Spatial characterization of long-term hydrological change in the Arkavathy watershed adjacent to Bangalore, India -- Response to Referee 1

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, $P_{total,ij}$, : : :) 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

de Graaf, I.E.M., L. P. H. van Beek, Y. Wada, M. F. P. Bierkens, 2014: Dynamic attribution of global water demand to surface water and groundwater resources: Effects of abstractions and return flows on river discharges, Adv. Water Resour., 64, 21-33, doi:10.1016/j.advwatres.2013.12.002.