Reviewer 2:

Reviewer 2, Hilary Mcmillan, has been largely supportive but has requested a substantial revision of the paper. We thank the reviewer for her careful reading. The minor changes will be incorporated in the rewriting.

Comment	**Response**
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.	We agree and thank the reviewer for helping us think this through. Several reviewers have raised this issue. We believe that the proposed restructuring of the paper will address this.
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 at a consistent depth?	The reviewer is correct that we make the assumption that water that travels below the rooting depth of the crops is available to recharge. This reflects the low topographic gradients in the catchment and the assumption that lateral flow is negligible except in close proximity to the land surface. In the absence of lateral flow, a one-dimensional water budget is appropriate, and recharge will represent any water that cannot be utilized by vegetation.
Line 255 - 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.	Analysis of fracture networks and well-to-well connectivity suggests that there is minimal lateral connection between wells even on distances of 5-10m. Thus the issue is not that lateral flow between tank aquifers is neglected, but rather that we aggregate and thus "average" storage across the fine-grained variations in the field to the tank scale. This raises the possibility that nonlinear interactions between local water storage and water use that could amplify these effects are being inappropriately averaged. We do not, in fact, introduce significant nonlinear assumptions regarding storage, so the averaging should introduce little error. This will be discussed in the revised paper.
The MWF model seems as though it would be very sensitive to rainfall intensity, but	MWF itself is not sensitive to intensity (i.e. the development of wetting fronts is insensitive to

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.	intensity). However the input to MWF – the infiltration flux, is sensitive to intensity - as any model that accounts for land surface partitioning of rainfall must be. Specifically, it is the relationship of intensity of the infiltration rate (Ksat) that is important. Both rainfall and Ksat have significant uncertainty in them – and Ksat is ultimately calibrated, subject to the available rainfall intensity data. Thus, improved rainfall downscaling might give a more certain estimate of Ksat, but given the calibration process, the water budget impacts would be minimal. Ultimately, the main point of the paper is to explain the long term decline in surface runoff. We have already established that there are no trends in rainfall intensity through multiple rain gages. Thus, we do not anticipate that the accurary of model results would change significantly given improved rainfall downscaling.
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?	Groundwater decline and land use change explains the stream aquifer disconnection and consequent baseflow decline (all of which occurred by 1995), as has been documented by many studies world over. After the mid-1990s, the link between stream flow decline and groundwater depletion is not as direct. There is no hydrologic (ie non-anthropogenic) mechanism that we believe could explain the continued decline of surface runoff beyond the mid-1990s. This is not an artefact of our model, but involves mechanisms common to all watersheds in India, which have been heavily modified with watershed structures.
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	We agree. We will address this in the revised and restructure manuscript.

also suggest that the information in Supplement 5 be added here as part of the model discussion.	
Table 2. The table seems to show more causes of groundwater increase than groundwater decrease, does this mean that groundwater should be increasing?	Yes. But the relative magnitudes of these contributions also matter. We will clarify this in the revision.