, 3, 1–10.
Bouma, J. A., Biggs, T. W., & Bouwer, L. M. (2011). The downstream externalities of harvesting rainwater in semi-arid watersheds: An Indian case study. Agricultural Water Management, 98(7), 1162–1170.
Calder, I., Gosain, A., Rao, M. S. R. M., Batchelor, C., Snehalatha, M., & Bishop, E. (2008). Watershed development in India. 1. Biophysical and societal impacts. Environment, Development and Sustainability, 10(4), 537–557.
Calder, I., Gosain, A., Rao, M. S. R. M., Batchelor, C., Garratt, J., & Bishop, E. (2008). Watershed development in India. 2. New approaches for managing externalities and meeting sustainability requirements. Environment, Development and Sustainability, 10(4), 427–440.
Garg, K. K., Karlberg, L., Barron, J., Wani, S. P., & Rockstrom, J. (2012). Assessing impacts of agricultural water interventions in the Kothapally watershed, Southern India. Hydrological Processes, 26(3), 387–404. http://doi.org/10.1002/hyp.8138
Garg, K. K., Wani, S. P., Barron, J., Karlberg, L., & Rockstrom, J. (2012). Up-scaling potential impacts on water flows from agricultural water interventions: opportunities and trade-offs in the Osman Sagar catchment, Musi sub-basin, India. Hydrological Processes, n/a–n/a. http://doi.org/10.1002/hyp.9516.

Comments from the attached pdf

1. Comment 6-1: Line 153: Thanks for pointing this out. The numbers reported in the earlier draft of the manuscript were incorrect. We have simplified this statement to state "with 50% of that falling during the Northeast monsoon season (Oct – Dec)."
2. Comment 11-1: Rainfall was used as the input, which along with catchment area and runoff coefficients was used to estimate runoff into the tank. We have added text, as elaborated above, to describe this in greater detail.
3. Comment 11-2: Into the tank. WE have added this
4. Comment 11-3: No we don't. We use the overflows (sluice outflow) as described in lines 458-460 ("For the WT case, we assumed the sluice outflow from the most

downstream tank in the catchment (T1 for C1, T2 for C2, T3 for C3 and T4 for C4) to represent the Q value for each catchment.”). Here, we are just describing what the catchments are. We have added a new Figure 4, which I think helps clarify some of these issues.

5. Comments 13-1 and 13-2: We have made the suggested corrections.
6. Comment 19-1: Apologies for the confusion. Here, we refer to the reduction in functioning due to sedimentation. It is often assumed that 70% of functionality of tanks is lost due to sedimentation (see lines 785-787). We believe, however, that this calculation of the increased benefit of tank rehabilitation was confusing, and we have simplified the section. We instead provide just a more straightforward calculation of the overall potential benefit of fully functioning tanks across the watershed (lines 791-801: “Our water balance calculations show that tanks, with adequate maintenance, provide a mean groundwater recharge benefit of 5,600 m^3 per hectare of tank waterspread area. At the scale of the Gundar basin, with its 2276 village-scale RWH tanks, each covering an area of approximately 40 ha (DHAN, 2010), these results suggest that fully functional tanks could provide a groundwater recharge benefit of 522 MCM. With a population of approximately 3,000,000, this difference translates to a difference in water availability throughout the Gundar Basin of 174 m^3 per capita. It is currently estimated that all of India is experiencing some degree of water stress, with per capita availability ranging from 1000-1700 m^3/year (Amarasinghe et al., 2005). Accordingly, maintaining tanks at full functionality has the potential to increase per capita water availability in the Gundar by approximately 10-15%.”
7. Comment 20-1: We have made the suggested change.
8. Comment 20-2: We don’t know the answer to that question. Probably, historically they were better maintained. But, I think now even when they are rehabilitated, the entire village is not as invested in the rehabilitation due to the availability of alternative groundwater sources.
9. Comment 20-3: Yes. We have now rewritten part of this paragraph to reflect this point (lines 837-849): “Notably, large quantities of surplus sluice water leave the tank soon after it fills. These surplus sluice outflows are not needed by the crops at the time they leave the tank and will either leave the catchment as evaporation or downstream runoff, or will recharge groundwater over the course of the outflow channel and become available to downstream users. Because the sluices are for the most part not actively managed or appropriately maintained in our study area, the sluices remaining perpetually open and outflows are purely a function of water levels in the tank rather than a timed need for irrigation water. As reported in **Table 5**, it was found that anywhere from 31-79% of IWD within the study cascade remains unmet, while approximately 15-50% of available sluice outflows leave the tank unutilized by crops in the tank command areas. This remaining irrigation water demand in many cases must be met by farmers using groundwater pumping to supplement tank water, and in other cases remains unmet, leading to reduced yields or crop failure.”

10. Comment 21-1: Soil moisture is taken into account using the Strange method approach.

11. Comment 21-2: Yes, we have addressed this point as suggested (lines 885-888): "These results suggest that, to optimize tank operations and to maximize the water-provisioning capabilities of the tanks, earlier planting times could be adopted by farmers, with supplemental irrigation from groundwater being utilized until the tanks fill."
12. Comment 22-1: Thank you. We have added this clarification (lines 917-918): "with ET accounting for only 13-22% of the outflows, including open water evaporation and ET of plants transpiring in the tank bed."
13. Comment 22-2: That is true. We have changed the text as suggested (lines 925-928): "Our results also highlight that a large fraction of the tank water is not used in the tank command area because, despite ongoing the efforts toward tank rehabilitation and maintenance in our study cascade, the sluices continuously discharge tank water, thus providing surplus water at times of lower demand"
14. Comment 34-1: Thanks for catching that. There was an error in the current tank capacity column. This is how the new table looks:

Tank #	Soil Type	Maximum Depth (m)	Maximum Tank Surface Area (ha)	Tank Command Area (ha)	Command Area/Surface Area Ratio	Tank Capacity (m^3) Historical	Tank Capacity (m^3) Current	Current Capacity/ Historical Capacity
Tank 1	Alfisol	3.2	15	27	0.96	357,700	276,405	0.77
Tank 2	Vertisol	3.4	51	45	0.77	656,500	407,513	0.62
Tank 3	Vertisol	4.0	14	19	0.93	237,000	217,633	0.92
Tank 4	Vertisol	3.3	21	24	1.25	168,000	139,270	0.83

15. Comment 43-1: Thanks for noticing this. Indeed, the axes were not aligned correctly. We have made the change.