Spatial characterization of long-term hydrological change in the Arkavathy watershed adjacent to Bangalore, India -- Response to Editor and Anonymous Referees 1 & 2

Dear Dr. Shraddhanand Shukla,

Thank you for conveying the referee comments to us. Both referees provided insightful comments that we believe will strengthen our manuscript. In response to their comments, we intend to make several revisions, including additional validation of our classification method and a modified discussion of the relationship between hydrological change and land use. We outline our major edits and additions in this letter. Other revisions and responses to more minor comments are included in the attached detailed responses to the referees.

Referee 1 raised two main concerns. The first related to (a) our ability to attribute hydrological change to land use and the second to (b) our treatment of dry-season water losses. With respect to point (a), we agree with the referee that we did not present a detailed evidence-base that quantifies the role of land use and land change in shaping hydrological change in the Arkavathy. Such a full attribution lies beyond the scope of the present paper, and forms the subject of an ongoing, separate study. We also agree that we need to offer more information regarding the feasible drivers of the observed hydrologic change (a topic that we explored in greater detail in a previous paper, Srinivasan et al., 2015) as context for the present study.

Therefore, we propose removing Figure 8b from the manuscript, and rewriting the discussion about potential drivers of hydrologic change that summarizes the available evidence (including that presented in Srinivasan et al., 2015) and provides an open-ended discussion of the spatial trends observed, and their interpretation in light of spatial patterns of change (including changes in land use and land use practices) and hydrological processes occurring in the basin. We will also include the possibility that the upstream-downstream dynamic could play a role in the pattern of hydrological changes, but our analysis to this point suggests that it is unlikely because of the overall watershed fragmentation and disconnection of surface water and groundwater (except downstream of urban areas which are affected by inter-basin water imports and urban effluent). Lastly, we will explore a more sophisticated analysis of hydrological trends and landuse in the northern part of the watershed and will present any additional findings in the revised manuscript.

With respect to point (b), we thank the reviewer for pointing out the potential difficulties in interpretation posed by the fact that the dry-season water loss metric used in the overall statistical model did not allow for that metric to vary over time, yet independent exploration of losses in the two northernmost watersheds suggested that the loss rate was time varying. While we agree that this situation is potentially problematic, we note that the direction of the trend in losses (decreasing over time) is opposed to the overall trend in drying in the watersheds. In other words, the assumption of stationarity in these 2 watersheds is a conservative assumption that will lead to us under-estimating the time trend in drying, and does not change the conclusions obtained by the statistical model. We will clarify this issue in the revised manuscript.

Referee 1 also made comments regarding our figures. In response to his suggestions, we will move Fig. 6 to the Supplementary Material, and Figs. S4 and S5 to the results section, excluding the comparison of water extent and precipitation which will remain in the Supplementary Material.

Referee 2 raised several significant high level concerns. In particular, they questioned (a) the suitability of the manuscript for HESS, (b) what relevant conclusions about regional and national water resources could be drawn from understanding the relatively small spatial scales addressed within the Arkavathy Basin, (c) the simplicity of the remote sensing classification, (d) the suitability of validation of the remote sensing classification, and (e) failure to address seasonal dynamics in the study.

To address (a) and (b), we will restructure the introduction and discussion sections highlighting the following arguments to emphasize the significance of this paper to the water resources community in southern India as well as the hydrology research community. Our revised introduction will make the following points:

