

Interactive comment on “Proximate and underlying drivers of socio-hydrologic change in the upper Arkavathy watershed, India” by Veena Srinivasan et al.

H. McMillan (Referee)

hmcmillan@sdsu.edu

Received and published: 19 October 2017

This paper describes a large, integrated study of the upper Arkavathy watershed in India, where anthropogenic change has caused substantial declines in groundwater and reservoir levels. The study employs sociological and hydrological modelling methods in an attempt to determine the dominant causes of these declines. The paper is generally well written and interesting to read, and the subject of the study fits well into the scope of HESS.

Major comments:

[Printer-friendly version](#)

[Discussion paper](#)



1. The structure of the paper is confusing for the reader. On reading, it appears as two papers back to back – the first addressing building and evaluating the nested hydrologic model (up to Section 6.3), and the second an investigation of the sociological drivers (Sections 6.4 and 7). Currently there is little connection between the two. I think either the second part should either be removed to a different paper, or it should be included up front as part of literature review and model development, and then the authors would need to show how this information is used within the hydrologic model.

Other comments:

1. Several typos throughout the manuscript, please proofread.
2. Line183 - The reasons for discounting the monitoring well data are not very well motivated, in such a data scarce catchment surely it adds some information?
3. In the methods section it is not always clear which work was already completed as part of previous studies in the watershed, and what is new for this paper. It would be helpful if the authors can try to clarify this where possible.
4. Line255 - The assumption of no groundwater connectivity between tank aquifers does not seem realistic even if there are no large fractures. There doesn't seem to be any reason why groundwater would be connected within tank basins and not connected outside. The authors should at least discuss the limitations of this assumption.
5. Figure 5. Please clarify in the caption whether this is data or model output.
6. The MWF model seems as though it would be very sensitive to rainfall intensity, but the rainfall is downscaled data and so may not represent the intensity accurately over large areas. Please can the authors comment on what impact this could have on the model accuracy.
7. Figure 7. From looking at the figure, recharge seems to be defined as “water below rooting depth” meaning that the depth at which water is judged to become recharge depends on the crop. Is this correct, and if so shouldn't recharge be deemed to begin

[Printer-friendly version](#)

[Discussion paper](#)



at a consistent depth?

8. Line322. Do you mean that groundwater decline and land use change cannot explain runoff decline under any circumstances, or just that it did not work in your model?

9. Some conclusion is needed at the end of the modelling section. Was the model deemed to be good/bad/useful? How will it be used in future? This is partly due to the problem of the paper structure, as normally the paper discussion and conclusion would be sited here to discuss the success or otherwise of the modelling effort. I would also suggest that the information in Supplement 5 be added here as part of the model discussion.

10. Table 2. The table seems to show more causes of groundwater increase than groundwater decrease, does this mean that groundwater should be increasing?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2017-543>, 2017.

Printer-friendly version

Discussion paper

