Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2016-562-RC1, 2017

Hydrology and Earth System Sciences
Discussions
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Interactive comment

Interactive comment on "Spatial characterization of long-term hydrological change in the Arkavathy watershed adjacent to Bangalore, India" *by* Gopal Penny et al.

Anonymous Referee #1

Received and published: 11 January 2017

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.

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. 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). 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. Are there other spatial patterns in rates of groundwater pumping?

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.

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.

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.

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.

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?).

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

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

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.

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