India faces an array of water scarcity challenges, many of which have been studied at the country scale (Devineni et al., 2013; Tiwari et al., 2009) or at the local field scale (Perrin et al., 2012, Van Meter et al. 2016). Other studies have applied hydrological models at the local scale (Glendenning and Vervoort, 2011) and regional scale (Gosain et al., 2006), but none of these studies describe patterns of surface hydrological change. What is missing from the hydrology literature is an historical analysis at spatial and temporal scales commensurate with the scales of the change. The absence of hydrological records is a primary reason for this gap in the literature (Batchelor et al., 2003; Glendenning et al., 2015), and new datasets are needed that indicate hydrological change at a scale that sufficiently captures the spatial heterogeneity. A study in Tamil Nadu considered changes in tank water storage at multiple points in time (Mialhe et al 2008), but to our knowledge there are no other studies that identify distributed hydrological changes throughout space. Such a spatial understanding is particularly pertinent to our study region where the hydrology is truly local. Hydrological records are insufficient to capture the spatial nature of hydrological change, as there are only two streamflow gauges in the Arkavathy watershed which spans over 4,000 sq. km. Furthermore, streamflow in the Arkavathy at a given point is not an integrated measure of upstream processes. Upstream and downstream subcatchments have been isolated by the fragmentation of the river network (due to tanks and check dams) and the subsurface disconnection due to the vastly depleted groundwater table (as we will clarify in our manuscript, urban effluent can serve to maintain a connected river network directly downstream of urban areas). The Arkavathy contains features that are characteristic of the landscape throughout much of Southern India, and although the findings from our study cannot be directly applied to the region as a whole (given the spatial heterogeneity of the change), the lessons from the Arkavathy can provide clues to hydrologic functioning in the broader region. The heterogeneity of observed changes in the Arkavathy emphasizes one of the problems associated with viewing water trends only at regional or national levels -- such large scale trends to not map directly to local scales, yet these are the scales at which people experience and must respond to change. Such local understanding is of great importance to water managers in southern India, as considerable efforts are underway for river and tank rehabilitation in some areas, without a clear understanding of the mechanisms underlying the historical degradation and loss of water resources (Kumar et al., 2016; Srinivasan et al., 2015).

Regarding point (c), we agree that the remote-sensing classification is simple, but that it has strengths: it is likely to be unbiased and stationary across all Landsat sensors and furthermore, we have demonstrated that it is sufficiently accurate for our purposes. We also note that although the reviewer suggested that our classification was solely based on NDWI, that NDWI in fact represents only the first stage of the classification, which then relies on use of the green, red, and NIR bands in our spectral unmixing of clear and turbid water.

We appreciated the referees suggestions regarding point (d). Google Earth images do indeed offer a high resolution visual dataset that gives us the opportunity to further validate the remote sensing approach. We plan to incorporate such additional validation in the revised manuscript. The earliest Google Earth images in our study watershed are in 2004. The details of our proposed additional validation strategy are outlined in the detailed response to Reviewer 2.

Finally, with respect to point (e), we agree that seasonal dynamics are interesting to understand in so far as they indicate the seasonal availability of surface water resources. However, we avoided a detailed description of these dynamics for several reasons. Firstly, since tanks are not widely utilized as a surface water resource throughout the Arkavathy Basin today, the importance of understanding these seasonal dynamics is not so great in the present context as in situations where those surface water stores are relied upon by communities. The importance of the tanks as studied in this paper is as indicators of long-term changes through space in the hydrological dynamics that produce the end of monsoon season storage. Secondly, for pragmatic reasons, it is challenging to study within-year variations other than in the dry season. For approximately six months of the year, extensive cloud cover obscures many of the tanks in Landsat images and active radar satellite imagery (which can effectively “see through” clouds) is too coarse to estimate water area in small tanks. We appreciate the referee comments and we will more carefully discuss dry-season dynamics in the manuscript.

Both referees provided clear suggestions for improving the manuscript. We thank the referees for their consideration of this paper and we include detailed responses to the referees below. We look forward to your comments on the manuscript and our response to the referees.

Best regards,

Gopal Penny
Veena Srinivasan
Iryna Dronova
Sharad Lele
Sally Thompson

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Response to Referee 1
Referee comments in black
Our (author) response in blue

Strengths

The premise of this paper is interesting and the application of remote sensing to measuring the extent of tank surface area is unique. The paper demonstrates the practical application of remote sensing for characterizing hydrologic change in an otherwise unmonitored setting.

I appreciate the challenge of accounting for various degrees of turbidity in the classification of these water bodies. The methods for measuring tank water extent were clearly presented, and the supplementary figures showing examples of classification were really useful. In my opinion, all of the figures and tables, including those in the supplement, are necessary and contribute to this paper, with the possible exception of Fig. 6. The supplemental tables should make it possible for someone to reproduce this
Analysis.

