

Interactive comment on “Why is the Arkavathy River drying? A multiple hypothesis approach in a data scarce region” by V. Srinivasan et al.

C. Scott (Referee)

cascott@email.arizona.edu

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Review comments on HESSD 12, 25–66, 2015, “Why is the Arkavathy River drying?”
V. Srinivasan et al.

General comments

The link between basin response and climate and human drivers is excellent. Applying and testing multiple hypotheses to the Arkavathy case is extremely insightful. I like this paper a great deal and think that with appropriate revisions, it should be published in HESS.

I am concerned about the link to water security, not so much because the case and

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analyses presented do not speak to this challenge, but rather that the authors do not seriously engage with the expanding literature on water security. Bakker's piece is useful, but perhaps more insightful and analytical is the work of her PhD student Christina Cook (published in GEC and other outlets). New analyses and case applications of water security frameworks, especially in water scarcity conditions and addressing adaptive management frameworks, continue to be published. I suggest looking into some of these, or dropping the water security framing of this paper.

The conceptualization and analyses are original. I think this is an excellent scholarly contribution to the expanding field of socio-hydrology.

Specific comments (with reference to page/line numbers)

27/15-25: There is a lot here that should be further developed and clarified, especially assumptions of prediction/ predictability leading to sound policies and the i, ii, iii “issues not addressed by . . . hydrologic models”. I would suggest you separate iii (policy based on other non-scientific factors) from i and ii (which are, following your logic and sentence structure, issues that models at least attempt to address).

28/21: cite more recent Vorosmarty (2010) piece

28/24: remove “could”, ie, “actions of millions of small water users have significant impacts”

29: section 1.1.2 – excellent

30/15: suggest you expand in a sentence or two on “policy relevant knowledge gaps”

31/16: good multiple working hypotheses, but following your own notion and Buytaert's that knowledge is dispersed, how were these five hypotheses arrived at? Were others considered and discarded?

32/17: tanks not takes

32/20-25: any estimation of volumetric storage capacity in top 20 and 60 meters (as a

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means to check the hypothesis that groundwater depletion reduces streamflow)? Your analysis of this on p. 46 seems inconclusive. The 0.24 m mobile storage estimate for reduction of peak streamflow to zero (49/21) provides a proxy estimate, ie, compared to ~0.8m rainfall, nearly a third of the basin water budget appears to be surface flow converted to groundwater. This makes the groundwater depletion challenge all the greater because it's not just resident groundwater but also short-cycle recharged surface water that is being depleted. In this respect, 52/16 ("losing river") is not really an additional explanation. . .

35/22: "five SUCH hypotheses" (added emphasis) appears somewhat offhand. How were these arrived at? Were some grouped? Any omitted? Can you order and rank these (just as you have pointed out on p. 34 that the ISRO study did not quantify or assess plausibility).

36-38: excellent!

38: do plantations (negatively) affect potential ET via wind speed (reductions resulting from surface roughness, windbreaks) and relative humidity (locally, due to transpiration)? Do none of the plantations get irrigated?

41/20: how have "groundwater levels changed in the last four decades"? Fig. 3c is the closest thing you have, but does not to me directly indicate that "groundwater is now accessed at great depth". In fact, with the exception of the 2013 spike at 400m, these data are inconclusive or show the opposite that 2013 depths were the same or marginally less than 2012 or 2011 depths. No doubt, depth bored in a given year is a result of many factors, not solely (even primarily?) actual depth of groundwater.

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