- We thank the referee for offering careful consideration and analysis of the manuscript. The referee brings up a number of valid points that we believe will strengthen the paper. We intend to incorporate a number of the suggestions from this referee. We will move Figure 6 to the supplementary material. We will move figures S4 and S5 from the supplementary material to the Results section of the manuscript (excluding the comparisons of water extent and precipitation, which will remain in the supplementary material).

Major Concerns

Although I accept that the multiple regression in Eqn. 1 is a reasonable technique to remove precipitation (climate) effects from the estimate of long-term trend, the analysis of hydrologic change related to land use change is not convincing. The visual comparison of percent agriculture with temporal trend in water extent shown in Fig. 8b does not show a clear relationship. It appears that there is only a temporal trend of magnitude greater than 1 ha decadeˆ(-1) 10 kmˆ(-2) (units should be clarified, is this ha/(decade * 10 kmˆ2)?) if the agricultural area is close to 0.75% (which I assume is a typo for 75%); however, low temporal trends are possible for any percent of agricultural land area. This is not a strong argument for a relationship between the two. In fact, the notable negative trends occur only in the two northernmost sub-catchments.

- Thank you for this comment. Our intention in presenting Figure 8b was to conclude the manuscript with some initial ideas about the attribution of hydrologic change to potential drivers. We agree, however, that despite the statistically significant Mann-Kendall trend, the proportion of a watershed covered with agricultural land use has a tenuous relationship with hydrological change (at a minimum, one might argue that using a snapshot of land use to explain a decadal trend in hydrology poses a problematic mismatch). We agree that removing Figure 8b is appropriate. We will replace it with a written discussion that offers a broader context for the observed changes. Furthermore, using recently developed maps of land use from 1973, 1994, 2001, and 2013 of the northern Arkavathy watershed (the 3 northernmost subcatchments), we will explore a more detailed analysis of the relationship between land use and hydrological change and present any additional findings in the updated manuscript.

An argument could possibly be made that this is an upstream-to-downstream effect, where water withdrawals upstream have a greater impact on stored water over time because return flows from irrigation dampen the effects of water withdrawals in downstream sub-catchments and/or the major reservoirs shown on Fig. 1 are operated in a way that mitigates long-term trends in water storage changes in the tanks (see for example de Graaf et al., 2014).

- Thank you for this interesting suggestion. Our current hypothesis is that the drying of the northern part of the watershed is linked to groundwater pumping that caused a disconnection between groundwater and surface water (see Srinivasan et al., 2015), leading to reduced baseflow, in a manner analogous to the model in de Graaf et al. (2014). Field studies addressing this mechanism are also currently in progress. Unlike the model presented by de Graaf, however, we are doubtful that the upstream-to-downstream effect is important in the Arkavathy today. Various sources of indirect evidence indicate that the water table is hundreds of meters below the surface in northern parts of the Arkavathy watershed (Srinivasan et al., 2015), suggesting that excess infiltration water is likely to move vertically. Similarly, the relief in the watershed is only about 100 m over a distance of 100 km, again promoting vertical groundwater movement and system-wide return flows connecting upstream to downstream are unlikely. We will make a note to this effect in the revised manuscript.

Additionally, as the authors note on p. 7, lines 28-35, the two watersheds farthest upstream (those that drive the trend) were the only two watersheds with a significant trend in dry season water loss, which they relate to the shift from tank irrigation to groundwater irrigation during the study period. Unless I have misunderstood how dry season losses were treated in the regression, this shift would be reflected in the long-term trend. The authors should test whether or not the change in drying rate is the dominant cause of the trend, and if without this shift, a relationship with the % agricultural area still holds.

- Thank you for this salient observation. Non-stationarity in the dry-season water loss term would indeed affect the magnitude of estimated hydrological change in tank clusters given that the regression relationship used to identify this change assumes a stationary loss coefficient.
- The violation of this stationarity assumption in 2 tank clusters might be expected to marginally increase the model error, and, if the time trend in the dry season losses was

aligned with the time trend in tank storage, it could indeed confound interpretation of the meaning of the storage trend. However, the trend in dry season losses in the northernmost tank clusters is, instead, in the opposite direction to the trend in storage. Dry-season loss rates have *decreased* over time in the two northernmost subwatersheds. We would expect this change to result in an *increase* in tank water storage after monsoon season (as tanks lose water more slowly). Yet we observe a statistically significant decrease in post-monsoon tank storage over time, in spite of the decrease in loss rates. Thus, introducing a non-stationary loss coefficient into the model might improve model fit (at the expense of the degrees of freedom of the model) and improve quantitative estimates of the rate of drying due to hydrologic change in the northern watersheds, but would not alter the main conclusion of the study, which is that these watersheds are, in fact, drying.

- We will add clarification of these points to the discussion.

Are there other spatial patterns in rates of groundwater pumping?

- Understanding the spatial patterns of groundwater extraction in the Arkavathy Basin would be very useful. Unfortunately, monitoring of groundwater use through space has been indirect and sparse. We are exploring whether proxies for groundwater irrigation could be developed from the remote sensing record as an ongoing project, but at this point we are not in a strong position to analyze the effects of such spatial variation on the spatial differences in surface water trends.

The authors develop a simple mathematical model to extract the trend (B) due to "hydrological change", by which I infer that the authors are referring to the "temporal trends in water extent: : :indicative of long-tem hydrological changes induced by human activity" (p. 3, lines 12-13). The intent would be clearer if the authors were to describe other potential causes of this change (for example, temperature change in the region) and to state when defining B in Eqn. (1) that it is the trend (primarily) due to human-induced hydrological change. Also, because dry season loss is a variable in this regression, it is important that the authors clarify exactly which change B is tracking. As described in lines 27-28, p. 7, the dry season loss term is actually the number of dry season days, rather than a volumetric water loss. As such, the trend B presumably includes year-to-year variations in dry season water use as well. This should be stated explicitly, and instead of loss (L) in Eqn. 1, the authors should refer to the variable as what it is, number of dry season days. In summary, the manuscript needs to be more explicit about what exactly the authors intend B to include and exclude, and why.

- We thank the reviewer for these helpful comments, and particularly the suggestion to frame the response to these issues in terms of the effects on the "meaning" of the trend term B. Many of the issues relating to human-induced change (rather than environmental change drivers) were addressed through a hypothesis testing approach in a previous paper (Srinivasan et al. (2015)), which concluded that hydrologic change in the Arkavathy derives from human activity rather than changes in climate or weather.

- We agree that the designation of L as a “loss” term is misleading (as L is the time variable and rather the loss rates arise in the coefficient C,3k). We will consider changing the letter designation of the variable as well as its name in order to clarify the interpretation of that component of the regression.
- We also agree with the reviewer that the magnitude of B could be affected by other sources of variation (that is we are potentially vulnerable to the unobserved variable problem). We note that random interannual variations in water use, dry season losses, evaporative rates etc would not alter the magnitude of B, as they would not change the long-term trend. Rather, random variability would widen the confidence intervals around B. We will clarify this in the manuscript highlighting the fact that temporal changes in B that are not statistically significant may not reflect true hydrologic changes.
- Finally, we will clarify in the manuscript the statistical and hydrological interpretation of B. Statistically, B is the temporal trend in total tank water storage over time, after controlling for a stationary relationship between the covariates we describe (Ptotal, Pextreme, L) and tank water storage. Hydrologically, B represents a change in the relationship between both precipitation and dry season water losses and streamflow. Because there is is no change in the effect of dry season water losses in 6/8 watersheds, we interpret B as a change in the rainfall-runoff response. In the two subwatersheds where we detect a change in the effect of dry season water loss on tank storage, we will clarify that B captures the combined effect of hydrological change (streamflow decline pushes B in the negative direction) and dry-season tank water losses (lower tank losses pushes B in a the positive direction). Because B is negative in this area, the effect of hydrological change must exceed that of reduced tank water losses.

Secondary Concerns

The one figure that, to me, is basically a throw away is Fig. 6 for multiple reasons. First, the reservoirs are explicitly exclude from all other parts of the analysis, so whether or not their time-trends are correct is immaterial. Second, the figure does not show an independent source of the temporal evolution of reservoir extent. Third, the conclusion that can be drawn from the satellite imagery matching the timing of reservoir construction is simply that the algorithm can distinguish if, in a very large body of water, there is essentially no water or a lot of it. If this were not the case, there would be no merit in even pursuing this approach at all. It would be reasonable to mention that the method shows the timing of reservoir construction and filling as a single sentence.

- Thank you for this suggestion. We agree with the referee’s argument. We will move Fig 6 to the supplementary material, and take the referee’s suggestion to summarize the results in a sentence or two.

In terms of reproducibility, it would be helpful if the authors could provide contact information (an address, perhaps) for Karnataka State Remote Sensing Application Centre as a source for a shapefile of tank boundaries in the Acknowledgments section.

- We will provide contact information to KSNDMC. To assist with reproducibility, we will publish the time series of tank water area for all tanks in the watershed, along with the geolocation of each tank. This will also allow other researchers to explore the remote sensing data.

MINOR STUFF:

- Broadly we agree with all minor suggestions made by the referee and will make appropriate changes. We offer explanations below as needed.

p. 7, line 20: please clarify why average depth is used for extreme precipitation events rather than total number of extreme events or total depth of precipitation in extreme events.

- We use average depth of extreme events as a way of approximating heavy rainfall, because our experience in the field suggests a prevalence of infiltration excess runoff. Larger storms are likely to have more infiltration excess runoff due to intense rainfall, and average storm depth is a rough way of approximating this in a way that is feasible (as only daily precipitation data are available) and meets the requirements for the statistical model. Total depth in extreme events is more likely to be correlated with total precipitation depth (and thus add less information to the model) than average storm depth. We will clarify this in the paper.

p. 7, last paragraph: reference Fig. S8.

p. 8, 2nd paragraph: define variable terms explicitly (i.e., The covariates total precipitation, Ptotal,ij, : : :) here, close to the equation, instead of in previous paragraphs. State near the equation that the loss is actually the number of dry season days

p. 8 line 19: clarify what is meant by “centered” (long-term means removed?).

- We will clarify that “centered” entails removing the mean (shifting the data to a mean of zero).

Fig. 7: it would be useful to overlay a drainage/stream map to show how subwatersheds relate.

p. 10, line 1: clarify what is meant by “The spatial scales of tank clusters are comparable with that of land use”

- This sentence will be removed after making changes in the discussion. We will clarify what we originally intended to say, which is that spatial heterogeneity of hydrological change is important, and that the observed pattern of hydrological change can be related to observed pattern of land use change if we can resolve the hydrological change at a sufficient level of detail.

p. 10, lines 16-17: quotes around “drying” make sense because this is referencing algae blooms giving the false appearance of smaller tank water extent. Quotes around “wetting” do not make sense because the increase in impervious surfaces actually causes tank water extent to increase. It may not be more water in the watershed, but it is more water in the tanks.

p. 10, line 29: instead of saying “: : :by focusing on land use from a single date.”, say “: : :because we only consider land use on [Mon. Day, Year]”

Figs. S4-S5: at least mention in the caption the water extent vs. precipitation plots.

References

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<u>Response to Referee 2</u>

Referee comments in black

Our (author) responses in blue

Overall this is a well written manuscript that attempted to describe trends and spatial differences in changes in hydrology in the Arkavathy watershed on the basis of changes in extracted tank water surface area from satellite images along with other attributes.

- We thank the referee for consideration of our manuscript and valuable advice in helping us clarify some of the key messages of the paper. The referee's feedback has been helpful in alerting us to pieces of writing that need to be improved, particularly in clarifying the broader perspective.

Although the methods were well described, the broader perspective of the analysis is not well presented. After all the study analyzed the tank's surface water dynamics for a very small area (the total area of the Arkavathy is not provided), so, what new information does the findings bring to the community compared to the known facts at regional to national scale for India?

- We will provide the watershed area (4,160 sq. km) in Study Site section. We will clarify the broader implications of our research in the manuscript by making the following argument:
- The Arkavathy contains features that are characteristic of the landscape throughout much of Southern India, and although the findings from our study cannot be directly applied to the region as a whole (given the spatial heterogeneity of the change), the lessons from the Arkavathy can provide clues to hydrologic functioning in the broader region. India faces an array of water scarcity challenges, many of which have been studied at the country scale (Devineni et al., 2013; Tiwari et al., 2009) or at the local field scale (Perrin et al., 2012, Van Meter et al. 2016). Other studies have modeled hydrology at the local scale (Glendenning and Vervoort, 2011) and regional scale (Gosain et al., 2006), but none of these studies describe patterns of surface hydrological change. What is missing from the hydrology literature is an historical analysis at spatial and temporal scales commensurate with the scales of the change. The absence of hydrological records is a primary reason for this gap in the literature (Batchelor et al., 2002; Glendenning et al., 2015), and new datasets are needed that indicate hydrological change at a scale that sufficiently captures the spatial heterogeneity. Such a spatial understanding is particularly pertinent to our study region where the hydrology is truly local, because upstream and downstream subcatchments have been isolated by the fragmentation of the river network (due to tanks and check dams) and the subsurface disconnection due to the vastly depleted groundwater table (as we will clarify in our manuscript, urban effluent can serve to maintain a connected river network directly downstream of urban areas). The heterogeneity of observed changes in the Arkavathy emphasizes one of the problems associated with viewing water trends only at regional or national levels - such large scale trends to not map directly to local scales, yet these are the scales at which people experience and must respond to change. Such local understanding is of great importance to water managers in southern India, as

considerable efforts are underway for river and tank rehabilitation in some areas, without a clear understanding of the mechanisms underlying the historical degradation and loss of water resources (Kumar et al., 2016; Srinivasan et al., 2014).

Given the size of the tanks studied, I would imagine the seasonal water area dynamics will have greater implications than the inter-annual dynamics. The manuscript did not discuss anything on the seasonality for these tanks, or how does that influence the trend?

- We agree that seasonal dynamics are interesting to understand in so far as they indicate the seasonal availability of surface water resources. However, we avoided a detailed description of these dynamics for several reasons. Firstly, since tanks are not widely utilized as a surface water resource throughout the Arkavathy Basin today, the importance of understanding these seasonal dynamics is not so great in the present context as in situations where those surface water stores are relied upon by communities. The importance of the tanks as studied in this paper is as indicators of long-term changes through space in the hydrological dynamics that produce the end of monsoon season storage. Secondly, for pragmatic reasons, it is challenging to study within-year variations other than in the dry season. For approximately 6 months of the year, extensive cloud cover obscures many of the tanks in Landsat images and active radar satellite imagery (which can effectively "see through" clouds) is too coarse to estimate water area in small tanks. We appreciate the referee comments and we will more carefully discuss dry-season dynamics in the manuscript.

The manuscript mentioned about differences in water quality, turbidity, vegetation in the water which are influential factors for changes in the reflectance. Even though the DN values were converted to reflectance, the manuscript used only one index (NDWI) to classify water surface area, while there were potentially many other methods or index (Senay et al., 2013) could be used to map water surface correctly, as no one index can cover it all.

- We agree that there is no one method for remote sensing classification of surface water. We selected a simple classification method that was consistent across all Landsat sensors (MSS, TM, ETM, OLI). Our method uses NDWI as an initial classification, and we then apply spectral unmixing using Red, Green, and NIR bands. Although more complex methods have been published, they may not result in a significant improvement in confidence in our model, which we believe is sufficient for our purposes.

While the analysis was performed for the time period between 1972 and 2010 the validation was done for 2014 results. To me validation needs to be done for the time for which the trend analysis is performed (few sample years both wet and dry between 1972 and 2010).

As the study area is so small Google earth might provide good data for validation. Have the authors looked into google earth images as a potential source of validation data?

- We thank the referee for this suggestion. Our ability to completely validate the model between 1972 and 2010 is limited by the availability of independent data-sources at higher spatial resolution for such a validation - specifically, the lack of accessible aerial data for the region and the lack of low-cloud commercial high-resolution satellite datasets prior to early 2000s. Since, however, there is no reason to anticipate that the classification relationships should be non-stationary, we consider the most compelling part of the reviewer's suggestion is to address both relatively wet and relatively dry years, which can be accomplished using a more contemporary dataset. In particular, the suggestion to use Digital Globe (DG) images via Google Earth is sensible, and allows us to use images from as early as 2004 (although we note that individual DG images cover only a portion of the whole Arkavathy, so that the earliest date of available imagery varies). Specifically, there are DG images which may be suitable for validation (being close to the end of the monsoon season and having a suitable Landsat image taken at a similar time) and covering portions of the watershed on the following dates : 7-Dec-2005, 30-Dec-2006, 30-Dec-2007, and 25-Feb-2009, 7-Feb-2004 On 11-Feb-2009, and 8-Feb-2010.

- We are working on the details of a validation approach based on manual delineation of tank water area from the DG imagery which will be included in the revised paper. The scale of the validation in terms of the minimal number of tanks required will be decided via power analysis as follows: We set the null hypotheses that the actual correlation between the area of classified tanks and the area of validation tanks is greater than the correlation for tanks classified in our initial (2014) analysis described in the manuscript (H0: R^2 > 0.95). The null hypothesis is therefore that the actual R^2 is less than 0.95. If the true R^2 is 0.9, we would need 30-50 tanks to achieve a power of 0.5-0.75 in this statistical test to reject the null hypothesis. We will attempt to reach this number in multiple years, noting the limited spatial scale of DG images and limited date range (2004 and later).

Page 10 line 5: claims that MK analysis confirms an increase in agricultural land use fraction is related to decrease in tank water storage. How? There is no evidence shown in the manuscript that suggests agricultural land use is increasing. This is vague to me.

- We are going to restructure this analysis, and will make sure to clarify a number of key points. Agriculture has not expanded so much as it has changed over the course of the study period, and the changes in the nature of agriculture could be the cause of drying in the norther part of the Arkavathy. Bangalore has urbanized rapidly over the study period, with its population increasing by a factor of 4. We will clarify these points in the revised manuscript. Furthermore, using recently developed land use maps from 1973, 1994, 2001, and 2013 of the northern Arkavathy watershed (the 3 northernmost subcatchments), we will explore a more detailed analysis of the relationship between land use and hydrological change and present any additional findings in the updated manuscript.

Page 10 line 11-12: statement connects with changes in land use and management practice with depleted subsurface stores without providing evidence.

- We will restructure this analysis as well. We will provide more context regarding changes in land use as well as management practices. Our discussion was intended as an initial attempt as understanding drivers of hydrological change. We will clarify that this analysis is exploratory, and we will also provide more details from other works that have been written already (Srinivasan et al., 2015; Lele et al., 2014).

Page 11 line 6-7: Target for classification is to identify water and not water cells, in that case how does incorporation of additional land cover will reduce the classification error?

- Because we are using spectral unmixing, the land class end-member affects the calculated water fraction in each cell. For this reason, having additional (and more precise land classes) could potentially improve classification. We will clarify this point further in the paper.

I think the method used in the manuscript is too simplistic, although producing time- series information of tank water surface area is valuable. I am not sure how much new information has been brought to the community by this study; therefore I am not convinced that HESS is the right journal for this article.

- We agree that the classification is fairly simple, but overcomes a variety of challenges related to the study, such as the need to incorporate imagery from four Landsat sensors (MSS, TM, ETM, OLI), spectral unmixing in all images, cloud and cloud shadow masking, and the temporal nature of water in tanks (and single image gap filling in SLF-off images). We also note that the classification serves its purpose based on the validation we showed in the manuscript. The overall objectives for the paper (and updated validation information) will be clarified in the updated manuscript, as we describe above and in the letter to the editor.

Senay, G.B., Velpuri, N.M., Henok, A., Pervez, M.S., Asante, K.O., Gatarwa, K., Asefa, T., & Jay, A. (2013). Establishing an operational waterhole monitoring system using satellite data and hydrologic modelling: Application in the pastoral regions of East Africa. Pastoralism: Research, Policy and Practice, 3, 20.